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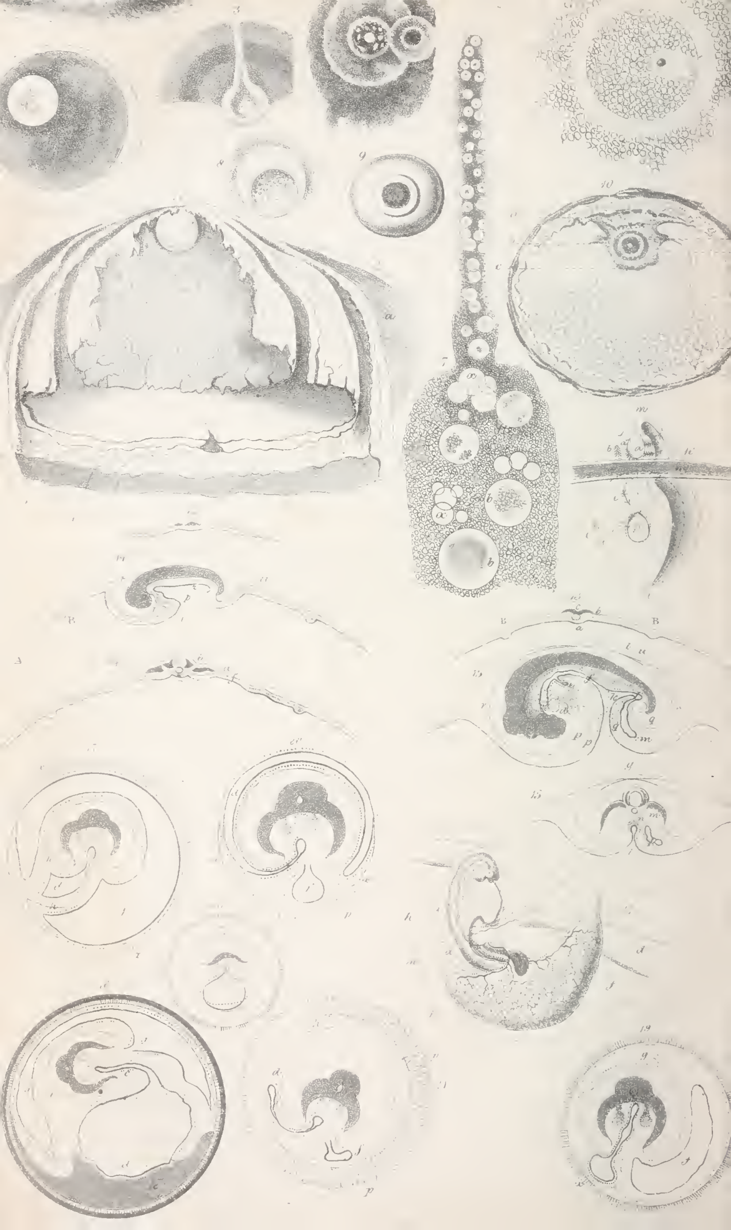

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PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *Über Entwickelungsgeschichte der Thiere. Beobachtungen und Reflexionen.* Von Dr. KARL ERNST v. BAER. Erster Theil, mit drei colorirten Kupfertafeln.—*Königsberg*, 1828. 4to, pp. 271.

Über Entwickelungsgeschichte der Thiere. Zweiter Theil, mit vier Kupfertafeln.—*Königsberg*, 1837. 4to, pp. 315.

Observations and Reflections on the History of the Development of Animals. By CHARLES ERNEST VON BAER. First Part, with three coloured Plates.—*Königsberg*, 1828. 4to, pp. 271.

On the History of the Development of Animals. Second Part, with four Plates.—*Königsberg*, 1837. 4to, pp. 315.

2. *Handbuch der Entwickelungsgeschichte des Menschen: mit Vergleichender Rücksicht der Entwickelung der Säugthiere und Vögel: nach fremden und eigenen Beobachtungen.* Von Dr. G. VALENTIN. —*Berlin*, 1835. 8vo, pp. 658.

Manual of the History of the Development of Man: with a Comparative View of the Development of Mammalia and Birds: after his own Observations and those of others. By Dr. G. VALENTIN.—*Berlin*, 1835. 8vo, pp. 658.

3. *Prodromus Historiæ Generationis Hominis atque Animalium: sistens Icones ad illustrandum Ovi Primitivi, imprimis Vesiculæ Germinativæ et Germinis in Ovario inclusi, genesin atque structuram per omnes animalium Classes multos que Ordines indagatam.* Auctore RUDOLPHO WAGNER, &c. &c. Accedunt Tabulæ duæ æri incisæ.—*Lipsiæ*, 1836. Fol., pp. 15.

A Precursor of a History of the Generation of Man and Animals: consisting of Drawings illustrative of the Origin and Structure of the primitive Ovum in the Ovary, especially of the germinative Vesicle and Germ; in all the Classes and many of the Orders. By RUDOLPH WAGNER, &c. &c. With two Copper-plates.—*Leipsic*, 1836. Fol., pp. 15.

4. *On the first Changes of the Ova of the Mammifera, in consequence of Impregnation, and on the Mode of Origin of the Chorion.* By THOMAS WHARTON JONES, Esq. *Communicated by* RICHARD OWEN, Esq. F.R.S. *Phil. Trans.* 1837. pp. 339, 345.

5. *Researches in Embryology. First Series.* By MARTIN BARRY, M.D., F.R.S E., Fellow of the Royal College of Physicians in Edinburgh. *Communicated by* P. M. ROGET, M.D., SEC. R.S. *Phil. Trans.* 1838. pp. 301, 341.

6. *Embryogénie Comparée. Cours sur le Developpement de l'Homme et des Animaux, fait au Museum d'Histoire Naturelle de Paris. Par* M. COSTE, *et publié sous les yeux du Professeur par les soins de* MM. Z. GERBE et V. MEUNIER. *Avec un Atlas, in 4to, composé de* 20 *Planches, dessinées d'après Nature,* par M. A. CHAZAL. Tom. Ire.—*Paris*, 1837. 8vo, pp. 477.

Comparative Embryogeny. A Course of Lectures on the Development of Man and Animals, delivered at the Museum of Natural History of Paris, by M. COSTE, *and published by* MM. Z. GERBE *and* V. MEUNIER, *under the Inspection of the Professor. With an Atlas, in 4to, composed of* 20 *Plates, drawn from Nature,* by M. A. CHAZAL. Vol. I.—*Paris*, 1837. 8vo, pp. 477.

7. *De Organis quæ Respirationi et Nutritioni Fœtus Mammalium inserviunt.* Prolusio Academica, quam scripsit DAN. FREDERICUS ESCHRICHT, M.D. Professor, &c.—*Hafniæ*, 1837. 4to, pp. 41.

On the Organs which are subservient to the Respiration and Nutrition of the Fœtus of Mammalia. By DAN. FREDERIC ESCHRICHT, M.D. Professor, &c.—*Copenhagen*, 1837. 4to, pp. 41.

THE production and the development of organic beings have afforded to philosophers of the greatest talent matter for speculation in all ages. In their endeavours, however, to explain what seemed to the rest of mankind so mysterious, they have been too frequently tempted to assign some external material cause as the effective agent of the process; and so have been lured into the devious paths of imagination, from the less attractive road of onward physiological pursuit. Hence arose the fanciful theories of pre-existence, and of indestructible organic germs; of oxygen and of spermatic animalcules: theories which, even when they had some ground in observed realities, did but remove to a distance, and obscure the difficulties of explanation, without in any degree resolving them.

Modern observations respecting the process of reproduction have been conclusive of two points at least: first, that organic bodies are not preformed; and next, that they are not suddenly produced from formless matter, as by the touch of some magician's wand. Those minute cercariæ, the spermatic animalcules, which were supposed for a time, by Prevôst and Dumas, to penetrate the formless matter of the germ, and so to constitute the first rudiment of the nervous system in vertebrate animals, have themselves supplied to Wagner, Siebold, and Valentin the means of detailing some portion of the history of their gradual development. And whatever simplest form of matter may at any time have been assigned as that upon which vital force, as an external agent, is about to begin its operations—to inform what is rude, to polarize what is indifferent,—yet always, in proportion as the instruments of observation have been per-

fected, and the zeal of observers has been stimulated by success in similar research, has that matter been shown to possess some organic arrangements, and to perform motions indicative of various energies.

In carefully observing any organism as it seems to come into being, the original primary matter, the secretion of a parent form, is seen to undergo separation and union in such a way that visible distinctions arise in the parts of that which was at first, to all appearance, homogeneous. From single and uniform it becomes multiple and different. The first separation is into solid and fluid, the more solid being still soft, and the more fluid in some degree consistent.* This first change, by which an incomplete antithesis is effected, is not followed by rest; it is but the precursor of others which appear in succession, and which draw new matter within the sphere of their action, derived either from the parent form as new secretions, or from external nature. New matter is constantly taken up, and old matter as constantly laid down. In the mean time, the separation into parts advances—their difference becomes more complete; and, with the new organs thus arising, new activities come into operation. The phenomena alter as each new element is introduced: and the activities with which matter is endowed are seen to change gradually with the gradual change in its form.

Minute anatomy and animal chemistry have not yet advanced to such perfection as to be able to define, satisfactorily, the different changes of structure and composition of matter which accompany the different manifestations of its activity. We know, in the gross, how the composition of muscle differs from that of bone: but we do not know in what respects the composition and arrangement of ultimate particles differ in the same muscle, when in action and when at rest. Yet without such knowledge (to which, indeed, in its perfection we cannot hope to attain,) our insight into the nature of organic processes must necessarily be incomplete. Still we must endeavour to advance it, as best we may, to the utmost; and not, as a refuge from difficulties, be willing to admit that living power is something superadded to dead matter; some distinct entity which, being added, enables the latter to evince those activities which we call vital: unless, indeed, we are content, at the same time, to allow the necessary consequences of that admission, and to confess that physiology is not an experimental science, and that medicine is an impossibility.

The just conclusion, however, appears to be, that the simplest animal matter which we can observe, as, for instance, the first limpid drop in a vesicle of the ovary, is simple only in consequence of the imperfection of our senses, and of the powers with which we aid them. For it performs acts of separation and of composition which are determinate for the species; and the results of these acts are new forms of matter, whose activities, in virtue of their composition, are again determinate: and so another set of consequences, still determinate, arises; and so on, in continual succession, until the creature has fulfilled, by means of its own material productions, the entire series of those acts which make up the idea of its being. Whilst we contemplate these, we cease to distinguish matter from its powers—a living organ from its activities. The change which the matter undergoes is its activity, its action, its power.

* Burdach.

The original matter, then, of any germ, however simple in appearance, has its particles so arranged, its qualities so co-ordinated, that from this arrangement and co-ordination, under the proper external conditions, the whole series of subsequent changes follow each other, step by step, with the utmost order and regularity. The first step involves all the rest: and hence the very simplest form of a germ is said to be *potentially* that animal which it will afterwards *actually* become. In this earliest stage, what is special and peculiar is inappreciable: it is veiled under what is common to the whole animal kingdom, viz. the first appearance of each individual in a fluid form. And when the embryo itself is first visible, whether in vertebrate or invertebrate classes, its form is a primitive streak. But now, though the primite streak be common to all, yet it presents special differences in the vertebrate and invertebrate classes. And this is the character of all subsequent changes which may occur, viz. that what is special (and it may be the precursor of very remarkable distinctions) appears as a mere variety or modification of what is common to large classes or groups. And hence it would seem to be an universal rule, that all varieties of form and structure are only modifications of a form essentially the same throughout the animal kingdom.

It is our wish in this article to lay before our readers the present state of our information respecting the earliest processes of development of the human ovum; for the purpose, not merely of showing how much of that which was lately only inferred from analogy is now actually proved, but, further, with the view of drawing attention to matters of considerable interest upon which enquirers, both in this country and on the continent, are not agreed: such as the origin of the membranes, as they exist in the uterine ovum—the structure of the placenta—the mode of its connexion with the embryo, &c. Yet, though human embryology be the proper end and object of our labours, we do not forget that it cannot be advanced in a right direction, by considering it under any other point of view than as the most elaborate instance of a general process. So that if we were not forced, by the very nature of the case, to supply, by reference to the embryology of birds and mammalia, that which must long be incomplete in the early history of human development, we should still, for the reason mentioned, point to the relations and differences in the corresponding processes which prevail in these different classes.

The papers of Mr. Wharton Jones, and of Dr. Barry, relate to changes of the ovum in the ovary and tube.

The bird's ovary consists of a parenchymatous cellular structure, supplied with vessels, &c., and surrounded by peritoneum, containing multitudes of vesicles which are to become the calices of yelks. In proportion to the vesicles the parenchyme is inconsiderable, so that the ovary has a botruoïdal aspect. In mammalia the parenchyme of the ovary is proportionally much more abundant: towards the surface it assumes more of a fibrous character, constituting the albuginea, and is, as in the instance of the bird, covered by peritoneum. This compact cellular tissue of the ovary, yellowish in colour, is called *stroma*, by Von Baër. It presents round, smooth cavities, in which are lodged the vesicles of De Graaf. Blood-vessels course amongst the fibres of the stroma, and ramify on its cavities.

The first rudiment of an ovum is, as we have said, a minute drop of

clear albuminous fluid, secreted by the inner surface of the cavity in the ovary. This fluid drop thickens towards its surface, and thus is an albuminous membrane formed, which is at once a limit between the internal fluid and the walls of the containing cavity, and the medium of their intercourse. Such a separation as this appears to take place more than once, in the fluid contents secreted by the vessels of the stroma; so that, after a time, the ovum within the bird's ovary consists of a fluid sphere and its membrane surrounded by a fluid shell and its membrane. The most internal of the two, viz. the fluid sphere and its membrane, is called, after him who discovered it, the "vesicle of Purkinje," or, generally, the "germ-vesicle;" the spherical fluid shell is called "vitellus," or "yelk;" and its membrane, "vitellary membrane," or "cortical membrane," or "chorion," by different authors. In the smallest ovules, says Valentin, the external yelk-membrane is, with difficulty, separable from the investing membrane of the ovary: it contains a fluid mass, holding suspended a great number of very small round corpuscles, transparent, and only slightly surpassing Brown's corpuscles in size. As the ovule increases in size, the homogeneous transparent mass connecting these bodies shows a consistence as of oil—becomes more tenacious; some round drops, like drops of oil, appear in it, whilst not only these isolated globules, but the whole ovule also, attains a deeper colour. When the ovule has acquired $\frac{1}{6}'''$ in diameter, in addition to these parts, the germ-vesicle, and the granules, and the cicatricula may be distinguished. And this general rule is found to prevail, that the germ-vesicle is largest, proportionally, in the smallest ovules; and in respect of absolute size, is only slightly larger in the most mature ova than in the smallest of the same ovary. In the hen's egg ($\frac{1}{9}'''$ in diameter), the vesicle measures $\frac{1}{20}'''$ (Purkinje).

We have detailed, with some minuteness (after Valentin), these particulars respecting the early ovarian ovule of the bird, that the comparison which he institutes between it and the mature ovarian ovule of mammalia may, hereafter, be intelligible.

The relations of those two important elements of the ovarian ovule—the yelk and the germ-vesicle—were very different from what we have described when first discovered by Purkinje, in 1825. We offer here his description of the envelope of a mature ovarian ovule, and the history of his discovery:

"The inner surface of the yelk-membrane is covered with a delicate and equable layer of globules, about the size of those of the blood, though more transparent; not presenting a fortuitous or irregular mass, but arranged in an organic fashion. Around the cicatricula the globules are more closely crowded, so as to form on all sides a little zone, more than half a line broad, whose inner circuit presents a whitish mammæform cumulus of these same globules, projecting towards the yelk, (see Fig. 1.) On the top of the cumulus is a pellucid pore, visible from either surface of the cicatricula, nearly one sixth of a line in diameter. This pore is never seen in ova of the oviduct or of the uterus: and since I did not find that authors had noted any difference between the cicatricula of the egg and that of the ovarian ovule, I thought it important to investigate the subject further.

"Under a single lens, $1\frac{1}{2}$ inch focal length, I set about destroying the mammæform cumulus in a direction towards its centre, in order to arrive at the pellucid pore. After many attempts, I did but wound a very delicate cuticle, inclosing

a limpid fluid. I fancied, rather than possessed any evidence to prove, that there lay something below, of a globular form. Chance at length favoured my numerous and unprofitable endeavours. Whilst removing, by suction, the water in which the object was immersed, the cumulus in question was left dry upon the boss at the bottom of the glass vessel, collapsed and almost dispersed, so that the diameter of the central pore became about double. Whilst examining this with a lens, a most beautiful vesicle, adhering in part to the margin of the pore, in part free, presented itself, to my great admiration. After this, it was not difficult to set it loose. A very minute vesicle, therefore, occupies the pore which seems to perforate the cumulus of the cicatricula; and, being immersed in it, presents only two free surfaces, one towards the outer membrane of the yelk, the other through the top of the little hillock, where it is surrounded, as if by a small crater, towards the interior of the yelk. . . . . Imagination hurried to the conclusion, that this globule might be the germ from which the chick is gradually developed. The next step, therefore, was to investigate carefully the condition of the cicatricula in a recent egg, before incubation, in order to ascertain what changes the vesicle has undergone. It was found that here the cicatricula does not adhere to the yelk-membrane, whilst in the ovarian ovule the two are separated with difficulty: and that in the latter it is readily separable from the subjacent yelk, whilst in the other it adheres thereto pretty closely. The zone of the cicatricula in the ovule, thin and narrow, passes into the cumulus, where the plastic matter appears concentrated. In the cicatricula of the egg, all is extended in breadth, the hillock has disappeared, the semi-pellucid blastoderma is everywhere of the same thickness, and offers not the vestige of a vesicle. The cicatricula now forms a double circle, of which the outer adheres to the yelk, the inner (a continuation of the former) is separated from the yelk by the fossula plana, or, *colliquamentum Malpighii.* This circular fossule resides in the yelk, presents in the middle a whitish umbo (nucleus of Pander), covered with a viscid, semi-pellucid matter, on which white granules, like flour, are sprinkled. (See Fig. 2.) . . . . The vesicle, therefore, of the ovarian ovule has been broken up, and seems changed into the colliquamentum of Malpighi."*

Purkinje is disposed to conclude that the vesicle bursts in consequence of the contractions of the oviduct upon the semi-fluid yelk, as soon as it has passed into the infundibulum—that its lymph is mixed with the substance of the colliculus, and that hence arises the doubly-circular appearance of the cicatricula, &c. The bursting of the vesicle, however, does not depend upon a mechanical cause, as Baër has shown, in his celebrated epistle.

The important vesicle thus discovered by Purkinje, which appears to prepare the cicatricula, after impregnation, for the development of the germ, naturally excited the utmost interest and curiosity. It has, in consequence, notwithstanding the extreme delicacy of the enquiry, been found to exist in every true ovarian ovule in which it has been sought. It was observed by Von Baër, in 1827, in mollusca, annelidæ, crustacea, insects; by Coste, in 1834, in mammalia (rabbit); by Purkinje, Bernhardt, Valentin, and Wagner, in the same year, in mammalia generally; and in this country by T. Wharton Jones, in 1835, in the rabbit. The Prodromus of R. Wagner is especially dedicated to the description of its origin and structure, in all the classes and many of the orders of the animal kingdom. It records, also, an important discovery of his own respecting this vesicle, of which he had previously published a notice, in Froriep's Notizen. If the vesicle be carefully examined under a sufficient magnifying power (a power magnifying 40 or 50 diameters,

* Symbolæ ad ovi avium ante incubationem. pp. 3-5.

for man and the mammalia), there will be found, at some point of its surface, an opaque round spot. This spot Wagner, at first, called macula germinativa. When, however, a higher power was used, he found that the spot is, in reality, a circular flattened layer of extremely minute granules, strictly agglutinated. In mammalia, birds, scaled amphibia, cartilaginous fishes, arachnoïdea, some crustacea, mollusca, conchacea, echinodermata, medusæ, polypi, the macula is almost universally single—and when more than one (which is very rare) there are many maculæ or acervuli, never two, (Figs. 4 & 6). In the batrachia, osseous fishes, some crustacea, the acervuli are numerous, constituting five, ten, or twenty maculæ, (Fig. 5). Hence Wagner now calls this spot "stratum germinativum primitivum." The granules adhere to the interior of the membrane of the vesicle, and are there in contact with its limpid fluid which contains no granules. In proportion as the ovule becomes mature, the connexion between the granules and the membrane of the vesicle is less marked, and the stratum assumes another form. The spot, in those instances where it had been single, spreads out into a number of smaller spots under the surface of the membrane, or even forms a thin nebulous stratum, still more diffused.*

The germ-vesicle, as we have said, at first lies towards the centre of the yelk, and gradually approaches its surface at that point which is most remote from the ovary. Here it is fixed by a layer, more or less circular, of granular matter derived from the yelk, and at length is received into the pore which perforates that layer, (Fig. 3).

The vesicles of the ovary of mammals, which now bear De Graaf's name, had previous to his time been noticed by Vesalius, Fallopius, Bartholin, and others. His contemporaries, also, Van Hoorne and Steno, had described them—by the last of whom they were termed ova—a name adopted by De Graaf, and used by him indifferently with that of vesicle. Valentin, in allusion to De Graaf's work, and in particular to his admirable chapter on the generation of the rabbit, sees reason to infer that probably De Graaf did not consider the vesicles as the ova which are really received into the tubes after impregnation, but that the existence of some smaller ovum, with its proper membrane within the vesicle, was surmised and anticipated, though certainly not demonstrated, by him. We have not received the same impression from De Graaf's work. When he became aware that the ovum in the tube of the rabbit, on the third day, is surpassed in size by the vesicle of the ovary tenfold, he was undoubtedly, as Valentin says, strongly impressed by the fact: but, as we collect from several passages, he accounts for it in another way. His observations of the vesicles in the rabbit, up to the fifty-second hour after copulation, detected no other change than a gradual loss of transparency—an increasing vascularity—their contents becoming more viscid—a papilla on their surface more and more prominent. The observation of the fifty-second hour showed the appearance now called corpus luteum, in four vesicles:† "quibus dissectis," says he, "materiem quasi glandulosam offendimus, in cujus medio exigua cavitas erat, in quâ cum nullum notabilem liquorem compererimus, suspicari cæpimus

* Wagner Prodromus, p. 4, 5. Froriep's Notizen, Nr. 994, p. 53.

† De Mulierum Organis Generationi inservientibus. In Mangeti Bibliothecâ Anatomicâ, i. 478.

num limpida eorum substantia, quæ propriis membranis obvolvitur, disrupta vel expulsa foret." After seventy-two hours, one of the vesicles was found without any fluid; wherefore, on suspicion that an ovum had escaped, it was sought for, and found in the corresponding horn of the uterus, having a size only one tenth of that of the ovarian vesicle: "quod eatenus contingere nobis videtur, quatenus scilicet in testibus (*ovariis*) existentia (*ova* sc.) adhuc aliam materiem complectuntur, illam scilicet, ex quâ glandulosa folliculorum substantia provenit." In another part of his work, alluding to this observation of the rabbit, he says,* "postero die post opacitatem illam conspectam, inter dictas ejus tunicas glandulosam quendam materiem totum ovum involventem globuli figuram repræsentantem intueberis, quæ sensim accrescens ovum undequaque comprimendo, illud tandem per foramen in ejus medio conspicuum expellit: quod in cuniculis tertio post coitum die, &c." It seems clear, from these passages, that De Graaf believed the corpus luteum to be formed between the coats of the vesicle, at the expense (in part at least) of a portion of the fluid contained within its inner tunic, and that the ovum of the tube is, in fact, the diminished vesicle. But this by the way. Many distinguished contemporaries of De Graaf, amongst them Malpighi and Valisnieri, dissented from his opinion that an ovarian ovum is received into the tube. But what more than any other circumstance diverted, for a long period, the attention of physiologists from the search for an ovum in the ovary was, as Valentin remarks, the authority of Haller. After maturely weighing the observations and opinions of his predecessors, he unfortunately came to the conclusion, that the theory of evolution is alone true; that an animalcule escapes from the ruptured ovarian vesicle on conception, so fluid and pellucid as to be invisible, yet still the same which is afterwards seen in the uterus, when its different parts have in some degree been evolved.† Hence the existence of the ovarian ovum, which was strongly suspected by Cruickshank,‡ was not demonstrated until, by the successful labours of Prevôst and Dumas, it was twice observed in vesicles of the ovary after impregnation, as a small spherical body, a millemeter in diameter. Since, however, it was less transparent than the ovule of the tube, they describe it as the ovarian ovule with some hesitation. The honour of discovering the ovum in the *unimpregnated* ovary of a mammal was reserved for Von Baër. He detected it in the dog—presenting an opaque, granulous centre, surrounded by a halo, like the ovum of the tubes (which in the dog he always found to be opaque, when not more than $\frac{1}{15}'''$ in diameter). It was found to vary in size from $\frac{1}{50}'''$ to $\frac{1}{20}'''$, and to be placed in a granular layer and cumulus, within the inner membrane of the Graafian vesicle, exactly as Purkinje's vesicle is placed in the cumulus of the ovarian hen's egg. This cumulus of the vesicle Baër unfortunately considered to be an analogous structure to that of the hen's egg—a mistake into which he was led by the supposed absence of Purkinje's vesicle in the ova of mammals. His view of the mammalian ovum is consequently in some respects exceedingly fanciful, viz. that it is an ovum within an ovum—standing in the relation of Purkinje's vesicle to the ovarian ovule of De Graaf, but in that of the vitellus and its appendages to the fœtus—and presenting, moreover, the strange anomaly of a discus proligerus *external* to the vitelline membrane.

* Op. cit. 463. † Haller, El. Phys. viii., 143, 151. ‡ Phil. Trans. Vol. lxxxvii.

In 1834, Coste corrected this view of Von Baër, and published his discovery of the germ-vesicle in the ovum of the rabbit—thus clearly establishing the general analogy between the ovarian ovum of mammalia and of birds.

The ovum of mammalia touches the inner membrane of the Graafian vesicle at that part which is nearest to the surface of the ovary, and is sustained in its position by a continuation below it of the granular layer called by Von Baër discus proligerus. But the cup which thus contains the ovum (the cumulus of the disc) is larger than it. There is a transparent shell between the two, a space which may perhaps be filled with a clear fluid. When extracted from this, the ovum of mammalia is found to consist, according to Valentin, of—1, an external membrane; 2, a granular layer beneath the former; 3, a contained semifluid, perfectly transparent; and 4, the germ-vesicle. (*Valentin*, p. 19.)

We shall now, after Valentin, point out some distinctions which prevail between the ovarian ova of birds and of mammalia. The external membrane is without any perceptible inner structure in both, unless we admit, in the case of the bird, a very few intricate fibres, irregularly disposed and scarcely perceptible; it has no organic connexion with the neighbouring parts. The yelk, in the unimpregnated bird's egg, consists of three different parts: (*a*) large, oily, yellow, or yellowish yelk-globules; (*b*) very small globules dispersed amongst the former; (*c*) a transparent fluid in which they (*a* and *b*) swim. In the mammalian ovum, the following analogous parts are found: (*c*) the clear transparent fluid, which seems to have a more oily consistence; (*b*) corpuscles, partly corresponding in size to the smaller gobules of the bird's egg, partly something larger than these. The large globules (oil-globules, *a*) are altogether absent. In the central cavity of the bird's egg is a peculiar semifluid mass. A fluid, oily and perfectly transparent is also found in the centre of the mammalian ovule. But in the last the central cavity is not nearly so well defined, nay, it is scarcely to be distinguished from the mass containing granules. The germ-vesicle of the bird's egg is sunk in the disc, just as the ovule of mammalia is sunk in the disc of the follicle: in the mammalian ovule, on the contrary, a much smaller portion of the vesicle is covered by the granular contents. The disproportion of germ-vesicle to yelk, in respect of size, is much greater in the bird than in the mammal. In the bird the egg is surrounded by the membrane and substance of the ovary alone; in the mammal not only by the ovarian follicle but by its disc. From this comparison, Valentin concludes that the ovarian ovule of the mammal is altogether unlike the ovarian ovule of the bird, both being mature. If, on the other hand, the comparison be instituted, between the ripe ovarian ovule of the mammal and the unripe ovarian ovule of the bird, then the similarity is most striking. For in its first stage of development, the bird's yelk is of a grayish-white colour; is composed of an internal membrane perfectly transparent and fibreless; its small globules are without a trace of the true large yelk-globules; and the fluid mass, perfectly transparent, connecting the globules, is especially collected in the centre. The germ-vesicle is now much larger in proportion to the ovule than afterwards, and there is no disc. Valentin asks, does not this description apply strikingly, word for word, to the mammal's ovule? and, in reliance on

the affirmative answer, concludes generally, that "the ovum of a mammal perfectly resembles the immature ovum of a bird, and essentially differs from the latter as soon as the yelk-globules have appeared in it."

From the greater relative size of the germ-vesicle in the more unripe ovules, it seems not unreasonable to suppose that if an ovule could be observed, at a period not distant from its first formation, it would be found to consist of germ-vesicle alone. Now, R. Wagner, one of the most skilful observers now living, and the discoverer of the germinative stratum of the vesicle, tells us that he has never been able to find that vesicle free in the mammal's ovary; that, even in ova, which did not exceed $\frac{1}{50}'''$ or $\frac{1}{100}'''$, there was always some appearance of yelk and of its investing membrane, which he calls "chorion," the vesicle being half the size of the whole ovum. When, however, he extended his observations to insects the result was different. The germ-vesicles and their spots are that which is first visible in the tubular ovaries of these creatures, the finest hollow filaments holding in their extremities free germ-vesicles, not more than $\frac{1}{100}'''$ in size, and each of them having a spot of $\frac{1}{400}'''$ (fig. 7). He thus greatly strengthened the probability that the germ-vesicle is the part first formed in every ovum. Dr. Barry has, in the paper before us, supplied, by direct observation, that which Wagner had inferred analogically. He is enabled to describe in two classes of vertebrata, viz. in the rabbit and in the pigeon, observations similar to those of Wagner in the inferior animals, and therefore confirms the belief of former writers, Burdach, Baër, Wagner, "that the germinal vesicle and its contents constitute throughout the animal kingdom the most primitive portion of the ovum."

Dr. Barry next relates his observations of the genesis of the containing membranes of the ovum in some one of the "myriads of minute vesicles, which give to some parts of the ovary, under the microscope, no mean resemblance to the roe of fishes." The fluid holding the granules which surround the germ-vesicle does not appear to be confined in any proper membrane as yet, the whole reside in a cavity sometimes formed in the stroma, sometimes in the walls of an adjacent Graafian vesicle. Afterwards there is gradually seen to form round the enveloping granules a membrane, which Dr. Barry calls the ovisac. It lies loose and unconnected with the walls of the containing cavity, is perfectly transparent, of considerable thickness (as much as $\frac{1}{12}$ of the diameter of the sphere which it bounds), distensible, elastic, apparently unorganized.

Changes now take place in those peculiar granules; the yelk begins to be formed around the germ-vesicle, and is itself now found to be surrounded by *two* membranes, "one the proper membrane of the yelk, membrana vitelli," the other, more external, is called by Dr. Barry the "true chorion." Dr. Barry adds, "the ovum is seen with great distinctness through the transparent membrane of the ovisac, and it is thus possible to follow its different stages of formation." "Subsequently a covering or tunic, consisting of a kind of dense cellular tissue, susceptible of becoming highly vascular and closely connected with the surrounding stroma, is gradually formed upon the outer surface of the ovisac, which, previously in a high degree transparent, now becomes translucent only." (pp. 310-11.)

Dr. Barry apprehends that from the union of this cellular covering

with the ovisac, there results what has been called a Graafian vesicle. He extends his examination to the ovisac of amphibia and fishes, and concludes generally, respecting these classes, that the primitive elements of their ova are contained in a vesicle (the "chorion" of authors), essentially the same as that called by him the "ovisac" of mammalia; that the ovisac of mammalia, acquiring a proper tunic, is called a Graafian vesicle, whilst the ovisac of the three other classes acquiring the proper tunic is called a capsule; and further, that, when in these last it becomes pendent from the ovary, and invested by what there is of the substance of the ovary, as well as in some instances by the peritoneum, it is called a calyx. Hence, Dr. Barry clearly shows, that his Graafian vesicle is neither a structure peculiar to mammalia, nor is it analogous to the whole calyx of other animals, but to the capsule.

We cannot praise too highly the care and accuracy with which Dr. Barry has conducted these delicate observations. But we beg to state that his idea of what is to be understood by a capsule and what by a Graafian vesicle does not agree with our own. The Graafian vesicle and the bird's yelk are equally products of the stroma of the ovary which surrounds them, and forms their capsule or envelope. The stroma becomes modified by the presence of its own secretion, the fluid of the vesicle, and undergoes changes such that the capsule, which it always forms, does not at all periods consist of the same elements. The capsule is stroma alone, when the ova of all the classes alluded to by Dr. Barry are mere fluid. And when the ovisac has been formed, the capsule is still stroma alone; whilst the follicle of De Graaf, contained in that capsule, is now ovisac and contents. Next, the stroma allows vessels to pass, and they form in its interior a new vascular membrane, which is more granular, and partakes, according to Baër, of the mucous character. And now the capsule consists of, 1, an external membrane, separable only by maceration from the stroma; 2, an internal, more vascular, membrane; whilst its contents (the follicle) consist of 1, Dr. Barry's ovisac, or Valentin's membrana folliculi; 2, Baër's and Barry's membrana granulosa, or Valentin's membrana cumuli (so called because it swells out round the zona pellucida to form the cumulus); 3, the ovum with the fluid that surrounds it. When the follicle approaches the surface of the ovary, the peritoneum is found to cover a portion of the capsule, and so enters partially into its formation. Dr. Barry believes that the two layers of the capsule, as described by Baër, correspond, the external to his proper vascular covering, the internal to his ovisac. But that is not the case. Baër gives good reasons for maintaining what we have stated above respecting the capsule, and of it Dr. Barry's ovisac forms no part. As a part of the follicle this last is extruded, sooner or later, after the capsule has burst; the capsule, as a part of the ovary, remains to resume its original form of stroma. With respect to Dr. Barry's description of the genesis of the ovisac, there is every reason to believe that it is correct. Its existence was established by what Pockels recorded respecting the ovum of the deer; and appears now to be admitted by Valentin, in his Repertorium, (vol. iii., p. 190,) as we have stated above, though it is not described in his earlier Handbuch.

* Muller's Archiv, 1836. p. 193.

In this respect only does Dr. Barry differ from Pockels, that he describes the cellulo-vascular layer as adhering more closely to the ovisac than to the external layer of the capsule. In the deer the membrane marked (4) in Pockels's plate (Fig. 11), and which corresponds to Dr. Barry's ovisac, is a distinct membrane. He tells us that it remains in the ovary of the sheep, goat, &c., for eight days or more after the escape of the ovule as a bladder full of yellow serum, the corpus luteum forming around it.

It seems probable that in different mammalia this outermost covering of De Graaf's vesicle escapes from the ovary at very different periods after the passage of the ovum into the tube: and moreover, that in the same species it may be, from unassignable causes, retained for a longer or a shorter time; or that, from disease of the ovary, it may not be removed at all. These considerations may assist us in explaining how it may happen that writers of great note differ so frequently in their determination of the seat of the corpus luteum, one placing it in the inner stratum of the theca, another between the strata. Doubtless it is always formed in the vascular inner stratum, which may occasionally be seen covered by the membrane which is, under ordinary circumstances, about to be removed.

But to return to Dr. Barry's paper. When the ovum is completely formed, it is placed in the central region of the ovisac, and keeps its situation there for a time in consequence of the equable diffusion of the peculiar granules in the fluid which surrounds it. These granules enter into the formation of several important structures, and therefore are minutely described. They are ellipsoïdal, generally flattened, sometimes nearly round, "presenting with more or less distinctness a nucleus, or, occasionally, even two nuclei in a single granule," varying in size from $\frac{1}{400}'''$ to $\frac{1}{100}'''$ in length, soluble in water. Their form varies in different orders of mammalia in respect of roundness, and in birds is thought by Dr. Barry to be less regular. They are exceedingly transparent, yet "often punctate, which latter appearance seems sometimes to arise from the presence of minute oil-like globules." They sometimes disappear, apparently by liquefaction. On changes occurring in these granules and the containing fluid, three structures are noted by Dr. Barry to arise. One of these has been described by previous authors as the *membrana granulosa;* the other two have not been hitherto described. Of these, Dr. Barry proposes to call one the *tunica granulosa*, the other (an assemblage of structures rather than a single structure) the *retinacula.* The tunica granulosa is a spherical covering surrounding the "thick chorion," concealing the outer line of its contour, and causing it to appear as a "zone," "halo," or "pellucid space." The retinacula are a central mass surrounding the tunica granulosa, and connected with the membrana granulosa by intervening bands. One use of these bands or cords is "to suspend the ovum and retain it in its situation in the fluid of the Graafian vesicle." It is next conveyed to the surface of this vesicle by subsequent changes in these same structures; the granulous cords on one side of the central mass disappearing, whilst those on the other side are shortened until the ovum has reached the periphery of the Graafian vesicle. "And what is very remarkable, and an interesting instance of *design*, the particular part of the periphery of the vesicle, to which the ovum is thus conveyed, is always that directed towards the

surface of the ovary." Here the ovum is held against that surface by the action of the cords which remain, and it is either pressed through the membrana granulosa, or else that membrane is removed by the pressure. Thus it is the tunica granulosa of the ovum, and the central portion of the retinacula, which make up the "cumulus" of Baër, and the band-like portions of the same structure which make up his "flat disc." (Fig. 10.)

The contents, therefore, of a mature Graafian vesicle, will be as follows, according to Dr. Barry:

1, Membrana granulosa, containing fluid, in which are,
2, Tunica granulosa and retinacula (the cumulus and disc of Baër);
3, Chorion ("pellucid zone" or "halo" of other authors);
4, Membrana vitelli;
5, Yelk;
6, Germ-vesicle, with Wagner's granules and its fluid.

We have already shown that Dr. Barry's ovisac ought to head the above list.

These observations of Dr. Barry are exceedingly interesting: they will doubtless be scrutinized strictly by those most capable of verifying or of correcting them. They impress upon us, from some little experience, the conviction that, before any beginner in such pursuits can venture to give an opinion concerning the truth of the results stated, he must well practise his eye to observe the ovum of any animal or class of animals at all periods of its ovarian development, otherwise he will certainly describe what he sees by wrong appellations. For we find that the external membrane of the ovum at one early period is not the external membrane of the next period. That membrane which is called "chorion" by some writers, is a future element of the Graafian vesicle, or capsule according to Barry; whilst his "true chorion," which from the name might be presumed to be really the outermost membrane of the ovarian ovum, is found still to be surrounded by another, viz. the tunica granulosa. The name "zona pellucida" is much less exceptionable than that of "true chorion;" it implies no theory, and certainly it has not yet been shown that any one element of the uterine chorion exists in the ovarian ovum.

Let us now see what other writers say respecting some of the above constituents of the ovarian ovum, which afford the subject of what is presumed to be new in Dr. Barry's paper.

It appears to us that Dr. Barry is original in his description of the tunica granulosa and retinacula; and in his record of the important offices performed by these structures as seen by himself. The existence of Baër's cumulus as a proper envelope of the ovum, has never before, as far as we know, been hinted at; neither has the necessity of accounting for the motion of the ovum from the centre to the surface of the Graafian vesicle been sufficiently obvious to others.

With respect to Dr. Barry's "true chorion," we have seen above, that Valentin describes the ovum as lying in a cup of the disc surrounded by a clear fluid which appears to connect the two intimately by its viscous nature. For he adds (p. 18), that he has never examined the ovum without perceiving traces of the disc. And again, in Müller's Archiv for 1838, p. 534, Valentin states, that when the ovum is found in the

centre of the follicle, it is not surrounded by a zona pellucida, but that this zone comes into view as the more fluid contents of the follicle accumulate at the centre, and as the ovum at the same time moves towards the surface; and that it then seems to be relatively broader than it is afterwards. In his Repertorium for 1838, p. 190, Valentin tells us that, according to his experience, the zone is not a transparent mass included in any proper membrane.

Krause, in Müller's Archiv for 1837, p. 27, describes the zone as surrounded by a proper very delicate membrane, which becomes visible when the surrounding granules are removed; and he considers it to be an albuminous fluid confined by that membrane (Fig. 8).

Wagner describes as chorion, in the ovum of mammalia, the same thick pellucid matter as Dr. Barry. (*Prodromus*, p. 11.)

With respect to the vitellary membrane, we are assured of its existence by Valentin and Krause, as well as Barry. Krause describes it as of uniform but small thickness, with well-defined double boundary.

On the other hand, according to Coste and Mr. Wharton Jones, there is only one membrane external to the spherical granular yelk in the ovarian ovum of mammals: this corresponds to that clear, thick envelope called "true chorion" by Barry, and "pellucid zone" by Valentin, but they call it "vitellary membrane."

We may add that the opinion of Carus, as far as concerns the zone, agrees with that of Krause. He considers the external membrane of the zone as described by Krause to be "chorion"—the internal to be "vitellary membrane," the intermediate fluid to be analogous to the "white" of the bird's egg—and that there is no other vitellary membrane.

It seems, therefore, that the question of the number of envelopes of the ovarian ovum of mammals is still undetermined. In our view it is important, inasmuch as the origin of the outer portion of the chorion of the uterine ovum is connected with it.

In mammalia the period of dehiscence of the Graafian vesicle is exceedingly variable. In the sheep, the vesicle opens a few hours after impregnation; in the dog, after several days have elapsed, according to Baër; in the deer, according to Pockels, after five months. Mr. Jones's paper relates principally to the change which the membranes of the ovum undergo in the tube. When he observed the ovum of a rabbit taken from the tube on the fourth day after impregnation, he found, "in addition to the component parts of the ovum of the ovary, a thick gelatinous matter surrounding it." (Fig. 9.) This envelope he believes to have its origin in the ovary as an effect of impregnation. "For in the ova of the rabbit .... before impregnation the proligerous disc, in which the ovum is imbedded, is observed to be composed of a gelatinous substance interspersed with grains, but as yet there appears no distinctly circumscribed envelope ..... . In a rabbit, six days after impregnation, no vitellary membrane was to be seen. The gelatinous-looking envelope constituted the only covering of the yelk, which now formed a vesicular blastoderma." Dr. Barry also, speaking of his membrana vitelli in the same animal, informs us that, after impregnation, "just before the ovum leaves the ovary, this membrane, previously so delicately thin, becomes perfectly distinct and very thick; and that

'the chorion,' imbibing fluid into its interior, becomes somewhat distended, so that a minute space is visible between the membrana vitelli and the chorion. This thickening of the proper membrane of the yelk, and distension of the chorion subsequently proceed much farther, as is proved by the state of the ova found in the Fallopian tube. I find also that the membrana vitelli is still visible, and has considerable thickness in minute ova met with in the uterus." We do not attempt to reconcile their discrepancies; but these authors agree in the fact that the membranes of the ovarian ovum undergo important changes whilst the ovum is in the tube; and we shall endeavour to explain what we conceive to be essential in these changes.

On conception, the vascularity of the ovary, tubes, and uterus is exalted, and the secretions of the different surfaces, viz. of that with which the ovum is, or of those with which it is to be, in contact, are consequently altered. These altered secretions are also much increased in quantity by the presence of the ovum itself when it passes along the tube, or is received into the uterus. We have shown that the external membrane of the ovum is not merely an envelope; but further, that it is the medium of communication between what is within it and what is external to it; and that, to this end, it is formed as a concretion on the surface of the fluid secreted by what is afterwards on its exterior. Now, it is most improbable that, in any case, the same membrane which properly fulfils this office in the ovary will fulfil it in the tube;—for it is under different external conditions: consequently it must be changed. But we shall shortly show that, in some instances, that change is so entire, that when the ovum is in the uterus an absolutely new external membrane is formed. We therefore look upon the above observations of Mr. Jones and Dr. Barry, in the instance of the rabbit's ovum, as proving absorption of fluid from without, by the temporary outer membranes of the ovarian ovum, as preparatory to their entire or partial solution and alteration. And this is quite consistent with what is observed in all other processes of development: for, as Valentin remarks, no part is at once laid down such as it is to continue, even when that continuance is to effect only a temporary purpose.

Mr. Jones believes that the groundwork of the chorion is formed in the tube: so also does Valentin; and we collect from a late notice of the proceedings of the Royal Society that Dr. Barry, in a second paper, relinquishes the opinion, maintained in his first, that the thick transparent membrane of the ovarian ovum deserves the name of "true chorion." In fact, we presume it will soon be generally taught that the external membrane of the ovum changes its form according to the nature of the fluid or solid with which it is in contact, and from which it is its office duly to absorb matter for the nutrition of the ovum. It may undergo either a gradual change, or it may be formed de novo. Of the latter we have instances in the sow and sheep; and it seems to depend upon the quantity of fluid secreted around the ovum; for in the dog, where the quantity is small, the change is gradual. Baër informs us (vol. ii. p. 185,) that he has followed the formation of the external membrane in the sow and sheep through all its stages. It is formed anew in the horn of the uterus. In the sow no trace whatever of that which is afterwards the external membrane can be perceived until the thirteenth day; but the yelk-

sacs are separated from each other by large quantities of albuminous fluid, which distends the sacculated portions of the uterus where they are deposited, and of which they are absorbing a portion, and thereby increasing in size. They are not able, however, to absorb the whole of this fluid ; and, therefore, as in the bird's egg, a membrane forms around it. On the thirteenth day this membrane is still so tender that it can only be detected by laying the opened uterus in cold water, when the inner surface is seen to be covered by it, as by a thin film. In ova which are two days older, it has acquired so much consistence that the ova may be removed surrounded by it. Baër was unable to observe this process in the dog or rabbit; for in them the yelk is not drawn out in length, as in the former instances, by the contraction and the form of the inner surface of the uterus, but is closely embraced by the uterus, very little fluid being effused around it. But let us hear Von Baër. "The ovum of the dog, arriving in the uterus, has, like all other ova, an external membrane. If this cuticular membrane can properly transmit the secreted albumen, so that the same may be collected within it, of course no other cuticular covering of the albumen will be formed—there is one already. Since, then, a very little albumen is produced for the ova of carnivora and rodentia—and since the uterus of these animals is so formed that it closes upon the ova at an early period, I think it not improbable that the same membrane which the ovum brought with it as cuticular or external, continues to be such. . . . . It is a matter of indifference to the ovum, if I may be allowed the expression, how it comes by its external membrane; whether that membrane which was external should persist as such (as I must still believe it does in the dog), or whether the albuminous fluid effused around it forms for itself a new one." (Vol. ii., p. 187-8.) In the bird it is the office of many different parts to supply the yelk with investments, both fluid and solid, to prepare it for incubation. The more condensed albumen is secreted around it by the first portion of the oviduct, the more fluid by the portion immediately above its isthmus. In the isthmus the membrane of the shell is formed, and in the uterus the shell itself. In the mammalia, also, analogy is borne out by fact, since new albuminous fluids, and new investing membranes, are formed around the yelks in the tubes and in the uterus.

The views of Coste respecting the chorion are very different from those which we conceive to be correct. He says (p. 107) "the vitelline membrane persists during the whole period of development; it is the analogue of that envelope of the fœtus which is known to anatomists under the name of chorion."

"Whatever be its origin," says Baër, "the external membrane of the ovum in mammalia has, under all varieties of form, the same properties as the shell-membrane of birds, except that it is less dry, inasmuch as it is bathed by fluid, and permeable by it. Until joined by a vascular membrane from the ovum at a later period, it is entirely devoid of vessels. It develops villi on those parts of its surface which come in contact with parts of the uterus which are not quite smooth, or with what may cover such parts. When it receives blood-vessels from the ovum they grow into these villi, and such of the villi as do not receive vessels are but slightly developed. Another general circumstance is, that it consists of two laminæ, at least from the moment when the villi are formed, for one lamina proceeds continuously underneath them. Yet it is not the external lamina which alone forms the villi: for that forms merely their surface. Their substance con-

sists of a mass which is gradually collected between the two laminæ, as may be easily seen in the villi of the ovum of ruminants. We may therefore properly recognize three layers, when the villi are more advanced." (Vol. ii. p. 188.)

This account of the compound nature of the external membrane appears to be received by Burdach, but to be rejected by Valentin, who looks upon the separation as altogether artificial. We give the passage from Baër, because it is not impossible that some one or more of the elements of the ovarian ovum may, in his next paper, be followed by Dr. Barry into the structures thus distinguished.

This villous membrane is that external portion of the future chorion which is called *exochorion* by Burdach and others.

Before we can satisfactorily follow the subsequent elaboration of the chorion, it will be necessary to consider the early development of the central portion of the ovum.

We have already given Purkinje's description of the production of the blastoderma, or germ-membrane, consequent upon the bursting of the germ-vesicle in the hen's egg. There it lies, at first, upon a small portion only of the spherical surface of the yelk; but its periphery is soon extended farther. So rapid indeed is the increase, that at the end of the second day, according to Baër, the germ-membrane covers one half of the surface of the yelk, and at the end of the fifth, the whole of it. As it advances, the yelk-membrane becomes more and more clear and thin on its exterior, and gradually disappears—a broad band of the advancing germ-membrane closely adhering to the retreating membrane of the yelk. This slow and gradual formation of the blastoderma in the bird's egg appears to be superseded in that of mammalia. For here it is found surrounding the whole of the minute yelk, as soon as the latter is so transparent, from solution of its granules after the bursting of Purkinje's vesicle, as to admit of observation. It is presumed, therefore, to be vesicular from the beginning, and Coste appears to be the first observer who insisted upon the probability that it is so.

The modern theory of development, as first proposed by Döllinger and Pander, and successively illustrated by Baër, Rathke, Burdach, &c., proceeds upon the separation of the blastoderma into superimposed layers or laminæ. Above, and most extended, is the *serous* layer; below, and least extended, is the *mucous;* between the two, and later in its appearance, is the *vascular layer*. In one or other of these, as distinct primitive forms, there lies concealed that which is essential in the different organs and tissues of which the body is composed, and in virtue of which they admit of being referred to distinct original groups.

On the serous layer arise the organs of animal life—the brain and spinal cord, organs of senses, skin, muscle, tendons, ligaments, cartilage, bone; on the mucous, the organs of vegetative life—the intestinal canal, the lungs, liver, spleen, pancreas and other glands. The heart and vascular system arise from the vascular layer, " if this is to be considered as a separate layer" (Val.) To which division the generative system is to be primarily referred is still undetermined. Rathke says the vascular.

The separation into two layers, serous and mucous, occurs in the bird's egg, about the twelfth hour of incubation. The mucous layer is much more delicate in the central clear part of the blastoderma, the area pel-

lucida, than in the part external thereto. About the twentieth hour, the layer of granules, forming the vascular layer (Pander), appears.

About the fourteenth hour, a mass of globules, loosely connected together in the transverse axis of the egg and centre of the pellucid area, is seen,—Baër's primitive streak: it is formed on the serous layer (Fig. 12).

The globules of the primitive streak next seem to be resolved: and then there is a change of appearances. On the sides of the streak are the two laminæ dorsales, which bound a median furrow; and below this furrow is the *chorda dorsalis*, which is the axis of the future embryo, and origin of the spinal column. That portion of fluid which separates the chorda dorsalis from the laminæ d., is the future cord and brain (Fig. 13). The chorda dorsalis thickens at the fore part, to form the first appearance of skull, and the fluid between the dorsal laminæ is in larger quantity in correspondence with it; so that the central parts of the nervous system, and their coverings, are laid down at the same time, and grow simultaneously. The separation between the spinal cord and brain is a very early one, and is coincident with a bending downwards towards the yelk of the anterior parts of the laminæ dorsales, which defines the limit between skull and column, brain and cord.

These occurrences have been observed in all the other vertebrate classes, more or less distinctly, except the human. Here, in the absence of direct observations, they are supposed to be referrible to the end of the second, or beginning of the third week.

Next follows the closing of the laminæ dorsales over the fluid which is the rudiment of both brain and cord. The brain, therefore, as Valentin remarks, ought not to be considered as growing from one extremity of the cord. At first there is only a single cerebral vesicle; for in the brain, as well as in the cord, granules accumulate first on the periphery, the central parts continuing to be fluid. The single vesicle is then elongated, and next appears constricted in certain regions, so as to form three cells, which communicate. The anterior cell corresponds to the cerebrum; the middle cell to the corpora quadrigemina and neighbouring parts; the posterior cell to the medulla oblongata and neighbouring parts. These three cells have been observed by Baër, in the dog; by Rathke, in the sheep; by Müller and Burdach, in man.

The deposit of granular matter which accompanies the further development of the brain and cord is seen on that side of both which corresponds to the viscera, sooner than on that which corresponds to the spine.

Two other laminæ (laminæ ventrales of Baër) are in the mean time proceeding from the axis of the embryo, one on each side. They grow out laterally, and tend to converge in the median line, as did the dorsal laminæ; but they form a larger curve, and follow a different direction—that is, they converge to meet *below* the axis: and they do so meet, except in the umbilicus (Figs. 14 and 14^1).

The mucous layer of the blastoderma gradually surrounds the yelk in birds, and at length covers it, in place of the vitellary membrane. In mammalia, like the blastoderma, it is supposed to have surrounded the yelk from the beginning. It soon permits of division into two portions, which communicate. The smaller portion, which is first beneath and then within the laminæ ventrales, the original of the intestinal canal and

its appendages and of the lungs, communicates with the rest of the yelk by the constricted intermediate portion, which becomes narrowed into a duct. (See Fig. 15, where the constriction is far advanced.)

When the earliest vascular system has been established upon the yelk, in mammalia, it takes the name of umbilical vesicle, or tunica erythroïdes. In birds, the yelk appears to be almost exclusively for nutrition of the embryo; its vessels probably derive little from intercourse with the atmosphere. In mammalia, on the contrary, nutrition seems to depend, in a very subordinate degree, upon the contents of the yelk-sac, and rather to be conducted by its vessels, as long as they subsist, by processes similar to the respiratory. Hence the bag is never drawn into the body of a mammal, as in birds, though in them the blood-vessels are more largely disposed upon its surface; but, by gradual lengthening of the intermediate tube, the bowel and the yelk-sac become distinct portions of the vegetative part of the germ (see Figs. 19, 20). And then, at various periods in the different families, the connexion ceases. It seems to persist longer, in proportion to the size which the sac attains, for it grows, even in man, for a time; but the duct is never open in any mammal, on its exit from the ovum, as in birds. There is great variety in external form and size of the yelk-sac, in the different families of mammalia. In most instances it deviates greatly from its early spherical form. In the ungulata, it is drawn out into very long thready productions, from either extremity of a wider middle portion, the former disappearing when the chorion is fully formed. In ruminants, even at a very early period, the middle portion of the sac is alone provided with vessels; and these soon disappear. In the hog, this middle portion persists for the first half of gestation. In the carnivora, the yelk-sac slowly changes its spherical form for that of an ellipsoïd, or spindle: it continually absorbs fluid, and therefore attains a remarkable size; it long maintains an open communication with the intestine, and preserves its rich vascular network until the period of birth. In man, the umbilical vesicle is contrasted to the yelk-sac of the other families, in preserving its form nearly spherical, but agrees with many of them in continuing very small (about four lines in diameter), and in ceasing early to take any share in the elaboration of the embryo. According to Velpeau, the ductus omphalo-entericus is obliterated in man in the fifth week. In some mammalia, the vessels persist, even when the duct is shrunk or obliterated, and the sac collapsed. Such is the case in the horse, the vessels being apparent in the eighth month, though the walls of the sac have fallen together in the seventh. In the rodentia, the cavity of the sac is empty in the mid-period of gestation; and its walls have either grown together, or have joined the chorion, on the one hand, and amnion on the other; but the vessels persist, as tunica erythroïdes, until the period of birth. And in fishes, and the higher amphibia, these vessels are the only bond of union between the vesicle and the embryo, in the later periods of fœtal life.*

It now remains that we notice two very important appendages of the serous and mucous laminæ of the germ-membrane, one the amnion, the other the allantoïs, with its vascular layer.

When, towards the centre of the serous layer, the rudiments of the organs of animal life have been laid down, there still remains a peripheral portion, which is extended over the surface of the yelk. Of this last, a

* Baër, ii. 190 et seq.

fold leaves the edges of the yet unclosed ventral laminæ, and leaving also the mucous layer and yelk, arches over the dorsal surface of the embryo, so as to represent a sac whose opening is the edge of the fold (Fig. 14, *t*, *u*). As the fold advances, the opening is constantly narrowed, and at length vanishes in a point, when the two surfaces of the duplicature constitute two distinct membranes (Fig. 15, *t*, *u*). That membrane which is next the embryo, and which includes it, being continuous with the edges of its ventral laminæ, is the amnion; that which is farthest from the embryo, and which passes into the serous lamina of the blastoderma, on the surface of the yelk-sac, is the "false amnion" of Pander, or the "serous covering" of Baër. That this singular process is conducted in birds in the manner thus described is now, we presume, universally admitted. In mammalia, it is effected with much greater rapidity, yet still in the same way: for Baër has observed it in all its stages, in the dog, the sheep, the pig; he has seen the embryo at first quite uncovered—then partially—then completely inclosed. The amnion is found to lie at first more closely upon the embryo of mammalia than of birds; but in both classes the serous covering, or false amnion, is soon separated from it, and includes both yelk and amnion. Dr. Allen Thomson, in his paper on the human ovum,* fully confirms Baër's account of the formation of the amnion in mammalia. Nor ought it to surprise us: we have seen that the central portion of the serous layer sends the laminæ dorsales arching upwards, to form a tube round the elements of the nervous system—and this is only a repetition of that process for the peripheral portion of the same layer, (see Fig. 22.)

Respecting the "serous covering," Baër remarks,

> "It has not previously been observed in the ova of mammalia; and yet without a knowledge of it these ova cannot be understood. Thus Cuvier remarks, that in the ovum of the rabbit the external membrane, which he calls chorion, is dissolved. Since however he recognizes, in addition to chorion, only amnion, allantoïs, and yelk, it follows that these sacs in the rabbit must lie free, and without a containing membrane, which is by no means the case (Fig. 20). It seems to me that we should not meet such strange contradictions amongst authors, in regard to the interpretation of the various portions of the human ovum, if this membrane were taken into account, which undergoes change more rapidly than any other, and which, from its passive rather than its active comportment, presents very different appearances in the same ovum, at periods nearly consecutive. In general, it moves from the yelk-sac towards the outer membrane of the ovum, with which it becomes incorporated. But when and how it gains that situation, depends upon other parts which urge it." (*Baer*, vol. ii., p. 192.)

We are surprised to find that M. Coste gives an account of the formation of the amnion, totally different from that of which we conceive the truth to be indisputably established by Baër and others. According to Coste, the serous portion of the blastoderma consists of two layers, of which the external is detached, like a pellicle, to cover the embryo in the shape of amnion, and to be reflexed into its interior in the shape of peritoneum, from whence it passes again outwards, over the surface of the yelk. Coste admits that direct observation cannot establish the stratification of the serous layer which he insists upon. He is aware also, that his description necessarily involves a denial of the existence of the false amnion; for, speaking of the umbilical vesicle, he says, that "it sometimes persists in animals, until the period of birth, and then only

* Edinburgh Med. and Surg. Journal, No. cxl.

serves as a coif for the embryo, with its double envelope, which has led to the belief in a false amnion."

The allantoïs arises on the fore part of the posterior extremity of the mucous layer, which is closing to form the intestine, as a growth of the intestine which proceeds very rapidly. It passes out where the ventral laminæ are still unclosed, in the region of the umbilicus, and in birds and mammalia reaches, either mediately or immediately, the inner surface of the exochorion. (Marked f^1, in all the sections from No. 17 to No. 22.) By the constriction of the navel it is separated into two portions, which communicate: that within the body of the embryo is the sacculated urinary bladder, with the urachus or tube of communication. It receives its vessels from the hypogastric, which are spread out as a vascular layer, especially upon that portion of its surface which faces the exochorion. According to Burdach, the vessels form a distinct layer, endochorion. This is certainly the case in pachydermata, Fig. 19, marked (*g.*) They perforate the albuminous matter which generally intervenes on the inner surface of the exochorion, and, reaching the latter membrane, form with it the placenta.

The form of the allantoïs is subject to remarkable differences in the different classes of mammalia. In carnivora, it resembles the corresponding membrane in birds,—passes from right to left over the back of the embryo, and is only prevented returning into itself by the intervention of the yelk-sac, which is to the left, and below. Towards the surface of the ovum, its opposite portions meet, so that there remains only a triangular space towards the centre, occupied by the yelk-sac. In this class the vascular and mucous layers of the allantoïs do not separate (Fig. 17).

In ungulata, the allantoïs grows so little in breadth, that it does not form a double arch over the amnion, but lies by the side of it. In length, however, it increases so remarkably, that, notwithstanding the great length of the ovum at the earliest periods, it bursts the outer membrane at each extremity, and passes out. Here the mucous and vascular layers separate from each other completely, as soon as the allantoïs has reached the albuminous matter on the interior of the exochorion, and the vessels pass through that matter to seek the villi of the external membrane (Fig. 19).

In rodentia, the allantoïs neither arches over the amnion, nor lies beside it, but keeps the situation opposite to it on the ventral surface of the embryo. It is cylindrical, but small when compared with the size it attains in the two former families. Its vascular layer surrounds it, and then leaves it to form the placenta (Fig. 20, *f*).

In man, the allantoïs continues very small, and appears to be active only in the earliest periods. How the vascular layer reaches the exochorion is not known: it may be raised from the allantoïs, as in the ungulata, and resting upon the amnion, more or less, may pass through the albuminous matter, which in that case would be collected between it and the allantoïs: or the allantoïs may itself bear its own vascular layer to the inner surface of the exochorion. Baër inclines to the latter opinion—because, if the process occurred by separation of the vascular from the mucous layer of the allantoïs, then the vascular layer ought to be found, for some weeks at least, resting upon the amnion in the human subject, as it is in the ungulata throughout the whole period of gestation: but it has never been observed by him in that position* (Fig. 21). We

* These statements are abridged from Baër, vol. ii., p. 195.

may state, however, that Burdach, Müller, and Bischoff, believe that the endochorion leaves the allantois as a single lamina, to join the exochorion.

In the valuable tract by Professor Müller, "De ovo humano atque embryone," published at Bonn, in 1830, the author gives the dissections of two ova, presumed to be of the sixth week, and of one presumed to be of the fourth, or thereabout. In one of the former, there was an appearance which concerns the present subject.

"The amnion was reflected towards its own cavity, forming a sheath for the cord; but that sheath was very short, and opened into the gelatinous space between chorion and amnion, at a distance from the spot where those two membranes were connected together, and where the umbilical vessels spread into the chorion. From the sheath of the cord there proceeded, on the one hand, a very slender duct, which ran along the outer surface of the amnion, and terminated in a pretty large umbilical vesicle filled with a thick substance, and presenting blood-vessels still visible; and, on the other hand, the umbilical vessels, still full of blood, proceeded from the sheath to the outer surface of the amnion. These vessels ran a considerable way in the opposite direction, resting closely upon the outer surface of the amnion, until they entered the chorion at that part where it was joined to the amnion. We must distinguish, therefore, between that part of the cord which is inclosed in a sheath—a small part, indeed, in this case—and that external part of it which is not so inclosed, and where the duct of the umbilical vesicle, and the umbilical vessels, proceed in different directions, yet both resting on the outer surface of the amnion, the one to terminate in the umbilical vesicle, the others to ascend to the chorion."

Müller describes the short portion of the cord included in the sheath, as of great thickness, seeming to hold another vesicle, perhaps the allantoïs, in its interior. He found fluid; but, on account of the extreme tenderness of the parts, he could not satisfy himself respecting their exact relations. This dissection would lead us to conclude, that in man the allantoïs is early shrunk, whilst its vascular covering, the endochorion, is especially developed. That vesicle has been found within the cord, in the position where it was sought for by Müller, in cases recorded by Meckel and Albinus.

We have seen that M. Coste represents the vesicular blastoderma as being transformed by constriction into two lobes, inclosed within his vitellary membrane, the amnion being detached as a pellicle or epidermis from the surface of the blastoderma. He continues the same hypothesis with regard to the allantoïs, which, according to his views, arises by another constriction, as a third lobe of the vesicular blastoderma, between the umbilical vesicle and pubis of the embryo. It expands and grows; and as the ventral laminæ of the embryo become closed, there are two vesicles with their pedicles projecting from its interior, viz. the umbilical vesicle and the allantoïs. At length the caudal extremity of the embryo is fixed to the chorion and uterus, by means of the allantoïs and its pedicle, the cephalic extremity hanging free. A spiral motion occurs round the support, and the two pedicles being in contact, are twisted together. The pedicle of the allantoïs, however, is essentially the future cord,* and the allantoïs, enlarged and flattened, the placenta.

We need scarcely say, that we consider this theory as involving great error; as far as we know, we have stated it fairly, though it is only to be collected by reading a large portion of M. Coste's volume. It does

* Coste, 279.

not account for the existence of a common vaginal portion of the cord which, as every one knows, includes the pedicles of both umbilical vesicle and allantoïs: nay, according to it, but for the spiral twisting, the embryo mammal ought to have two navels. These difficulties are necessary consequences of M. Coste's false estimate of the mode in which the amnion is formed. That error constrains him to consider the allantoïs as formed of all the laminæ of the blastoderma, and not of two only (as it really is), the mucous and vascular. We may add, that according to this view, no explanation can be given of an irregular membrane, frequently observed in a more or less perfect state, between amnion and chorion, and which is the false amnion of Pander, or serous covering of Baër; but which, as we have before mentioned, M. Coste rejects altogether.

Some observations of Baër seem to show that the albumen which is collected under the external membrane of the ovum, exerts a peculiar attraction upon the vessels of the allantoïs. In the pig, the larger trunks reach the layer of that substance on the twenty-third day, on an average: but before the next twenty-four hours have elapsed, the entire vascular lamina of the allantoïs is found to rest upon the exochorion and its albuminous layer. The vessels shoot into the villi of that outer membrane, which rapidly increase in size. From this period, the vascular and mucous layers of the allantoïs appear to separate, and the former to attach itself exclusively to the exochorion. And now (i. e. at the end of the fourth week) vascular layer, albuminous layer, false amnion, exochorion, and villi, grow together, and form a single membrane, of which the lately distinct elements can no longer be separated, the vascular chorion (Fig. 19).

The albuminous substance which appears to play so important a part in guiding the vessels of the embryo to the villi of the chorion, has frequently been noted in dissections of early human ova. Thus Müller, in the work just referred to, in his description of one of the ova in the sixth week, says, "Inter amnion et chorion inde ab insertione funiculi umbilicalis spatium magnum intercedit, substantia gelatinosa plenum, filis varie per gelatinosam materiem ab una ad alteram membranam trajicientibus."* And Dr. Allen Thomson, in examining a human ovum, which he considered to be certainly not older than twelve or fourteen days, found that "the space intervening between the outer surface of the umbilical vesicle and the inner surface of the chorion, was occupied by a thin, tenacious web of albuminous filaments, probably formed by coagulation in the spirits."† And again, in an ovum fifteen days old, he found the same reticulated web;‡ and again in one aged five or six weeks.§ This is the substance which Velpeau and others have supposed, in the human ovum, to be analogous to the allantoïs of animals,—but erroneously: for, when found, the human allantoïs is always either within the vagina of the cord, or lying as a small compressed vesicle between chorion and amnion, with its duct more or less in the interior of the cord. Dr. Allen Thomson's first two ova, however, supply conclusive evidence that Velpeau's "magma reticulé" is not the allantoïs, for in them the embryo was in so early a stage of development, that it lay flat on the surface of the um-

* Op. cit., p. 8. † Edinburgh Medical and Surgical Journal, cxl., p. 129.
‡ Op. cit., p. 132. § Op. cit., p. 133.

bilical vesicle, the intestine not yet formed, and without allantoïs or amnion.

There must be a concurrence of many structures, to secure the formation of a placenta. This we learn from Baër's valuable observations of the ova of mammalia, in the second volume of his work. The vessels of the allantoïs are conducted to the outer membranes of the ovum, by the albuminous layer which is collected beneath it. They proceed there, in order that capillary meshes may be formed round the villi which are in near connexion with the uterus; and they dwindle, and finally disappear, where the villi are not found, or do not increase in size. But villi are only formed on the original external membrane of the ovum. When that external membrane is burst, as it is in carnivora, rodentia, &c., by the rapid growth of the allantoïs, or the yelk-sac, no villi are formed upon the membrane which projects through the openings in it. Nor is the presence of villi the only necessary condition for the further development of the fœtal vessels; there must also be near contact with the uterus, and with the vessels of that organ which, in consequence of the gravid state, are also becoming largely developed. Wherever the villi of the mucous surface of the uterus and their vascularity are undergoing increase, there do the villi of the ovum and its vessels grow out to meet them. And the placentæ will have different shapes, in the different classes of mammalia, according to the forms in which the uterine vessels are developed. In the ruminants, the villi and vessels of the uterus are developed in patches, which form what are called cotyledons: the fœtal placentæ in this class are multiplied to meet them; whilst the uterus on the one hand, and the exochorion on the other, is smooth in the spaces between the cotyledons. In carnivora, the placenta is belt-shaped. In pachydermata, it occupies the whole surface of the uterus, &c. &c.

In all the mammalia, either before or after the passage of the ovum into the uterus, there is found to be effused on the surface of that organ, wherever its vascularity has been exalted, a gelatinous or albuminous secretion. In the human subject, this secretion is the decidua of Hunter, about which so much that is unsatisfactory has been written. It was Hunter's opinion that it covers the whole uterus, with the exception of the three openings into its cavity; an opinion supported by Bojanus and R. Lee. On the other hand, by far the greater number of observers, as Lobstein, Moreau, Breschet, Velpeau, Carus, Burns, trace the gelatinous matter into the tubes, and show that it also closes the neck of the uterus. When the ovum is in the uterus, it is found to be surrounded entirely, or almost entirely, by another gelatinous membrane, apparently similar to the former, and which supports the ovum in near connexion with the wall of the uterus—the decidua reflexa. Different writers maintain very opposite opinions respecting the mode of formation of the reflexa, according as they believe the ovum to attain the interior of the decidua through the opening opposite to a tube, or to push away that decidua which closed the tube, and tended to intercept its passage. The second opinion accords best with observation. Dr. Allen Thomson found, in examining the uterus from which the ovum of fifteen days was taken, that the decidua vera was generally one fourth of an inch in thickness. "The cavity of the decidua was occupied by the usual fluid, and from the posterior wall of the decidua vera, there projected forwards the swelling of the decidua

reflexa, containing the ovum imbedded in its substance." So that, as we stated in our Number for July last, when reviewing Dr. Meig's work on midwifery, the ovum must be supposed to glide between the decidua and walls of the uterus, and to carry the decidua before it. Bojanus's observations led him to suppose that for a time there is thus left a portion of the surface of the uterus, between the ovum and it, uncovered by decidua vera,—and that this portion subsequently receives a covering, as a new secretion from the uterus, which he called *decidua serotina.* And this is also Baër's view of the matter, in consequence of his own observations. Both Bojanus and Baër represent the uterine vessels as shooting into the decidua serotina, and so forming the maternal portion of the placenta. In Dr. A. Thomson's case, "the decidua adhering to the inner surface of the mucous membrane of the uterus presented, throughout its whole extent, small blood-vessels, which passed from one membrane to the other." It would seem, in this instance, that the soft and ductile decidua vera closed over the ovum, between it and the uterus, after having allowed the ovum to pass on,—and so involved it in its thickness.

M. Coste maintains the opinion that the caduca is formed in the human uterus, as an adventitious product, *after* the descent of the ovum into its cavity, and supposes that, when formed previous to the descent, it may be a cause of abnormal detention of the ovum in the tube. He rejects the reflexa altogether. Ninety pages of M. Coste's volume are dedicated to criticism of the opinions of others on the subject of the decidua. He denies that it is supplied with vessels from the uterus, and speaks of a maternal portion of the placenta as of a mere phantom of the imagination.

Comparative anatomy teaches us that in all mammalia which have the organ, there are two portions of the placenta; one having its vessels from the embryo, the other from the uterus: and that the two orders of vessels do not communicate.

We shall now review shortly the structure of the placenta in different families of mammalia, in order that—having determined which of them most nearly resemble the human—we may judge how far they can guide us in our estimate of the true structure of the human placenta. That structure, as our readers are aware, has been very differently described by different writers of eminence, who all found their opinions upon observation of facts. Dr. W. Hunter says, in his *Anatomical Description of the Gravid Uterus*, when speaking of the maternal and fœtal portions of the placenta, "each of these parts has its peculiar system of arteries and veins, and its peculiar circulation, receiving blood by its arteries, and returning it by its veins; that the circulation of these two parts differs in the following manner:—in the umbilical portion the arteries terminate in the veins by a continuity of canal, whereas in the uterine portion there are intermediate cells into which the arteries terminate, and from which the veins begin." On the other hand, Dr. R. Lee concludes from his observations (as does M. Coste), "that the placenta does not consist of two parts, maternal and fœtal, and that there is no communication between the uterus and placenta by large arteries and veins. The whole of the blood sent to the uterus by the spermatic and hypogastric arteries, except the small portion supplied to

its parietes and to the membrana decidua by the inner membrane of the uterus, flows into the uterine veins or sinuses, and, after circulating through them, is returned to the general circulation of the mother by the spermatic and hypogastric veins, *without entering the substance of the placenta.* The deciduous membrane being interposed between the umbilical vessels and the uterus, whatever changes take place in the fœtal blood must result from the indirect exposure of this fluid, as it circulates through the placenta, to the maternal blood in the great uterine sinuses."*

In the mammalia, in all cases, the fœtal portion of the placenta is formed, as we have seen, by the ramifications of the hypogastric vessels in the villi of the exochorion. In ruminants the ramified villi are collected into masses, called cotyledons: and here the maternal cotyledons which grow to meet them are formed principally from the substance of the uterus. In these elevated masses there are branching cavities, which correspond to the form of the fœtal villi, and receive and embrace them. The uterine vessels surround these cavities, and appear (according to Baër) also to penetrate, to a certain extent, a secreted matter on their surface,—the portion which thus becomes vascular of that matter growing to the surface of the maternal cotyledon.

In the sow there are no cotyledons. The surface of the ovum and of the uterus both present minute folds, which are mutually in near contact—a fold of the exochorion being received between two adjacent folds of the uterus, and vice versâ. In the later periods of gestation the uterine folds, always transverse to the axis of the cavity, are connected by many conjugate folds, so as to assume the form of cells. At the same time the fœtal folds, which were from the first somewhat denticulated, are prolonged into villi, which are received into those cells. The corresponding structures are largely supplied with fœtal and maternal capillaries, which nowhere communicate.

Eschricht describes the placenta of a porpus, in which the rugose chorion is studded with villi closely packed together. On the surface, also, of the uterus are rugæ, but less strongly marked,—with interposed cells, into which the villi of the chorion are received. Each surface has its network of capillaries, which nowhere communicate.

In all these families, the capillary network of the fœtal vessels, though brought into near connexion with the capillaries of the mother, are still separated from them by an epithelium and a mucous secretion. These placentæ have further this common character, that the maternal portion continues in the uterus after birth, the fœtal portion only being thrown off; the separation, too, taking place without rupture of vessels, and without hemorrhage.

Of the human placenta, on the contrary, both the fœtal and maternal portion is extruded at birth, and always with considerable hemorrhage, in consequence of rupture of the maternal vessels; which vessels, according to the generally received opinion, are in the decidua. But this is not peculiar to the human placenta; the same thing occurs in monkeys and in the carnivora.

In the human placenta the villi of the exochorion are longer, thinner,

* Phil. Trans. 1832. p. 63.

more ramified, than in other mammalia, and were therefore long supposed to be vessels. But Lobstein and E. H. Weber have shown that each villus is supplied from the umbilical vessels with a small artery terminating in a small vein on the rounded extremity of the villus. The important question then remains—by what are the apices of the villi, which are collected into masses forming the lobules of the fœtal placenta, surrounded? The Hunters taught, as we have seen that the uterine vessels, after passing through the decidua, open into large cells, formed by cellular substance which loosely connects these lobules. The description of E. H. Weber* has been believed very generally by continental writers to approach the truth more nearly. According to him, the large vessels which leave the uterus to pass into the decidua are deprived of all except their innermost tunics, which are found as soft and tender as coagulated lymph. The veins form a network, and have this peculiarity, that they become wider the more deeply they penetrate between the lobules. Thus the veins themselves form cells or sinuses into which the fœtal villi project. The delicate and yielding coat of the vein is borne inwards by each villus pressing upon its exterior, and so is itself the covering of all the villi which compose the fœtal lobules, and which seem to project into its interior.

Professor Eschricht, one of the physicians of the lying-in hospital at Copenhagen, had an opportunity of examining the placenta, in the case of a woman who died in the moment of delivery. The uterus was carefully removed, the placenta being still retained. The directions of Weber were implicitly followed, the umbilical arteries and veins being alone injected, and not the uterine vessels. And the preparation was exhibited to the Royal Medical Society of Copenhagen, in proof of the accuracy of Weber's description: for, as Eschricht then thought, the fœtal villi were in places seen hanging down into the interior of the venous sinuses. On more careful examination, however, when the sinuses and their large branches were laid open, in many of them no villi whatever could be detected; which appeared the more remarkable when their great number was considered. On further scrutiny, those projections which had before been presumed to be villi were found, beyond dispute, to be merely fibrous threads derived from bloody coagula.

The investigations of Baër having shown a considerable resemblance between the placenta of the carnivora and the human, inasmuch as in both classes not merely the fœtal portion, but the uterine also is extruded at birth; the Professor betook himself to the former, in the hope of clearing away the difficulties which seemed to thicken round the subject. In a cat, immediately after death by drowning, the uterine veins were filled with blue, the arteries with white injection, afterwards the umbilical vessels with red. On dissection, the placenta was found almost entirely detached. Still, as not only the external surface of the placenta, in contact with the uterus, was of a blue dye, but also presented intensely blue points and white pendulous threads, it was concluded that the injection had succeeded. And of this there could be no longer any doubt, when on the fœtal surface, besides the red vessels and their

* Hildebrandt's Anatomie, vol. iv., p. 497.

capillaries, another beautiful ramification of vessels, white and blue, was also clearly visible beneath them. The structure of the placenta was then accurately examined. The blue dye on its uterine surface was found to have its seat in a friable and almost grumous mass, resembling the human decidua. Here the uterine veins, or rather their dilatations only, terminated; for intensely blue points still were seen on the blue ground. White points or threads, indicating the uterine arteries, were also seen, on careful examination, on every part of the same ground. When this soft stratum, corresponding to that part of the human decidua which invests the placenta, was removed, the same membrane, though less soft and friable, was found to penetrate the substance of the placenta. This, according as it was viewed in different directions, presented singular interlacings of the red, blue, and white injections, whilst careful examination of any minute portion proved that the fœtal and maternal vessels did not communicate. The red fœtal capillaries adhered to the most minute laminæ, the villi of the chorion, as described by Baër in the last period of gestation. The blue and white network was composed of vessels three or four times larger than the red, and adhered to the thick and soft laminæ which were evidently productions from the membrane covering the uterine surface of the placenta. It is Baër's opinion that the fœtal laminæ are included in these last; and Eschricht believes it to be well founded.

Since the maternal portion of the placenta is formed in ruminants, pachydermata, and cetacea, almost entirely as a growth of the uterus itself, it might perhaps be concluded analogically, that in carnivora also that portion is merely an altered form of the mucous surface of the uterus, and not a new production during the gravid state. And further, this conclusion might appear to be confirmed by the fact that the vascular villi of the cat are invested by the vascular uterine membrane, according to the disposition observed in those families. Eschricht, however, assures us, that when the surface of the uterus, from which the placenta has been detached, is examined, the mucous membrane is still found to cover it: it is thick, soft, and jagged, from rupture of vessels, but has no resemblance to the decidua; "valde rugosa erat (sc. membr. muc.) et mollis quidem sed neque grumosa nec quasi dissoluta, ita ut membranæ placentam investienti haudquaquam esset similis."

M. Flourens, about the year 1836, revived the notion of a direct communication between uterus and fœtus, in certain families of mammalia, by means of considerable vessels.* He was led by his experiments to divide the mammalia, in this respect, into two classes: viz. those which have a vascular communication by means of a single placenta, and those which have a communication merely by contact or adhesion, by means of multiple placentas. The former case he called communication by continuity, the other communication by contiguity. M. Coste shows, from physiological and other considerations, that Flourens must be wrong. It is more satisfactory, however, to find, from Prof. Eschricht's injection and dissection, that the presumed continuity in one important instance resolves itself into contiguity.

The human placenta and that of quadrumana have, as Hunter pointed out in his paper, undoubtedly very remarkable peculiarities; but let us

* Comptes rendus des Séances de l'Académie des Sciences. 1836. p. 170.

not forget the resemblances which they present to some of those which we have been considering. Now the human placenta resembles the feline in being totally ejected at the period of birth, not merely the fœtal portion, as in pachydermata, ruminantia, cetacea, but its uterine portion also. In both, again, the decidua enters between the lobules, conducting vessels of considerable magnitude. And if the form of the decidua be different, as indeed it is, in these two classes, that difference is referrible to the form of the fœtal villi, which in the human exochorion are arborescent, in the feline laminated. But we are at a loss when we attempt to carry the parallel further; for we do not know how far the decidua penetrates into the interior of the human placenta by means of its productions, nor how the uterine vessels are distributed in the interior of that viscus. Of course we presume, with far the greater number of anatomists, that here the decidua does enter between the lobules, and that it conducts vessels of considerable magnitude, and may suspect that the villi of the exochorion, here so delicately subdivided, do not merely form, as M. Coste supposes, a confused agglomeration, a tangled compressed mass to be clapped upon the walls of the uterus.*

As to the depth to which the decidua penetrates the substance of the placenta, the history of the ovum (says Prof. Eschricht) supplies the best answer. All allow that the villi of the chorion do penetrate the decidua; and therefore the decidua, at least in the earliest periods, supplies vaginal coverings for the villi. What reason then is there for supposing that, at a later period, their ramifications are not similarly covered? It may be more difficult, indeed, to demonstrate a covering now so much the thinner as the ramifications are more numerous, and which can, with difficulty only, be seen distinct and separate from the large vessels which traverse it. If villi, however, be examined with a microscope, says the Professor, they will be seen to be covered with a grumous mass which is doubtless a production of the decidua; and adds that he should scarcely have noticed the vaginæ of the feline villi, though pretty thick, had they not been injected.

With respect to the other question as to the mode in which the uterine vessels terminate in the interior of the placenta: they are supposed to degenerate into crypts or sinuses. But this answer is not satisfactory. For when a change takes place in blood conveyed by vessels, the most minute vessels are, in all other cases, those which are most important. Absorption and secretion are performed by capillaries. And if such a network of uterine capillaries has never yet been seen in the human placenta, Dr. Eschricht very pertinently asks whether any observer has hitherto sought for it by the aid of the microscope in a placenta filled with coloured matter from the uterus? He alleges that no sufficient reason has yet been adduced for concluding that there is no system of uterine capillaries in the placenta intermediate between the arteries and veins.

Doubtless there is much difficulty in conceiving such sinuses as Weber has described, so formed as to receive all the infinitely numerous villi of the exochorion projecting pendulously into their interior from behind their coats. Eschricht avers that he has never seen any such appearance

* Coste, p. 150.

as this; but, on the other hand, that he has often seen the villi covered with decidua. He says that a piece of decidua, torn off for examination, seems, under the microscope, to be studded with fibrils which are obviously the villi of the chorion inserted by their apices firmly and deeply into that membrane. It is plain that *these* villi at least have never floated in any venous sinus. But the supposition is scarcely admissible that one set of villi is thus covered with decidual vaginæ, and that another set is disposed of in sinuses.

Professor Owen, in a letter published by Dr. R. Lee in his paper to which we have before referred, when speaking of the uterine veins which appeared to him to be in general closed "by the apposition of the deciduous membrane and placenta," adds: "the decidua is certainly thinner opposite these orifices than elsewhere; and in some places appeared to be wanting, or, adhering to the vein, was torn up with it; but in these cases the minute vessels of the placenta only appeared, and never any indication of a vascular trunk or cell commensurate with the size of the vein whose terminal aperture had been lifted up from the part." In this there is nothing repugnant to the opinion that the smaller vessels and capillaries may be continued from the uterine vessels which, in some instances, are admitted to have traversed the decidua, and in other instances were closed in consequence of the subsidence (as may be supposed) of that soft and flaccid membrane. The microscopic examination of a monkey's uterus and placenta injected, after the manner employed by Eschricht in the cat, immediately after death, by an observer of Professor Owen's accuracy and skill, would doubtless settle the interesting question of the mode in which the uterine vessels are distributed to the substance of the placenta in the quadrumana—and, by a not unfair inference, in the human subject also. This is a matter deserving the consideration of a society with ample means like the Zoological.

With the exception of the marsupialia and monotremata, which appear to have no placenta, all other mammalia are, in respect of that organ, separated by Prof. Eschricht into two classes. In the one there is a noncaducous uterine portion of the placenta, in the other a caducous uterine portion. To the first class belong the cetacea and all the ungulata; amongst the latter the ruminants being distinguished by the singular form of their cotyledons. To the second belong orders having three different types of placenta: 1, Glires; 2, Carnivora; 3, Simia et Homo.

We have now fulfilled the object which we had principally in view in this article: viz. to place before our readers not only the earliest passages in the development of mammalia and of man, but also the conflicting opinions of the latest writers respecting the mode in which the connexion between embryo and mother is effected, and the nature of the connexion. The enquiry has necessarily led us to consider the development of the peripheral rather than of the central portion of the blastoderma. Had we touched upon this latter part of the subject so fully as its importance demands, we must have occupied an unwarrantable extent of our present Number in a single disquisition. With respect to it, we have merely shown how the primitive organs or tubes are formed; and have avoided almost entirely the subsequent *morphological* modifications of these and

their *histiological* separations—or, in ordinary language, the production of the several organs and of the tissues of which they are composed. These may well supply us with matter for some future article, involving, as they do, questions which affect the entire foundations upon which physiology is reared as a science: viz., the origin of the blood, the nature of nutritive secretion and assimilation; the mutual action and reaction upon each other of the differently affected products of those separations to which we have alluded, or the functions of organs, and the external conditions under which alone these actions and reactions can be performed. There are other questions also of the deepest general interest connected with this portion of the subject. How does it assist us in our estimate of the relative perfection of animals and of the mode in which we may legitimately imagine them to be developed from one another? What light is reflected by it upon the relations of the different classes of animals to one another; and upon the progressive development of the entire animal kingdom considered as a whole?

The works which we have placed at the head of this article are all of distinguished merit; two of them of surpassing excellence. To rank amongst the great captains of any science is the lot of very few; their true followers may be many. Baër is one of those illustrious men who have founded the science which has engaged us; and Valentin, though not so distinguished for original discovery, is a successful labourer in the same field: one prepared by education, by taste, by talent, not only to investigate and to adorn the regions laid open by his predecessors, but also to advance their conquests. They who would be imbued with the spirit of the science must study the works of these two authors. With respect to M. Coste, our readers will conclude, from remarks which we have introduced into the body of our article, that we cannot speak of his labours in such decided terms of praise. He is not of the same school, but he is well satisfied to be of another; though whenever he differs from those whose conclusions deserve infinitely greater attention than he affords them, he differs only to involve himself in inextricable difficulties. The execution of his plates is very beautiful. Mr. Jones's paper is a very short one; but he is known as the first observer in this country who detected the germ-vesicle and spot in the ovum of a mammal; and this he claims to have done without previous knowledge of M. Coste's earlier discovery of it: and as the first in any country who discovered these objects in the human ovum. We have already expressed our great satisfaction with Dr. Barry's paper; it is an elaborate and ingenious production. We rejoice to see that the number of our countrymen is daily increasing, who, with Owen, Grant, Allen Thomson, and others, are earnestly contending in the same field where Harvey and Needham and the Hunters formerly achieved a lasting renown, with better means and clearer prospects.

Description of the Plate.

Fig. 1. *Purkinje.* The cumulus of the cicatricula facing the yelk: on its top is a small crater, the inner opening of the pore.

Fig. 2. *Id.* The cicatricula of a laid egg: the inner circle of the blastoderma is raised, so that the cavity of the colliquamentum, the naked nucleus, and the powdery granules are seen.

Fig. 3. *Id.* A segment of a hen's yelk which has been divided in a

direction from the cicatricula to the centre. Here is a space containing a pale fluid, from whence a canal is continued to the cicatricula. The germ-vesicle passes from the centre to the surface in this canal.

Fig. 4. *Wagner.* The ovule with its yelk, germ-vesicle and germinative stratum of *Aranea* s. *Epeira Diadema.*

Fig. 5. *Id. Astacus fluviatilis.* A very small ovule $\frac{1}{80}'''$ with a germ-vesicle $\frac{1}{150}'''$. The existence of a limpid yelk is determined from the distance between the vesicle and outer membrane alone. The germinative stratum consists of many points.

Fig. 6. *Idem.* The human ovule from a drawing by Valentin. Most external is some granulous matter of the disc: next the pellucid zone, which bounds the yelk-membrane and yelk: *within is the germ-vesicle* (elongated by the compressor), *with its distinct, round, single macula.*

Fig. 7. *Id.* The posterior termination of the oviduct of an insect, *Acheta Campestris:* the attenuated portion is full of germ-vesicles, $\frac{1}{100}'''$ in size. Below this portion the oviduct is suddenly dilated: here the granular yelk appears, not included in any membrane; oil-globules in groups (fig. x) are sprinkled about: germ-vesicles (b b) having passed out of the narrowed extremity of the oviduct, are seen of increased magnitude ($\frac{1}{50}'''$ to $\frac{1}{30}'''$). As the oviduct is dilated, the ova are matured, the vesicles increase; each of them is soon surrounded by a yelk and outer membrane, and so the ovules become distinct and separate.

Fig. 8. *Krause* (Müller's Archiv, 1837). Ovule of a goat, which has been subjected to pressure under the microscope. During the process the yelk changes its position within the zona pellucida; the membrane of the latter becomes more visible, the granules of the disc being in part detached and removed; the membrane of the yelk is much thicker, its double boundary is perceptible when magnified 140 times.

Fig. 9. *Jones* (Phil. Trans. 1837). An ovum found in the Fallopian tube of a rabbit the third day after impregnation; magnified forty diameters.

Fig. 10. *Barry* (Phil. Trans. 1838). Ox. The ovum approaching the periphery of the Graafian vesicle (a), being conveyed by (b) the retinacula. Tunica granulosa (c), membrana granulosa (d), (e) the ovum.

Fig. 11. *Pockels* (Müller's Archiv, 1836). The different strata of the theca and nucleus of the deer. (a), Stroma. 1. Peritoneal covering. 2. External stratum of theca. 3. Internal or vascular stratum of theca. 4. Nucleus, ovum, or follicle of De Graaf, its outer membrane. 5. Membrana granulosa.

Figs. 12, 13, 14, 14′, 15, 15′. From Baër. Sections of the chick at early periods of incubation, magnified about three diameters. The yelk membrane is indicated by an interrupted line, the blastoderma by lines of three different kinds, viz., the mucous layer by a thick dark line; the vascular layer by a closely dotted line; the serous layer by a thin clear dark line. The same lines are preserved in the parts into which these layers are transformed. Fig. 12, the serous and mucous layers, with the primitive streak on the former transverse section. Fig. 13, Laminæ dorsales (b c). Chorda dorsalis (a), (B) limits of the vascular area, transverse section. Fig. 14 and 14′, longitudinal and transverse sections at the end of second day. In the former (A) is the edge of the blastoderma;

(B), section of terminal vein of vascular area; (*a*), anterior extremity of chorda dorsalis. The embryo, bent downwards towards the yelk at its anterior and posterior extremities, represents a shield or boat, the blastoderma forming a cap for the head and for the tail of the embryo, (*t*) the anterior part of the fold of the amnion, (*p*′) the inversion of the serous layer, and (*p*) of the mucous layer, a considerable space being left between them where the heart (*v w*) is formed on the vascular layer: from the heart proceed branchial arches, which coalesce to form the aorta (*z*). In the latter, or transverse section 14′, the dorsal laminæ have met and surround the fluid of the central nervous system, (*b d*) the ventral laminæ, (*o*) the aorta, (*h*) the superior angle of the mesenteric lamina of the vascular layer, (*f*) the lateral fold of the amnion. Fig. 15 and 15′. Sections on the fourth day. *Longitudinal section,* (*d*) anterior extremity of intestinal canal, afterwards mouth, (*l*) rectum, (*m*) allantois with its vascular layer. Of the serous layer, the reflected peripheral portions (*p*′ *q*′ *r*′ *s*′) have met, by the junction of the points (*t*) and (*u*) of the last figure, so that the amnion (*p*′ *r*′ *t u s*′ *q*′) is formed, and Pander's false amnion, or Baër's serous covering (*rtus*) at the same time. (*e*) The respiratory apparatus; (*f*) stomach; (*g k*) the unclosed part of the intestine. The advanced formation of the heart is seen; and the breadth of the vascular layer, interposed between the chorda dorsalis and intestine, which forms the mesentery. The allantoïs is between the amnion and yelk bag. In the transverse section: (*m*) Wolff's bodies, the false kidneys; (*h*) the superior angle of the mesenteric laminæ, which meet and surround the intestine, which is closed except at (*l*), where is the ductus omphalo-entericus; (*p*) the section of the allantoïs; (*g*) the point where the folds of the peripheral portion of the serous layer meet to form the amnion and serous covering of Baër. Fig. 16 is an ideal drawing to show the mode in which the central portion of the blastoderma is transformed from a plane into the embryo: (*a*) chorda dorsalis—from which the particles of the serous layer, at first horizontal, proceed in the direction of arcs, to form tubes, an upper and a lower set (the arrows indicate the directions); (*b*) arc of formation of dorsal laminæ; (*c*) ditto of ventral laminæ; (*d*) of nervous tube; in the vascular and mucous layers (*e*) is the directing arc of vascular tube; (*f*) of mucous tube; (α β) central line of all the fundamental organs. In each of these tubes there is a central line and a terminal or closing line, both of which lie in the line α β. The central lines are those nearest to the axis, or chorda dorsalis—the closing lines those farthest from it. Since the primitive laminæ increase from the centre to the perpihery at the same time that each of them curves downwards to form a tube by closing in the central line also, every point which enters into the formation of the tube must follow the curve determined by these two different motions, and indicated by an arrow. (*n*′ *n*) and (*m*′ *m*), would have been the paths described by the points (*n*) and (*m*), which respectively tend to close the lower and upper tubes formed by the serous laminæ, if those tubes had not been formed until the blastoderma had gained all its development horizontally:—but since the tubes are being formed as the blastoderma grows, the paths of *n* and *m* are very different.

The following drawings, also from Baër, show the relation of the

peripheral portions of the blastoderma to the embryo and yelk-sac in their productions—the amnion, serous covering, and allantoïs; and the origin of the placenta in different families of mammalia, as compared with the corresponding processes in the bird.

Fig. 16. Section of the bird's ovum about the eighth day; the embryo being, however, drawn more completely in the long axis of the egg than is natural at this date, for the sake of better comparison. The allantoïs (*f*) has not entirely covered the membrane of the shell. Section of the embryo (*a*), of amnion (*b*): in the interior of the embryo is seen the section of the false kidney and intestine, as in fig. 15, and from the latter the vitelline duct (*c*) passing to the yelk-sac which is entire (*d*). On the yelk-sac are the omphalo-mesenteric vessels and the terminal vein. Section of urachus (*e*) passing into the sac of the allantoïs, of which the external half (*f*) attaches itself to the membrane of the shell, the inner half (*g*), which is the *membrana media* of older writers and the *endochorion* of Dutrochet, surrounds the amnion, and never touches the membrane of the shell. The allantoïs is seen passing from within the embryo on the right side, and not only arching over its back, but also extending in an opposite direction; the former direction is that which is according to rule, but as the sac grows rapidly and is distended by the secretion of the primordial kidneys, it spreads out wherever there is least resistance. It meets with some resistance from the serous covering and albumen. The remains of the serous covering (*hh*) adhere to the yelk-sac beyond the terminal vein, the allantoïs pushing them before it. Beyond the serous covering is the more compact albumen (*i*) still adhering. At (*k*) is seen the membrane of the shell, and around it the section of the shell itself. To assist the comparison of this egg with that of mammalia, a minute space is left between the shell-membrane and shell, in which the villi of the former are seen: this space is not natural.

Fig. 17. Section of the ovum of carnivora: (*d*) is the transverse section of long yelk-sac which lies in a cavity whose formation has not been properly described by any writer previous to Baër. This space is surrounded by thin laminæ (*hh*) which rest on the neighbouring structures. They are the remnant of the serous covering, the part last removed from the yelk-sac: for the vascular area spreads gradually over the entire yelk-sac, and so the serous covering which is then separated from it, is pushed on by the advancing allantoïs. The allantoïs with its vascular layer inserts itself between the yelk-sac and exochorion, and surrounds the embryo with a double covering (its inner and outer half), one on the outside of the amnion, the other on the inside of the exochorion. The mucous and vascular layers are seen to remain inseparably connected throughout the entire allantois, but the vessels of the latter grow into the villi of the exochorion (*xx*), and so form the fœtal portion of the placenta.

Fig. 18. Section of a very young ovum of a carnivore, to show the serous covering in its perfect condition. It still adheres to the amnion at (*n*); the vascular yelk-sac is seen, but the allantoïs is not yet formed. When it is formed, the part of the serous covering which it will remove last, is that which adheres to the part of the yelk-sac most remote from the embryo.

Fig. 19. Section of ovum of pachyderm, pig. The formation of the

chorion is not yet complete. The mucous membrane of the allantoïs is here deserted by the vascular membrane (*g*), which passes on the right hand through the albumen to the villi of the exochorion, and on the left hand rests upon the amnion. Vessels also are seen at (*x*) which leave the vascular layer of the allantoïs to pass into the albumen and villi of the exochorion on the left of the embryo. From these sources the highly vascular placenta is formed. The ovum of ruminants is very similar to this.

Fig. 20. Section of ovum of rabbit. The allantoïs is very long, but small, as the section (*f*) shows. There is some little space between it and the vascular layer, which surrounds it and then gives its vessels to the placenta (*p*). The other parts of the ovum are held together by a very thin membrane (*x*), the serous covering, which having adhered to the yelk-sac passes from it to the placenta. The yelk-sac (*d*) here resembles the allantoïs of carnivora.

Fig. 21. An ideal section of the human ovum, to illustrate Baer's notion of the way in which the vascular layer is conveyed by the allantoïs to the albuminous matter and exochorion to form the placenta: yelk-sac or umbilical vesicle (*d*), allantois (*f*), placenta (*p*), serous covering (*h*) still attached to the amnion.

Fig. 22. The ovum of a pig. The drawing is for the purpose of showing that the amnion is really formed as in the bird. In consequence however of the amnion (*a*) lying very close to the back of the embryo, whilst the serous covering (*h*) is raised to a considerable distance from the yelk-sac (*d*), the opening into the space between amnion and embryo is in form of an infundibulum (*m*), and not a mere oval aperture as in the bird (Fig. 14, *tu*).

Art. II.

Questioni di Medicina Legale, secondo lo spirito delle Leggi Civili e Penali veglianti nei Governi d'Italia. Del Dottore Giacomo Barzellotti, già P. P. di Medicina Legale, ec. nell Imper. e Reale Università di Siena. Ediz. 3a Pisana e 7ma Italica. T. iii.—*Pisa*, 1835-6. 3 vols. 8vo, pp. 493, 704, 514.

Questions in Medical Jurisprudence, based on the Civil and Criminal Laws of Italy. By Dr. G. Barzellotti, formerly Professor of Legal Medicine in the University of Siena, &c.—*Pisa*, 1835-6.

It is now twenty years since Professor Barzellotti first published a work on Medical Jurisprudence: that work, after having passed through many editions with various improvements, has served to form the basis of the present voluminous treatise. The author has followed the example of his great countryman, Zacchias, in attaching the modest title of "Questions" to this monument of his labour and industry; and, although we cannot dispute its appropriateness, yet we think few of our readers would form an idea from it of the extended and systematic manner in which all subjects purely medico-legal, and others relating to medical police, are treated. As it is a work which may not readily find its way into the hands of English practitioners, and one that relates to a very important, although, in this country, a still much neglected branch of

medical education, we shall consider it our duty to furnish the readers of this Journal with a full review of the author's opinions on subjects that have any practical bearing. That the work is adapted to Italian jurisprudence will be no obstacle to this course; since, as the Professor judiciously remarks in his preface, "customs and laws may change, but facts are immutable:" so we say of medical jurisprudence, that the laws of nations may vary, but the well-established doctrines of the science will be the same in every civilized country. Thus the medical proofs of the crimes of infanticide and poisoning are the same in Italy as in England, although the laws of the two countries may define the crimes differently; and one may require a greater amount of evidence to establish legal guilt than the other.

No particular arrangement has been followed in treating the subjects, a circumstance which will not excite surprise, when it is considered that medico-legal questions may be viewed under very different aspects, according to circumstances; and that there is scarcely a case in which we do not find questions put to witnesses, that relate to subjects having no connexion whatever with each other. It is only then in the broad outlines of the science that a medical jurist can pretend to classify what falls before him; if he attempt more than this, he will, like many writers of the German school, sacrifice perspicuity to method. In the first volume, among many subordinate matters, the following subjects are examined: pregnancy, abortion, infanticide, and insanity; the second is entirely devoted to toxicology; and the third is equally divided between the subjects of wounds and medical police in relation to contagion and quarantine.

1. We pass over the author's remarks on age and sex, merely observing that they present all that is now known on those subjects. The questions relative to CRIMINAL ABORTION then offer themselves; and here we find a full exposition of the views of modern medical jurists. The causes of abortion and its effects on the mother and child are well described. In relation to the practice of inducing premature delivery, the author observes:

"Another question here presents itself; it is this: whether abortion may be lawfully brought on in medical practice, to prevent the risk which a female with a deformed pelvis may incur, by allowing her to go on to the full period? There is no question in legal medicine which, in a humane, moral, or legal point of view, is of greater importance than this, none which more closely concerns the honour of medical practitioners. But, however difficult this question may at first sight appear, from its being doubtful whether the mother should be saved to the exclusion of her child, the difficulty is not lessened when we consider that we have no means of determining at what period of gestation the child will have reached a stage of development, beyond which it cannot pass without endangering the life of the female. It matters not whether the pelvimeter has been applied to determine the diameters of the openings; because, as it is impossible to measure the head of the child, the application of this instrument can lead to no useful result, not to mention that there are other objections to its employment. Again, it can avail nothing to calculate from the period of pregnancy the degree of development in a child's head, and apply the inference to a particular case, since the heads of children are not of equal size at the same period: some are large, others are small, and, while in some the fontanelles are expanded, in others they are contracted. Moreover, even admitting that the narrowness of the outlets in a pregnant female, determined by the pelvimeter, were such that she could not give birth to a moderately-developed child without danger to the lives of both, I am totally opposed to the practice of inducing pre-

mature delivery, which professes to save the life of the mother and sacrifice that of the child. The reasons for my opposition to this practice are as follow: By its adoption, while there would be the almost certain loss of the life of the child, there is no certainty of saving the female, knowing, as we do well by experience, that abortion, maliciously induced by any means whatever, has always more or less compromised the life of a woman; hence, even if the practice were not deemed inhuman, it would be unjustifiable; because it cannot ensure the life of that one of the two beings for whose sake it would be employed. Moreover, if the deformity of the pelvis be known to the parents or professional men, why has the marriage been permitted to take place? Why should children be begotten when they can only be allowed to reach a certain stage of development, and are not likely to survive their birth above a few seconds? But, since the Cæsarean section or symphysotomy is more certainly adapted to save the lives of both when the child is mature, than the use of abortives when the child is immature, so it follows that while the adoption of these means is lawful, the use of abortives for the purpose of exciting premature delivery is unlawful and criminal." (Vol. i. p. 115.)

All who are engaged in obstetric practice must regard this as a momentous question; and we have therefore thought it right to state the author's opinions in his own words. There are two circumstances which, it appears to us, a medical man has to take into consideration before he engages in this practice, and these are embodied in the objections of the author. 1. Is it impossible that the woman can be delivered in a natural way at the full period? 2. Is it not very probable that she herself may fall a victim to the means used to procure premature delivery? In regard to the first point, the Professor truly observes, that no measurement can reach the child's head, that the size of the head is subject to great variation; and hence, although we may have evidence of the deformity of the pelvis, we are not in a condition to say that delivery in a natural way would be impossible; since the head may be smaller than the average, and the effects of nature may accommodate it to the contracted dimensions of the pelvis. This is no speculative view: we think it must be within the experience of most men who have been extensively engaged in obstetric practice, to have met with cases in which, in spite of great deformity, delivery has taken place at the full period, with safety to the mother if not to the child. We do not say that this has been without great risk to the mother; but it must be remembered, as a matter of responsibility, that it is better this risk should arise in the course of nature, than that it should be brought on by the means of art. In a remarkable case which occurred to Dr. Lilburn, reported lately in a contemporary journal,* we have an instance of what the efforts of nature will sometimes accomplish. A woman, labouring under great deformity of the pelvis, was twice delivered in safety and the child survived. In a very large number of cases, we think the question here proposed could not be answered from medical data, for we should have a difficulty in saying that it was *impossible* for the woman to be delivered at the full period. But, admitting that there are some cases in which the deformity is palpably great, and, at the same time, every allowance is made for smallness of the child's head and the efforts of nature, it may become a question whether premature expulsion should be resorted to. If it were simply a question whether the life of the mother should be saved to the exclusion of that of the child, we see not the least ground

* Med. Gazette. Vol. ix., p. 933.

for hesitation: but this is not exactly the position in which we are placed. Most experienced accoucheurs admit with the author, that abortion, however brought on, is attended with great risk to the female; and that, if weak and delicate, she may, in spite of the best care, easily sink under its effects. The saving of the life of the child cannot be generally looked to, and, indeed, it commonly happens that the child is still-born. Thus, then, the question may be said to be limited to the probable consequences of the process on the life of the woman. It is in order to save her, or to give her a greater chance of safety, that the operation is undertaken: the practice would never be justifiable, either legally or morally, for the mere purpose of saving the life of the child. Barzellotti rests his views upon the results of those cases in which abortives have been *criminally* administered to pregnant females: but the supporters of the practice would scarcely deem this fair; since it is well known that poisonous substances are commonly given in large doses by ignorant persons, and the death of the female, with or without the expulsion of a dead child, is the almost certain consequence. In this country, we believe, it is the custom of practitioners to excite premature delivery by the exhibition of regulated doses of secale, and by rupturing the membranes. Although this may lessen the danger to which the female is exposed, yet we think most practitioners would say with our author, that the operation was attended with considerable risk. It may at first sight appear to be justifiable on the same grounds as a severe surgical operation; but the comparison can scarcely be admitted; since, when a surgical operation is performed, it becomes justifiable by its being rendered absolutely necessary, and the danger to life being imminent. Can it be said that this is the case with a female, the deformity of whose pelvis, on which the whole question of necessity rests, cannot be accurately determined, who is only half advanced through pregnancy, and whose health at the time may not be in the least affected by her condition? But in cases of considerable deformity, in which alone it appears to us, if at all, the operation would be justifiable, at what period is the practitioner to perform it? If he perform it at the earliest stage at which pregnancy is discovered, he certainly exposes the life of the woman to great danger, because it is only by a most violent shock to the system that the undeveloped embryo can be separated: if, in the latter stage,—his attempts may be fruitless, since the head may have already passed beyond that degree of development which would have enabled it to traverse the outlet; and he is thus placing his patient in a worse condition than if the course of nature had been allowed to continue. If a fatal result follow, a not unlikely circumstance, the death of the female would be ascribed to his interference. Baudelocque and Capuron, two most eminent obstetric practitioners, have condemned this practice of exciting premature labour as an outrage against the laws of God and man. This, perhaps, is going to an extreme; the destruction of a child at birth by craniotomy is not recognized by divine or human laws; but every accoucheur knows that it must be occasionally resorted to, and that without it, not merely the child but the mother must perish. When the practice is undertaken on pure motives and with a view to save one life at least, we cannot ascribe immorality to the act, however we may condemn the conduct of the practitioner as indiscreet and imprudent. But what says the law of

England on this subject? The late statute, 1 Victoria, c. 85, s. 6, does not make any exception in favour of medical practitioners; it leaves the question entirely open. To render a person guilty under it, it is necessary that the means to procure "miscarriage" should have been *unlawfully* applied or used: but what constitutes an *unlawful* application or use is not defined. A medical man, if charged with the crime, upon the fact of his having made the attempt on a pregnant female, would be required to establish the purity of his motives, and to justify his conduct by a reference to general medical practice. In the first point he might succeed, but in the second, his success, if we are to be guided by the published opinions of great authorities, would be somewhat doubtful. His conduct might not be deemed illegal, but censured as indiscreet. On the other hand, if the female were to die under the process, he might find himself in a more serious position. The justification of his conduct must be more clearly made out than we think in most cases it could be, or, otherwise, a verdict of manslaughter might be returned against him. Thus, then, under all circumstances, the practice of exciting premature labour is not merely hazardous to the life of the female, but, whether she live or die from the attempt, it may seriously involve the reputation of a practitioner. Nor is it any answer to this view that it is often adopted with impunity to the woman and without the knowledge of the attempt becoming known; for the imprudence and illegality of an action are not affected by the occurrence of a few successful instances, or by the secrecy with which it is performed.

Our author dwells on the prevention of marriage by parents when the pelvis is known to be deformed, and on the nonprocreation of children among parties who are aware of this condition; but while pregnancy may occur in those who are unmarried, and a deformed female, who is exposed to the chances of pregnancy, does not speculate on the anatomical relations of her pelvis, we are called upon to meet the case of a woman so situated, and not to say how, when the difficulty has occurred, its occurrence might have been prevented. He suggests symphysotomy or the Cæsarean section as lawful substitutes for the practice which he holds to be unlawful and criminal, observing that the adoption of either of these at the full period is calculated to save the lives of both mother and child. Symphysotomy has, we believe, met with but little favour from English practitioners; they have preferred craniotomy; and it appears to us the last would be in most cases required, even where the first had been performed: hence little could be gained by its adoption. As to the Cæsarean section, it has been almost uniformly unsuccessful in England, chiefly because it was performed at too late a period. English practitioners look to this, rather as a means for saving the child than the mother. It is at all events a formidable operation to undertake upon a living healthy woman, and one which, in its results, is likely to be attended with as much responsibility on the part of the practitioner as the exciting of premature labour. The experience of Englishmen does not warrant us in adopting Barzellotti's view with regard to the Cæsarean section, namely, that it is more certainly adapted to save the lives of mother and child than the practice which he condemns. To us it appears the operation places the mother in equal if not in greater danger, while it rarely offers a greater chance of preserving the child.

It would seem that manual means, by which we are to understand surgical or obstetric operations, are not often criminally resorted to in Italy for the purpose of procuring abortion.

"Pharmaceutical means are sought for, and great art is shown by the guilty person in endeavouring to render a medical practitioner an unconscious accessary to the crime. Thus, pregnant females or their accomplices will invent some story to induce the practitioner to bleed, to apply leeches, to administer an emetic or drastic purgatives, at the same time, perhaps, concealing from him their real condition. Sometimes, when they are not successful in their application, they will resort to these means of their own accord, or will take savine or other substances popularly regarded as abortives. Notwithstanding these attempts, gestation will sometimes continue; but if it should happen that abortion followed, then the case may be treated as criminal; and an important medico-legal question may arise, as to whether the abortion was really caused by the means employed. This question will require some consideration, since, as we know that the simple means above mentioned are often used by pregnant women without any mischief following, how are we to judge in a particular case that the expulsion was really due to their employment? Much circumspection is here demanded of the witness, since, without it, he will either aggravate the case against the guilty person or lead to an acquittal. The judgment of the witness must be based on numerous circumstances, as on the time at which abortion occurred; on the condition of the female; her mode of life, her constitution, her moral conduct; and on the means actually employed by her, if known. An acquaintance with these circumstances becomes necessary to enable him to say whether, in his opinion, the abortion was or was not brought about by criminal causes." (Vol. i. p. 117.)

These remarks are judicious, and convey an important lesson; for although a substance may have been administered with intent to procure abortion, and abortion have followed its use, it is not a necessary consequence that the expulsion should have been caused by it. This is a conclusion at which, for want of proper reflection, we might hastily arrive, through the proneness of the human mind to connect an effect with some immediately antecedent condition, which may be, however, only apparently the cause. This error of judgment will be avoided by attending to the advice of the author. It is singular that in our own statute law it does not seem to have been contemplated, that any woman would attempt the crime on herself, for the words obviously refer to the attempt made by others. This, indeed, is what we generally find. There are few women who, without the assistance of an accomplice, have sufficient knowledge to make the attempt.

In remarking on the difficulty of determining whether abortion has taken place at the early stages of gestation, more especially where the embryo or fœtus has not been found, the analysis of the sanguineous discharge will serve to throw some light on the question. Thus the lochia may be confounded with the menstrual secretion; but, in the author's view, they may be distinguished by the fact that the lochial discharge contains a large quantity of fibrine, while the menstrual secretion contains but little. We cannot say that we consider this would be admissible as a proof in an English court of law.

But, supposing that we have the best evidence of abortion having taken place in a particular female, how are we to determine whether it arose from a natural or a violent cause? The last condition must be positively established before legal guilt can be fixed upon a party: the question will not be allowed to remain as a mere matter of suspicion or inference from circumstances. The answer will be made up by a reference to

certain facts which are within the province of a jury and others which are purely medical.

"The fœtus may be found buried or concealed; the female may have taken violent medicines, such as drastic purgatives or emetics, without the advice of a practitioner: she may have applied leeches to her feet, have abstained from food, or have employed mechanical means for the purpose of rupturing the membranes and discharging the waters and fœtus. On the contrary, suspicion would be removed, and the influence of natural causes rendered probable, if the woman were weak and of a languid constitution; if she had suffered from previous disease or violent emotions, arising from the loss of friends or fortune; if she had been subject to copious discharges of blood from the uterus or other organs, to diarrhœa or dysentery; if she had experienced a fall, or had taken violent exercise: any of these moral and physical circumstances are well known to favour the separation of the fœtus from the womb. The proof will be rendered still more satisfactory if she have not taken any precautions to conceal her pregnancy or delivery." (Vol. i. p. 120.)

When the dead fœtus is found and the female is unknown, the only medical grounds for her discovery and identification will rest upon the opinion formed as to the time which the fœtus may have survived its birth, and the period during which it has been exposed. The degree of its development and the advance of putrefaction will here form the data for a medical opinion. If we can say how long a period has elapsed since the expulsion of the child, circumstantial evidence may lead to the discovery of the mother; but to establish the case against her, something more than medical evidence will be required. There must be such a correspondence of time and circumstances as to leave no doubt in the minds of the jury, not only that she has been delivered of a child, but that she was delivered of this particular child. If many days have elapsed, the obtaining of medical proofs by an examination of her person will be hopeless. Abortion may be feigned for the purpose of extorting money, but a careful examination would soon enable a practitioner to detect the imposition.

"So it will happen that a mass of blood, a mole, or a group of hydatids may be mistaken for an aborted fœtus; but if the female be married and live in good society, there will be no difficulty offered to its production. Objections are always made when there has been illicit intercourse, more especially if the cause of abortion have been violent and criminal. In conclusion, I must caution medical witnesses that they ought not to allow the cause of justice and humanity to suffer, merely because the criminal or accomplice happens to be a member of their own profession." (Vol. i. p. 122.)

From the satisfactory manner in which this subject is treated, we are a little surprised that two cases, in which medical evidence may be material, have escaped the author's notice. Thus, a female may imagine that she is pregnant, and be so reputed by the world, when she is actually labouring under ovarian dropsy or some other abdominal or uterine disease. This impression will especially exist and continue for many months, when the female, if unmarried, has exposed herself to the chance of pregnancy. It is easy to conceive that abortion might be attempted in such a case as this; and the crime is complete under the statute, although the means employed may not have succeeded in producing expulsion. The accused party may be placed on trial, and, supposing that the medical witness is able to prove either that the female is not pregnant, or was not pregnant at the time the attempt was made—what would be the result? It is reasonable to presume that the charge would fall to

the ground, for there would be, as the law terms it, no *corpus delicti.* It is singular that the statute makes no mention of the attempt being made on a *pregnant* woman; it merely says "to procure the miscarriage of *any woman:*" but two cases were tried in 1838, in which the judges respectively decided that pregnancy in the female at the time of the attempt must be proved, or the charge could not be sustained. One of these cases was that of a medical man, charged with having attempted abortion by the performance of an operation; he was acquitted, because the female denied her pregnancy, and there was no positive evidence of the fact.

But another and more difficult case presents itself, where, with almost every sign of pregnancy, the woman is not pregnant, as where the uterus contains a mole or a group of hydatids. Abortion may be attempted here, but the woman may recover from the effects of the means employed. If the contents of the uterus be expelled, their production would at once announce the nature of the case; but if they should not be expelled, then a most difficult question will occur; for a certain diagnosis could not perhaps be made, and it is not improbable that the medical witness would depose to the female having been pregnant at the time. But in the event of a mole or hydatids, or a mass of blood having been expelled instead of a fœtus, is the proof of this an answer to the charge? If the words of the statute were taken strictly in a popular sense, the term "miscarriage" would apply to all of these cases. It remains to be seen, however, whether our judges would or would not restrict its legal signification to the expulsion of a *human fœtus.* In a case of non-expulsion, where a mole or hydatids exist, from the great difficulty of diagnosis, it is most likely a criminal would be proceeded against and punished just as if the woman were really pregnant, unless, by the lapse of time before trial, the presumption of pregnancy had been refuted.

2. The question of LEGITIMACY, which is next examined, loses much of its interest, when we consider how little our courts of law are guided by the opinions of medical witnesses on the subject of gestation. It is now admitted that pregnancy may be protracted beyond the average period in some females, and that there are few in whom the period is exactly the same. So again in contested cases, courts of law have been known to declare some children illegitimate that had been born at the average period, and others legitimate which had been born considerably beyond it. This is as it should be. Whatever medical opinions may be offered, and these we know are sufficiently conflicting, owing to each witness being commonly ruled by his own experience and rejecting that of his fellow-witnesses, the court always places the greatest reliance on moral proofs. It is, indeed, difficult to conceive a case in which if a witness were justified, from medical data, in pronouncing for illegitimacy, as where a child is said to have been born at the fourteenth or fifteenth month after intercourse between its reputed parents, there would not be, at the same time, an abundance of moral proofs to establish the fact conclusively, without the assistance of a single medical opinion.

3. In a country where the law of primogeniture prevails, the determination of the order of births, in a case where a woman is delivered of twins or triplets, becomes a matter of importance in relation to the civil rights of children. By the Roman law this is not necessary, since all children, born of the same father, have equal rights of succession;

when, however, property is specially bequeathed to the eldest child, the same necessity exists for determining these cases, as where the law of primogeniture is in force. (p. 138.) This is a question which can rarely present itself, and we think when it does occur, the court would obtain the necessary evidence rather from parents and nurses than from medical witnesses. Prof. Barzellotti admits the doctrine of superfœtation, although we find no new cases in support of it; those which he adduces are well known to the profession, being quoted from the works of Orfila, Baudelocque, and Foderé. It appears to us that the author, in treating of this question, has confounded two conditions which ought to be held distinct; namely, of one conception following another either immediately or at an interval of some months. Those who are disposed to admit the first, that two conceptions may take place at or about the same time, may not feel disposed to believe the second, considering the well-known changes which take place in the uterus when the fœtus has reached the second, third, or fourth month of its development. Many instances have been reported in support of the last view, but to us the evidence does not appear satisfactory. In admitting the facts stated by the author and those whose opinions he adopts, we differ from him, in thinking that they allow of another explanation more probable and more consistent with the laws of the pregnant state than that assigned by the advocates of superfœtation. The only possible legal case where a question of this kind could be entertained in this country, is where the children, alleged to be of the same father, are born at different periods, we will say at three months' interval, and are equally mature and developed, as for instance, where a widow gives birth to two children after the death of her husband. These have in general been regarded, and very properly, as twin cases, in which one child was born before the other. The statement of the children having been equally mature at birth is not admissible, because it has rested on mere hearsay or ocular inspection, instead of being founded on the weight, length, and other characters of development. The author, however, maintains that one of the proofs of superfœtation is, that children may be born at the same time, presenting very different degrees of development, one will be large and robust, the other small and weak (p. 141); therefore, he argues, one must have been conceived after the other had become developed. We certainly are not in a condition to say at what particular period after the first impregnation, the power of conception ceases in a woman; but we should consider it a very unlikely occurrence after the first month; and we think that cases which are set down as superfœtations within this period are equally well explained by supposing that the children were really twins and one was developed more than the other. A bilocular uterus was supposed to be favorable to this condition, and the author advances the opinion, but there are some difficulties to its admission. A woman died after delivery, and on inspection, a bilocular uterus was found; but impregnation had only taken place in one cornu:* in another instance, a woman, in whom superfœtation was believed to have existed, died, and on dissection, the uterus was found in its normal state.† The practical application of this doctrine in jurisprudence is limited to those cases only in which moral and medical proofs would easily solve the question.

* Med. Gazette, Vol. xix., p. 507. † Devergie, Vol. i., p. 469.

Such a case never has occurred, and does not appear likely to occur. The delivery of a woman of two *equally mature* children at intervals of two or three months is contrary to general experience. An artful woman might make use of the doctrine to substitute one child for another, as a male for a female, or a living for a dead child; but this is supposing her to be profoundly versed in the difficulties of legal medicine. The occurrence of such a case must be generally considered too improbable for women in ordinary life ever to have recourse to a sort of imposition which would be so easy of detection.

4. The chapter relative to the RIGHT OF SUCCESSION in new-born children is full of important matter; and we are pleased to find that the author takes a much more liberal and enlarged view of this subject than the medical jurists of France and Germany. He does not examine the question of "what constitutes birth," but in England this may give rise to grave enquiry. The practice of our law is not to consider a child as born, in civil or criminal cases, until the *whole* of its body is in the world. The establishment of respiration before birth amounts to nothing; the child must be entirely born and live *after birth* to entitle it to the rights of inheritance. On the other hand, to render proof of life after birth admissible in law, it is not necessary that the child should have breathed, its legal survivorship may be established by the evidence of other vital acts. On the continent, in general, before a child can take property by descent or bequest, it must not only be proved to have been born alive, but it must be proved to possess *viability*. Foreign jurists attach different meanings to this word; but the general interpretation seems to be that the child should have all its organs and functions perfect, and that there be no vice of conformation about it likely to lead to a speedy termination of life. According to some it must also be born at a certain period of gestation; its body must be of a certain length and weight, and present certain marks of development, or else its rights of inheritance will be disputed, and probably set aside in law. We may well ask what relation have all these accidents with the natural claim which every child has to inherit the property of its parents? We can only reply that they give rise to abundance of litigation and to the display of much legal and medical ingenuity, but the common-sense view of the subject is lost sight of in the purest fictions and technicalities. The Professor justly remarks, that the proofs of the act of life after birth, and of viability or the capacity to live, should be considered as perfectly distinct. (p. 178.) He very properly regards the division of life into uterine and extrauterine, or vegetative and animal, as absurd, if it be taken from what it properly belongs to, namely, speculative physiology, and applied to the practice of jurisprudence. There is no doubt of the fœtus possessing life while in the womb, and if at its entrance into the world any of its uterine functions be still carried on, this is satisfactory evidence of its being alive. Thus, "the persistence of the circulation after birth, as evidenced by the motions of the heart and arteries, ought to leave the question of the child being living undisputed; and we might even say that it possessed a greater share of vitality than it did in the uterus, because it can now perform certain actions which it could not while within that organ." (p. 179.) "Even where all pulsation has ceased and there is no sign of life, its death may be only apparent, for it may still under proper care be resuscitated." (p. 180.) The question

of life being determined, the author passes to the examination of viability. Into this we shall not enter, since it is an exposition of the French doctrines on which we have already commented. Barzellotti contends that the establishment of respiration should not be deemed a necessary proof of viability, for, he observes, the lungs may be well formed, and the non-establishment of the process be due to some simple accident. The proof of circulation should be sufficient, without its usual concomitant respiration; and he very properly asks, "why should well-organized and living children be declared not *viable*, and deprived of their civil rights, because they happen to be in a state of apoplexy or asphyxia, when others (adults), affected with apoplexy or asphyxia, have not their rights denied them." (p. 183.) That courts should come to contradictory decisions, while such absurd points are insisted on as matters of proof, cannot excite surprise. Two examples are quoted from Zacchias, illustrative of this uncertainty of the law.

"A woman died during labour, and the child was extracted by the Cæsarean section. It moved its arms, legs, and mouth, so that those present had no doubt of its being alive. A division of opinion, however, arose, on the matter becoming litigated, whether this child was *viable*, i. e. endowed with capacity to live. Zacchias strongly maintained that it was not, because it had not come into the world in a natural way, but had been removed from its mother's womb by an operation. The Roman court decided in accordance with this opinion; but it is easy to perceive that their decision was based upon defects connected with the mother, not with the child, and that more weight was attached to the manner in which it came into the world than to the signs of life which it had really manifested." (p. 185.)

In another case, however, somewhat similar in its details, Zacchias gave an opinion in favour of the viability of a child, because it had reached maturity and had manifested *signs of life* after birth. The court again decided according to this view, and, therefore, in opposition to its former judgment.

The author next refers to the remarkable case of Fish v. Palmer, which was tried in our Court of Exchequer in 1806, not in 1796, as stated by him. Here the question was, whether the motion of the lips of a child after birth should be deemed evidence of *life*. Drs. Babington and Haighton gave an opinion in the affirmative, Dr. Denman in the negative. The jury decided that the evidence was sufficient to prove life, upon the same principle that dead muscle cannot spontaneously contract. Foderé condemned this decision, but our author upholds it with irrefragable arguments, although we think it requires but little reflection to perceive that the verdict was founded on the strictest equity. He is, however, in error when he says (p. 187) that the verdict declared the child to be *viable*, for the law of England knows nothing about the *viability* of children. Their capacity of continuing to live after birth never becomes a question. All that is required to be proved is that a child was not dead when born; and, as the above decision shows us, the least action, indicative of vitality, as the quivering of a lip, is sufficient to establish this point. Respiration, the act of crying, or the free motion of the limbs, will, *à fortiori*, be good evidence; but these actions are not indispensably required to be proved in order that a new-born child should take its civil rights. We gather from our author that the Italian law demands proof of viability besides evidence of life, although, at the same time, it construes the facts most leniently towards the child.

Thus it will not insist upon proof of all the functions of life being exercised by the offspring (p. 186); but we are entitled to ask, why should any proof beyond that of the possession of life after birth be required? The slightest reflection will show that the greatest possible injustice must be inflicted where proofs of viability are rigorously insisted upon; the opinions of medical witnesses cannot be founded on any fixed rules, and the decisions of courts of law under such legislation will always be arbitrary and often conflicting.

5. The chapter is closed by some remarks on the civil rights of MONSTERS. Continental codes of law are chiefly based, not so much on the degree of deformity in these beings as on their viability or capacity to live. Our English law is regulated by external form only: "a being to inherit must have *human* shape." Slight external defects, as a superabundance or deficiency of fingers or toes, do not debar from civil rights either in England or on the continent, still there is this remarkable difference between our laws and those of France and Italy:—according to the codes of the latter countries, monstrosity may be *internal* and cut off civil rights, as where there is a transposition or deficiency of important viscera, leading necessarily, and sooner or later, to the death of the being: but in the law of England, so far as we can judge from the highest authorities on the subject, the want or misplacement of internal organs cannot take away the civil rights of a child provided it be born with life. The author does not appear to have met with the excellent remarks of Geoffroy St. Hilaire on Teratology in relation to Medical Jurisprudence. He thinks, however, that both disomatous and dicephalous monsters should be allowed the rights enjoyed by other beings, and he adduces the case of Christina Ritta to show that they ought to be deemed viable, this compound monster having lived about eight months. He does not mention the Siamese twins, probably the most remarkable disomatous monster ever known, and one well calculated to baffle all legislation on the rights and responsibility of such beings. A case like that of the Siamese twins might give rise to a question more serious than one relating to the right of succession, as when, for instance, murder is committed by one half of the monster, without the criminal participation of the other. St. Hilaire quotes a case of this kind, which actually occurred in Paris, in the seventeenth century. A disomatous monster killed a man by stabbing him with a knife. The being was condemned to death, but was not executed, on account of the innocence of one of its component halves.*

6. We now pass to the questions connected with INFANTICIDE. The practitioner is first directed to determine the age and probable period of death of a child found dead, and particularly to attend to the phenomena of uterine putrefaction. The moment we have discovered that uterine putrefaction has taken place in the body of a new-born child, we do away with the charge of infanticide. The signs of putrefaction in utero are chiefly extracted from Orfila. In the Fourth Number of this Journal our readers will find a notice of the valuable remarks of Devergie on this subject. These comprise all that is known respecting it, and having so recently given a full exposition of them, it is unnecessary for us to quote those contained in the work before us. Respiration, in the

* Annales d'Hygiène. 1837. Vol. i. p. 431.

opinion of the author, is the chief character of extra-uterine life, an opinion in which we fully coincide, with the understanding that the function may have been performed and the child not have been born living; as also, that a child may have lived after birth, in some instances many hours, without leaving any traces of the performance of that process in the lungs. The test of Ploucquet, founded on the relative weight of the lungs to the body before and after respiration, is first examined; but the author places no reliance on it, chiefly from the conflicting results obtained by Schmitt and Chaussier. Although we think this test is not of much service in a practical view to the medical jurist, yet we are not led to this opinion by the statements of the writers on whom the author relies. When we know that to obtain these results they experimented on the lungs of children born at various periods of gestation and of both sexes, we can easily account for the wide difference between their ratios and those of Ploucquet. Experience clearly shows that the ratio is different according to the degree of maturity of the child and the period of gestation at which it is born.

The author seems inclined to place great reliance upon the complicated process, suggested by Professor Bernt, of Vienna, for solving the question of respiration in a new-born child found dead. The principles of Bernt's process are minutely described. This medical jurist takes the length of the body, the weight of the lungs and heart together, the weight of the lungs alone, as well as the specific gravity of these organs. Averages are first obtained by experimenting in this way on children of the two sexes and at different ages, which have respired perfectly, imperfectly, or have died without respiring, These results, in an unknown case, may, it is alleged, be employed as standards of comparison. The only objection to this process is, that if it were possible of application in the hands of practitioners, it would be inadmissible as evidence in a court of law. No accused party would be convicted upon mere averages, for how are we to be certain that we have obtained correct averages? The rules of law imperatively require that certainty and not probability should be adduced to establish a particular fact. When this certainty cannot be obtained, the court will prefer leaving the point undecided rather than incur the risk of deciding incorrectly. As a medical question, we would ask what relation can be suspected to exist between the *length* of the body of a child and the absolute *weight* of its lungs? Experience, so far as we are aware, does not show that there is any connexion between these conditions, and it would be very singular if there were: yet the author seems to admit, although not from any observations of his own, that there is a close relation between them. (p. 259.) Orfila showed that Bernt's conclusions were not supported by his own (Bernt's) experiments: but Barzellotti objects to Orfila's criticism, that he did not include the length of the bodies in the cases to which he refers. For reasons already stated, we are inclined to adopt Orfila's view of the insufficiency and inadequacy of the process suggested by the learned Viennese Professor.

The hydrostatic test is somewhat briefly disposed of. We find here introduced the novelty of connecting the weight of the lungs with the length of the child's body and comparing it with a standard, prior to drawing any inference from the sinking or floating of the organs in

water. Thus, he observes, "if the lungs float without having an absolute weight equal to that of the lungs of children of *the same length*, which have respired, or which, by immersion, have not displaced so much water as these, there is reason to believe, either that they belonged to children which have died in utero, or that the air they contained was due to artificial inflation, putrefaction, or emphysema." (p. 273.)

The author has here departed from the usual practice. Most medical jurists look to the age or degree of maturity of the child before they draw an inference from the application of the hydrostatic test; but the degree of maturity does not depend upon the length of the child's body alone. This is only one of many characters, and consequently error is more likely to arise in this case than when all are taken. The following is a singular and to us a novel application of the test:

"Supposing the lungs to be absolutely heavier than the length of the body would indicate, and, at the same time, specifically heavier than water, while they are free from disease, so much air should be gently blown into them, by an opening made in the trachea, as the organs will conveniently admit. In the case here imagined, the lungs would weigh more than an ounce (the average before respiration), and would fall to the bottom of water. After this operation, the absolute weight need not be again taken, since it will not be altered by the introduction of air. Let the lungs thus inflated be placed in water, and let it be observed, whether the inflation has rendered them buoyant. Now, removing them from the vessel, so much blood, drawn from a living animal, should be injected into them, through the pulmonary artery, as they will receive; in the absence of blood, milk may be used. The pulmonary artery being again secured by a ligature, the lungs are to be accurately weighed and the difference between their present and former weight noted. They must then be put into water, and the quantity of water displaced by them measured. Let the results, thus obtained, be compared with those derived from the lungs of children that have been born, after having breathed, ("di feti nati dopo di aver respirato;") and if, after the inflation and injection, the condition of the lungs be the same in the two cases,—we are justified in asserting that the organs experimented on, had not respired (after birth); for by this process we place them under the same circumstances as those with which they are compared. A fraud may be in this way detected: thus, if lungs, which originally contained air, have been so compressed as that their buoyancy is destroyed, it may be discovered by inflation and injection; for the organs thus treated will then admit more air and more blood than those which have not really respired. The experiment is also applicable to cases in which the lungs are diseased." (Vol. i. p. 273.)

The Professor seems to be well aware of the fallacies to which this plan of determining the question of respiration is exposed. In admitting its ingenuity, we doubt how far it can be safely applied in practice, or even, if it were safely applicable, how far it would add to the utility of the processes already employed. The principle on which it rests is that the fœtal lungs cannot become so readily penetrated by air, and liquids resembling blood as those which have actually respired. But by what standard is the examiner to regulate the force of inflation and injection? If the fœtal lungs of two children were given to two experimentalists, it is quite possible, according to the force employed, that in one case the lungs would be brought to resemble those which had respired, and in the other, those which had not. We know, from actual experiment, that the fœtal lungs more readily receive air by inflation in some cases than in others, and this may easily occur in the injection of liquids, for the lungs of the fœtus contain variable quantities of blood, and, therefore,

will admit variable proportions of injected liquid, without reference to the influence of the varying degree of force employed in the injection. If occurrences of this kind are only *possible*, how is such a test to be made practically available in legal medicine? Besides, we do not see that even were it otherwise, any advantage would be gained by resorting to it. The absolute weight of the lungs, coupled with their healthy appearance and the fact of their sinking in water, furnishes just as much evidence as could be obtained by adopting the processes here suggested. In so far as they appear to us exposed to error, they lead to one of a dangerous nature, namely, that of causing us to say that lungs may have breathed which have never received air by respiration.

In endeavouring to solve this question, we are further advised to notice if the thorax be flattened or not; also the position of the diaphragm, and the state of the abdominal viscera. These facts will enable us to decide whether the child had died during birth. "If a child has breathed, but dies before it is born, owing to narrowness of the outlets, this ought to be indicated by marks of violence on the body: if these should be wanting, there will be no other evidence of the fact." (p. 276.) We think that scarcely sufficient stress has been laid, in the examination of this question, on the difference between the evidence of life and of live birth in the child. All the proofs adduced go simply to show that a child has *breathed*, but not that it has been *born alive;* and a medical jurist ought always to treat these as matters essentially distinct. We are not informed whether the Italian law includes under the head of infanticide those cases in which a living child is destroyed during the act of birth. By the English law, this is not treated as child murder; our judges invariably holding, when the question is raised, that to constitute the crime, the whole of the body of the child must be in the world when the murderous violence is offered. We have already recorded our opinion that the English law is, in this respect, impolitic and bad.

We shall not follow our author in his account of the causes of death of the child, nor into the question of presumed survivorship, where mother and child are found dead. These subjects are treated with the care which their importance demands.

7. We next arrive at the description of the various forms of death by ASPHYXIA, as drowning, hanging, &c. Syncope and asphyxia are considered to be synonymous terms; and either is employed by the author to signify apparent death, though he thinks the word syncope the more applicable. The sign upon which he relies most as evidence of drowning is the presence of a bloody mucous froth in the trachea. A remarkable case is related in this section, which shows that much caution is required in drawing an inference from the presence of marks of violence on the drowned; we here furnish a brief abstract of it.

"A prisoner, while being escorted by a party of soldiers along the banks of the Po, took an opportunity of throwing himself into the river, for the purpose of escaping. His arms were at the time bound together; it was alleged that he had perished by drowning. Thirty-three days after the accident, the body was discovered, and submitted to a close examination. Besides the ordinary signs of drowning in the respiratory organs, some appearances were seen on the neck, which led to the suspicion of strangulation. There was a livid circle extending completely around the neck, about a line and a half wide; and immediately below it another mark, lighter in

colour. The skin over the trachea was found to be ecchymosed. Blood was extravasated in the brain, and between its membranes. The presence of the marks, which some supposed to have originated from strangulation, was explained by the fact of the deceased having worn a thick coarse shirt, tightly buttoned round the neck. The water, it appears, caused by imbibition a retraction of the stuff, and thus made the shirt-collar act as a tight ligature." (p. 329.)

Barzellotti and Orfila gave it as their opinion that this was a case of drowning, and the persons who were charged with having strangled the deceased were set at liberty.

We knew of an instance where the body of a female who had perished by accidental drowning, off the coast of Norfolk, presented, as in the above case, a deep livid mark around the neck. This was found, by careful examination, to have been produced by the string of a cloak which the deceased wore at the time of the accident. She was not more than ten minutes in the water, and it was probable the mark had arisen from her struggling in one direction, while the tide was drifting her cloak in another. Supposing that the accident had not been witnessed; that the cloak had separated, and the body drifted on shore, what might have been the medical conclusion? It would perhaps have been said, how could a mark like this have resulted from accident?

We fully agree with the author that the question whether the drowning is suicidal or homicidal, is rather for the jury than the medical witness. It is not wonderful that a question of this kind should so seldom be raised in an English court of law, when we consider that coroners, to save time, trouble, and expense, generally direct the verdicts of juries from their own casual inspection of the body. They do not require medical evidence even to establish the real cause of death. A dead body having been found in the water is in general held to be a sufficient proof of death by drowning!

8. In treating of *hanging*, the author seems to be fully aware of the fallacies which may arise from trusting to the presence of a mark of the ligature on the neck, as evidence of vital hanging. Ecchymosis, even below the cord, cannot be trusted to as a proof of this, unless the effusion of blood extend beneath the skin and adjacent muscles. The subject of *suffocation* is shortly discussed. We were not aware before that the Emperor Tiberius fell a victim to the process of "burking," or that that form of murder was so ancient. (p. 335.) On the whole, the remarks on the subject of death from asphyxia, in its various forms, are practical and judicious.

9. The next chapter is entirely devoted to an exposition of the rules for conducting medico-legal inspections, and for drawing-up the necessary reports. The following observations plainly show that the customs of Italy and England are widely different in regard to the manner in which these enquiries are conducted. "In every case of a body found dead, whether known or unknown, whether of high or of low condition, a post-mortem examination is required by the court, as the foundation of all legal proceedings." Again: "There is not a single medico-legal case relating to the dead body, in which a post-mortem examination will not be of the highest service." (p. 339.) We have in these remarks the explanation why medical jurisprudence thrives more abroad than it does in England. It would, we think, be impossible to point out any other

country in which we have the spectacle of some men devoting their lives to the cultivation of a science, the principles of which they are not permitted to practise; while others, who have confessedly never attended to the subject, are taken at random to answer questions involving the life or liberty of an accused party. The rule followed by our coroners in the very few cases in which they now require a medical opinion on the cause of death, is to summon that individual of the profession who happens to live *near* to the spot where the dead body has been found. Whether the witness so summoned be a surgeon or a druggist—a qualified or an unqualified practitioner—a master or an apprentice—whether he have had an opportunity of seeing such cases or not, and, therefore, whether he be competent or not to express an opinion as to the cause of death in this particular instance, are matters into which the coroner does not deem it necessary to enquire. Our professional brethren who have had any experience of coroners' inquisitions will, we think, bear us out in the statement that this is no exaggerated picture. Nor is it any answer to this accusation that coroners do sometimes select well-educated and qualified practitioners as witnesses. Our complaint is against the general rule that is followed, not against these occasional exceptions.

It is said that a knowledge of the cause will suggest a remedy for an evil. The cause in this case is easily discovered: it consists, we firmly believe, in the very defective manner in which individuals are appointed to the office of coroner. Our continental brethren will, perhaps, be astonished to learn that individuals are elected to this office in a way somewhat similar to that in which members of parliament are chosen to represent counties.* No qualification beyond that which pecuniary or other such influence can command, is required. The candidate need not possess the least knowledge of medicine or medical jurisprudence; it is not even necessary that he should have ever seen a dead body. We do not by these remarks wish to imply that a complete medical education is essential to one filling this office, although the proceedings in the large majority of inquests show that a much greater knowledge of medicine than of law, is really required on the part of this officer. General belief, however, appears to be so far in favour of the reverse, that most coroners are men who have been educated exclusively to the law. It is a favorite position with many persons that a sound knowledge of law is indispensable to the exercise of the office; but that this is a mistake is clearly proved by the fact that there is scarcely an inquisition in the proceedings of which, if it were necessary, a clever barrister could not easily detect many technical defects, that would lead to their being quashed before a superior tribunal. The credit for legal accuracy, therefore, which the law-proceedings of coroners' courts obtain in the world is really due to there being so rarely a necessity for disturbing them. Again, it is plausibly stated that the coroner can always command the best medical evidence. Not to mention that this position assumes a man to be well fitted to receive and judge of the value of evidence (in relation to the cause of death) on subjects of which he is totally ignorant, it is completely set aside by the facts: 1, that the *legal* coroner now rarely summons a medical witness before him; 2, when he does depart from this rule, he takes no care to inform

* The very lowest and most ignorant class of freeholders, who are not entitled to vote for members of Parliament, can vote on the election of a coroner!

himself how far that witness is competent to give *evidence* of *opinion*. Let us establish these two points before proceeding further.

a. By the 6 and 7 William IV. c. 89, the power of summoning medical witnesses is given to a coroner or a majority of the jury, and the witnesses are directed to be remunerated for their attendance. This was a considerable improvement on the old plan, in which a compulsory summons to attend was issued, without any remuneration being granted for the duty performed. The statute, however, led to this effect—inquests were now conducted without medical evidence being required so frequently as formerly; the option of summoning a witness being left with the coroner and jury. In the year 1837, another act was passed (1 Victoria, c. 68,) in which the expenses of medical witnesses were directed to be paid, in the first instance, by the coroner, to be repaid to him quarterly, at the General Sessions. This has turned out to be a most unfortunate enactment, since it has rendered the receiving of medical evidence at inquests a temporary burden on the coroner. We cannot be surprised, therefore, that these functionaries should take every opportunity of dispensing with the attendance of medical witnesses and with a post-mortem examination. Is it necessary for us to say more than this? Is it not within the knowledge of our readers that a large number of inquests are annually held in this country in which medical evidence is not demanded? We have now before us an authentic document, in the form of a Quarter Sessions' Report, by a magistrate of one of our midland counties; this report was made in October, 1838. We learn from it that out of the total number of inquests for the quarter (127), medical witnesses were summoned in 21 cases only, and therefore about *once* in *six inquests*. One coroner, it appears, held 82 inquests, and dispensed with medical evidence in 79 of these! The report proceeds to say, that "due regard should be paid to the expense entailed upon the county, from requiring such evidence; and due caution against incurring that expense without adequate reason"!

But it may be said there are many cases in which medical evidence is not necessary. We cannot give our assent to this statement. The duty of a coroner is to examine into the *cause of death*: if this be so clear as not to require a medical opinion, we cannot see the necessity for holding an inquest at all. The correctness of our views would be at once tested in paying the coroner by a fixed salary, and not by a fee upon every inquest which he holds. On the present system medical evidence is often not called for where disinterested persons would allow it to be necessary. Some cases of a recent occurrence will show this: A girl died in a low lodging-house, under circumstances which led to a strong medical suspicion that she had been poisoned by opium. An inquest was held—no medical evidence was required, and a verdict of "Died by the visitation of God" was returned. A man was found dead on the bank of a river, with marks of bruises and other violence on his person. The inquest was disposed of without medical evidence or a post-mortem examination, the coroner stating to the jury that it was most probable that the deceased was drunk; that the injuries might be ascribed to a fall; and that he was probably accidentally drowned. About a year afterwards some persons were arrested on a charge of having murdered the deceased; but when the case was heard before a magistrate, no

material evidence could be brought against them, since the body had not been examined at the coroner's inquest; and the *real* cause of death was wholly unexplained. We might easily adduce many more such instances, but we think the occurrence of one case sufficient to show that the discretion now left to the coroner may be used to the direct impediment of the course of justice.

b. That witnesses, when summoned, are not selected, but taken without reference to their professional knowledge or experience, is a matter too well known to need illustration by examples. In some instances, where several witnesses have appeared, a coroner has actually balanced the numbers and decided by the numerical majority. Thus if four witnesses gave one opinion, and two another, a coroner has been guided by the first, because there was a majority of two in its favour; not considering that the evidence of one experienced man ought to be of more weight than that of ten who have had no experience on the subject. This proceeds from that officer being totally unacquainted with legal medicine; from his not knowing, for example, the difference in the symptoms produced by arsenic, opium, or carbonic acid; from his being unable to distinguish between a false and a true opinion, or between an inexperienced and an experienced witness. Let the case go before a higher tribunal for trial: the first question asked by the prisoner's counsel is, whether the medical witness has had any experience in similar cases; if not, which is most probable, an appeal is made to the jury, and sometimes with success, in the prisoner's favour. Now, we would ask, why, if such a question as this is allowed to be put in a superior court, should the summoning of witnesses before a coroner be a matter of pure accident? The witnesses at the inquest are commonly the witnesses at the trial; and if experienced men be required in the latter case, which the questions of prisoner's counsel seem to imply, why should they not be sought for by the coroner? This discretion left in the hands of the coroner becomes then a means of ultimately favouring the escape of the guilty.

Again: inquests are held unnecessarily, and the peace of private families is often intruded on in a most officious manner, more especially in cases of sudden death. Inquests are held in cases of apoplexy, where a little medical knowledge would inform the coroner that there could not be the remotest suspicion of poisoning: and thus we have constantly before us the empty verdict of "Died by the visitation of God," which leaves the enquiry exactly where it began. In other cases, from his want of knowledge, a coroner will set at defiance well-grounded medical opinions, and direct a verdict according to his own view of the medical circumstances. During the time of the cholera a coroner committed two persons for manslaughter, whereas all the medical witnesses, to the number of seven, deposed that the deceased had died of cholera. Before a superior tribunal the case was at once dismissed; but in the mean time the accused had been unjustly imprisoned, because the coroner could not distinguish between death from natural and violent causes, even where there were good opinions to guide him.

As inquests are at present conducted, a great source of knowledge is cut off from practitioners; there is, probably, not one in five hundred of the profession who has seen a drowned subject inspected. The reason is obvious: out of many hundred such cases occurring annually in or

about the metropolis, the enquiry is generally conducted without medical evidence, and not one drowned subject in a hundred is submitted to a post-mortem examination by the order of a coroner. Then, again, suicidal cases of poisoning by oxalic or hydrocyanic acid are very frequent; but the body is rarely, if ever, permitted to be examined. The inquest is held, and either no medical opinion is sought for, or no inspection is made. If a trial for murder by drowning, or poisoning by oxalic or prussic acid come before a superior court, the medical evidence is either slighted or rejected, because the witness has not seen *that* which the coroner does not permit him to see. We consider the onus to rest with the coroner; for it is in the power of this officer alone to cause every body to be inspected.

Our readers will now be prepared for our conclusion, which is, that medical jurisprudence is extensively *taught* in England, and but little *practised*. The Apothecaries' Company have done much by enforcing attention to this subject; and the College of Surgeons have tardily recognized the necessity for its forming a part of the education of an English surgeon. But, notwithstanding the regulations issued, there is an indifference to the study—a circumstance which cannot surprise us, when we consider: 1. That teaching is not accompanied by practical expositions: medicine, surgery, chemistry, botany, all have the means of illustration; medical jurisprudence has not. 2. Students, in taking out a diploma, are not examined on the subject. 3. Future medico-legal practice is purely a matter of accident, no preference being given to one who has really studied the science.

The second defect we are glad to see remedied by the regulations recently adopted by the University of London. Strict examinations are instituted, and a nominal attendance at lectures is not held sufficient. At the same time, we wish it were in the power of the University to obtain for teachers the means of illustrating their lectures practically. This is done in France, Germany, and Italy; and, as it is admitted by all to be the only true way of teaching a science, we do not see why our own country should remain an exception to the general example of Europe.

The means by which, in our opinion, this science may be raised to the rank it deserves to hold are as follows: 1. That coroners should be educated men, acquainted with the principles of law and medicine; it matters not from which profession they be taken, but, to prevent any imputation of interested motives, we think they should not practise either. It may be said this is insisting upon too much; the judges of our superior courts are not versed in medicine, and yet they are called upon to try cases, involving medical facts and opinions. Our answer to this is, if coroners possessed only one half of the medico-legal knowledge possessed by judges, these objections to their competency would not exist. We wish to see the words of the ancient writ "*de coronatore eligendo*" rigorously adhered to: "*quod talem eligi faciat qui melius et sciat, et velit, et possit, illi officio intendere.*" If freeholders of every grade are to have the election of these officers, let their choice be at least restricted to persons who are fully qualified for the office; and it appears to us that in the present day there ought to be some other qualification than that of a man having "lands enough to be made a knight." 2. The salary paid

to this functionary should not be regulated by the number of inquests which he holds. It is a premium upon their being held unnecessarily, a fact established by daily experience. We are glad to find ourselves supported by a high law authority in this view: Blackstone says (vol. i. 347), in relation to the coronership, "This office has been suffered to fall into disrepute, and get into low and indigent hands." Again: "For many years past they (coroners) have only desired to be chosen for the sake of their perquisites." (*Ib.*) 3. No inquest should be held without the evidence of a properly qualified and experienced medical practitioner being imperatively required; nor should a post-mortem examination be omitted. This rule ought to be adopted, if not as a matter of necessity, at least as one of sound policy, in order that good experience may be acquired by practitioners for occasions in which such experience is held to be essential to the due course of justice, namely, before the high tribunals of the land. This would not add to the expenses of the proceedings; for, so soon as the present improper mode of remunerating coroners was discontinued, fewer inquests would certainly be held; and those which were held, would be more carefully conducted.

We turn from this view of the state of medical jurisprudence in England to the work of our author; and we shall now proceed to examine his opinions on two other important subjects, Toxicology and Wounds.

10. The second volume is devoted exclusively to TOXICOLOGY, and the general history of poisoning, with the properties and distinguishing characters of each poison, are fully treated. Among the diseases of the stomach, which in their symptoms approximate to the general effects of irritant poison, perhaps scirrhous ulceration, leading to perforation, is the most striking and the most liable to mislead a practitioner. The following cases, given by the author, are interesting.

"A man, after a meal of dressed grain and new wine, was seized with a great desire to vomit; the abdomen became tense; there was the most violent pain in the stomach, and the skin was cold. There were great anxiety, impossibility to keep the recumbent posture, and difficulty of swallowing. The man died; and, on inspection, an aperture of about an inch in diameter was found near the lower orifice of the stomach, through which the contents of the organ had escaped into the abdomen. The margin of the aperture presented traces of scirrhous disease. The man had suffered from dyspepsia for some time previously. In another case a man, addicted to the use of spirituous liquors, was seized with the most severe pain in the abdomen and vomiting. A tumour appeared in the epigastric region, which evidently pulsated, and led the author to suspect that it was an aneurism of the abdominal aorta. The pain in the abdomen increased, but the tumour disappeared. The patient died; and, on inspection, there was no trace of aneurism, but the stomach was scirrhous and perforated." (Vol. ii. p. 52.)

These cases, which simulated poisoning, had their true nature explained by the long previous illness and by the post-mortem evidence, derived from the diseased state of the stomach. We agree that there are satisfactory means of diagnosis; but the author has omitted to state that a person suffering under such a disease of this organ may have poison administered to him, and die from its effects. A case of this kind occurred in the west of England lately, and created some difficulty in the medico-legal investigation. The discovery of arsenic in the stomach, with the symptoms produced by that poison, superadded to those under which the deceased had been already for some time suffering, served as the basis

for a satisfactory diagnosis; and the prisoner was convicted on this evidence. The symptoms of Asiatic cholera may be mistaken for those of irritant poisoning. Our author thinks that a good diagnosis may be obtained by an examination of the blood; but we should rather trust to the character of the evacuations, and coldness and blueness of the surface. The general characters of irritant poisoning are well laid down. We must, however, distinguish between the action of irritant and corrosive poisons; for all irritant poisons are not corrosive. Corrosive poisons are known by their symptoms being produced *immediately;* they chemically destroy the organic tissues on contact; they give rise to the most violent pain and vomiting, while the evidence of their action after death is plainly discoverable, from the mouth to the stomach, if the person have not long survived the effects. These poisons not merely inflame, but corrode and perforate; whereas simple irritants are not often attended with either condition. From the description of symptoms by another, a practitioner may at once know whether the poison was a corrosive or not. "One remarkable circumstance must not be forgotten: these corrosive poisons do not always act on or destroy the parts with which they come in contact." (p. 62.) This last circumstance, though of rare occurrence, well deserves to fix our attention. Sulphuric acid has been sometimes swallowed without corroding the stomach; a fact referred by some to the acid being in small quantity, and becoming enveloped in mucus, which seemed effectually to prevent its contact with the parietes of the organ. The rules for preserving the identity of suspected liquids, as well as for conducting the analysis in unknown cases, are carefully described; and the necessity for previously determining the purity of the tests employed, rigorously insisted on. Much trouble will be spared by enquiring into the manner in which the deceased died, and into the nature of the symptoms under which he suffered previous to death. Thus we should not commonly look for arsenic in a case in which there had been no symptoms of irritation; nor for opium where there had been violent vomiting and diarrhœa, and no coma. When the poison has not been swallowed, of course these observations cannot apply.

The irritant poisons are examined in succession, beginning with the most important, as mercury, arsenic, &c. Each poison is systematically treated, so that any particular point connected with it may be at once found by a practitioner. In this respect Barzellotti's work is superior to that of any continental writer which we have yet seen. The history of the poison; its various forms; the symptoms and post-mortem appearances produced by it; the remedies or antidotes to be exhibited; a series of cases in which it has proved fatal; the chemical processes for detecting it; and, lastly, a general summary (epilogo dei fenomeni) of the pathological and chemical characters of the poison, render the toxicological history of each substance as complete as can be desired. One circumstance has surprised us, and created in our minds a feeling of disappointment. Out of the large number of cases reported in the volume, with but one or two unimportant exceptions, none are derived from the author's own observation, or from that of his countrymen. If we except a few ancient cases from Morgagni, nearly all are translated from Orfila. We have not met with above two or three from any modern Italian records. This appears to us singular, since instances of poisoning must, we should imagine, be as frequent in Italy as in France or England. We

cannot, therefore, present any cases in illustration of the author's views: those of Orfila are so well known to English readers as to render it wholly unnecessary to quote them.

In poisoning by mercury, gluten is recommended as superior to albumen for its antidotal powers. (p. 100.) Taddei had for this reason proposed that it should be kept ready-prepared by apothecaries; but this is a matter of difficulty, and it appears to us that albumen is to be preferred, if for no other reason, at least for its being generally at hand, and requiring no time or complicated process, like gluten, to prepare and apply it. In administering antidotes the old motto of "*bis qui cito*" should be constantly borne in mind. There are various opinions as to the manner in which albumen operates in counteracting the effects of corrosive sublimate, and also with respect to the nature of the chemical compound formed. We certainly do not think the antidotal powers of albumen or gluten are to be explained by the fact of their forming insoluble compounds with this salt, because some substances, far less soluble in water than it, are capable of acting as energetic poisons, as, for instance, the arsenite of copper and the carbonates of lead, copper, and barytes.

The chemical analysis of this poison is very well described; but we think the tasting of corrosive sublimate and receiving its vapours into the nostrils and fauces may be dispensed with, since, while this is a dangerous method of experimenting, it does not add to the certainty derived from chemical tests. Differences of opinion will exist among medical jurists, as to the relative value of the processes for detecting poisons; but we shall say at once of this work that we think it, in the chemical department, superior in accuracy to those of Orfila and Devergie, although it is very closely modelled on the treatise of the former. After the very modest appeal to his readers, made by the author at page 682, we should not feel ourselves justified in criticising his remarks, even had we found much room for criticism, which we certainly have not. If, then, we make a few observations on some of his processes, we trust he will consider them more in the light of suggestions for improvement than of any attempt to depreciate what he has already done.

In the reduction of corrosive sublimate, carbonate of soda appears to us preferable to caustic potash, because it is more universally found, is more readily miscible, and is not deliquescent. As an antidote to arsenic, we find a solution of sulphuretted hydrogen recommended; but this seems to us an objectionable practice, since, if given in small quantity, it cannot decompose the poison in the manner alleged, and if given in large doses, it may itself act deleteriously. The hydrated peroxide of iron is also recommended, followed by an emetic; but Taddei has proved that the arsenite of iron, stated to be formed, is itself a poison. The opinions of English practitioners are much divided on the alleged antidotal properties of the hydrated peroxide. For ourselves, we have never seen any good effects from its use, and the most rational plan of treatment appears to us to be the application of the stomach-pump, or the timely use of an emetic, should not the poison itself operate as such. In describing the processes for detecting arsenic, the author represents this poison (arsenious acid) to have "an acrid, styptic taste, and to excite salivation." (p. 162.) This appears to us to be a mistake; it is well

known that many persons who have been poisoned by it have not observed any particular taste. Besides, Dr. Christison has shown that this supposed acrid taste is founded on popular error. The use of chlorine or animal charcoal to decolorize organic liquids suspected to contain this poison, is very properly denounced by the author. The colouring matter presents no obstacle to the safe and certain action of sulphuretted hydrogen gas. The copper test should be rejected in all coloured liquids. In the event of our not finding arsenic in the viscera, we are recommended to examine the blood, lymph, and other secretions. We believe arsenic has not yet been detected in the blood of persons poisoned by it; we have frequently sought for it by the delicate "hydrogen test," but without success.

There is but little to remark in relation to the other metallic irritants. Albumen is recommended as an antidote in poisoning by copper; but it is not stated that the albuminate of copper formed, is readily soluble in an excess of the poisonous salt: a fact of some importance in the treatment, since it shows that for this substance to act with any prospect of benefit it must be exhibited in very considerable quantity. A strong characteristic of poisoning by copper is that the salts give a deep blue or green colour to the substances with which they are mixed, as well as to the vomited matters. In analysing the contents of a stomach, we must remember that sulphate of copper is sometimes given as an emetic, and very largely; a circumstance which might complicate the analysis, were we not prepared for it. The professor dwells much, chiefly on the authority of others, on the presence of traces of copper in most organic substances used as food. We have already, we think, disposed of this apparent difficulty in one of our late Numbers, in some remarks on the blood. To discover antimony we are recommended to calcine the precipitates obtained by the tests "with a mixture of potash and charcoal, in a *platina* crucible." (p. 219.) Surely this must be an oversight: antimony, in a metallic state, combines and forms so fusible an alloy with platina, that the crucible would be destroyed, and the antimony lost, if any were present. Oxalic acid probably is not so much used as a poison in Italy as in England; at least this is the only way in which we can account for the few lines devoted to its history and properties. The process for detecting this, to us, important poison, is not so satisfactorily given as we could wish to see. Considerable space is occupied by an account of the vegetable irritants, and of the best chemical means which we have at hand to discover the presence of the active alkaline principles to which they owe their poisonous effects.

The narcotic and narcotico-irritant poisons are next disposed of. The author here, as in the other parts of his volume, seems to have consulted every available source. In treating of opium, he objects to Christison's view of directing our analytical researches to the discovery of meconate of morphia alone; but we do not think his objection valid or of any practical force. It is not likely that meconate of morphia would be found in the human stomach without some strong suspicion or even circumstantial evidence of opium having been taken by the deceased. The discovery of the meconate would then be all that the law requires or ought to require. If we are to take the author's view, we ought to obtain narcotine, narceine, and codeine; but we might as reasonably go

a step further, and insist upon the resin, gum, tannin, and colouring matter, with other vegetable principles, being reproduced, before speaking to the presence of the drug. Such a refinement is not, in the present day, possible of application; and even if it were we should say that it would be unnecessary. The history of poisoning by hydrocyanic acid is very complete; but we certainly should not recommend the tasting of the poison as one step in the analysis. The author frequently subjoins to his description of the individual poison the mode of determining its quantity, a necessary kind of information to the medical jurist, which is not commonly met with in toxicological works. To the history of each class of poisons is appended a useful Table, containing a brief summary for each substance of the information contained in the foregoing chapters. Let us notice another excellent peculiarity of this volume, which is, that the subject of compound poisoning, one of considerable difficulty to the medical jurist, is discussed with brevity and clearness. The author acknowledges that he has taken the idea of this from Orfila.

The second volume is concluded with an examination of a very important question, viz.: how far those engaged in the general practice of the profession should be permitted to conduct investigations in toxicology? In examining this question the author repudiates the idea of throwing the least censure upon the general acquirements of his professional brethren; but he contends that these are, by the system of education adopted, chiefly directed to one object alone—namely, that of fitting a man to practise medicine and surgery. "Unless the attention of a student be earnestly and assiduously given to the study of chemistry, he is as likely to be unacquainted with the niceties of this science as with those of mathematics or astronomy." (p. 672.) "A man may be clever in his profession, but how is he to have at command the knowledge of a subject which he has never studied?" If this be true, courts of law ought not to receive their evidence in the same way as they would receive that of others who were well skilled in the science. To support these views he selects some examples in illustration of what a medical witness may have to do. "The analysis of corrosive sublimate is a very simple matter when it is presented in a pure and unmixed state; but when it is not found either internally or externally, how is the suspicion of poisoning to be supported or rebutted by the practitioner? Mercury may, however, be found in the body in the state of calomel, or oxide, or pure metal; and how is an opinion to be expressed here as to the cause of the symptoms, or the probability of the poison having been decomposed by the organic tissues? Either by showing the corrosive action on the viscera or by discovering the poison in a state of combination with organic matter, a process far from easy." (p. 675.) "But supposing that none of the poison is found, and there are no marks of its action on the living body, are we to deny the fact of poisoning?" Before an answer can be returned to this question, many minute circumstances must be taken into consideration, which will require a large share of experience on the part of the practitioner. In compound poisoning, how will the analysis be conducted, except by one who has at his command all the resources of science? Opium furnishes another example of difficulty to a medical witness who attempts to search for it without being well versed in chemistry. But here the author thinks that it is not

enough to detect meconate of morphia; he must be able to separate the other principles, and identify them by their properties, before he proceeds to infer that opium was present in the substance analysed. If this were indispensably necessary, we do not think there is one chemist in a thousand who would be competent to the task. Nux vomica presents another case of complex analysis. "This poison contains two principles, strychnia and brucia, which have not hitherto been found united in any other substance." (p. 679.) To justify an opinion that nux vomica was present it is not sufficient to show strychnia only, or brucia only; for the one exists in the bean of St. Ignatius, and the other in the false Augustura. Both of these principles must be produced—they must be separated from each other, and their properties respectively demonstrated. We agree with the author that but few of those engaged in general practice would be competent to perform such a duty; but at the same time we think there are few professed chemists who would succeed, unless they had specially directed their attention to the separation of the alkaloids from vegetables, and had acquired considerable experience in this department. The fourth illustration is an unfortunate one for the position assumed, since it goes against the wants of science generally, not against those who profess it. When a person has died from the effects of sulphuretted hydrogen gas, it is said the cause of death cannot be determined, since there is no poison capable of detection left within the body. The odour of the gas in the body would not furnish sufficient evidence, because in putrefaction the same odour is given out; but we think a case of this kind is easily disposed of. Death from sulphuretted hydrogen is the result of accident or homicide; in either case the body is found in the infected spot, and circumstantial evidence supplies that which was defective in a medical view. Besides, it by no means follows that the bodies of all who are destroyed by sulphuretted hydrogen should be found in a state of putrefaction.

Although we admit that there is a great deal of truth in the author's remarks on this subject, we cannot agree with him that these investigations should be taken out of the hands of general practitioners. It would entirely do away with the teaching of medical jurisprudence; for who would learn a science, the practice of which would be confined to a few? Our opinion is that the practical study of the subject should be enforced on all, and that the means of acquiring a sound knowledge of it should be freely at the disposal of every one who enters the profession. Nothing, as we have endeavoured to show, can be worse than the present system in England, in which individuals are summoned to give evidence, not simply of facts, but of *opinion*, whether they have been three months or thirty years in the profession; and evidence of the composition of a suspected poisonous liquid is taken from one who perhaps never attempted an analysis before. In spite of the discouragement thus given to those practitioners who have attended to this department of science, it is certain that a very great improvement has taken place of late years in medical evidence relating to poisoning. There are few of the modern school who are not competent to detect arsenic and some of the more common poisons readily, even under circumstances of difficulty; and we may hope that a few years will enable others to advance still further in the practice of toxicology. If the plan recommended by the author were

adopted there would be an end to all further improvement in medico-legal knowledge in the great body of the profession; but we frankly admit with him that, as the case at present stands, our legal authorities are bound to give a preference to those who have studied the subject over those who have not.

11. The first half of the third volume is devoted to the subject of WOUNDS and other local injuries, which the author treats differently to most other medico-legal writers. He gives to this branch the name of Surgical Jurisprudence (Chirurgia Forense). A large space is occupied by the account of injuries to different parts of the body; and a peculiarity of his plan is that he subjoins the surgical treatment of these injuries, which, in our opinion, is passing rather beyond the true bounds of the science. A large number of cases are added, chiefly derived from Morgagni, Uccelli, and Boyer. These cases are not so much adapted to illustrate medico-legal doctrines and opinions as the rationale and results of surgical treatment. They are mostly hospital cases, not cases which have come before the courts for investigation or trial.

The author defines a wound to be "a breach of continuity in the organic tissues violently produced through external causes." (p. 16.) This definition would of course bring fractures and dislocations within the meaning of wounds, a position which ought to be adopted in legal medicine, although most surgeons would dissent from it, as applied to surgery. In speaking of *ecchymosis*, he complains of Orfila for rejecting the word *sugillation*, as it is used to designate certain forms of ecchymosis, depending on internal causes. (p. 31.) We cannot conceive, however, on what consistent grounds this word "sugillation" is to be retained. The term ecchymosis expresses all that is required in legal medicine.

We do not intend to follow our author in his account of injuries to the different parts of the body. The following case shows what serious mischief may proceed from a slight local injury: "An old man, who was stealing wood, was suddenly surprised by the proprietor, when, after some words between them, the latter struck him on the back with a stick; the old man ran two or three paces, and then fell dead. On examination there was no mark of external violence, but internally the aorta was found ruptured. This was the cause of death." (p. 109.) One of the most common consequences of a wound of the bladder is immediate extravasation of urine, from which death takes place in about a week. "A medical student was shot through the bladder and rectum in a duel. He did not die until the twentieth day, and for the first few days no extravasation took place. This happens in gun-shot wounds only, when the sloughs produced by the wound fall off." (p. 174.)

A wounded man may die either within a short time or at a long period after having received the injury. In either case death may not depend on the wound, for the person may have been at the time labouring under disease, or circumstances may have sprung up to aggravate the effects of a comparatively slight injury. Serious questions of responsibility on the part of the aggressor will arise in such a case, and much will depend on the medical evidence given. It is well known that many subtle distinctions are made in the laws of continental countries, regard-

ing the relative mortality of wounds. The following case shows, in our opinion, the ill effects of such distinctions: it is quoted by Barzellotti.

"A woman, one evening, received a violent blow on the head, which felled her to the ground, in a state of insensibility. She remained in this condition all night, senseless, and without assistance, and the next day she died. On inspection, the skull was found fractured, and there was a large extravasation of coagulated blood under the dura mater. The medical witnesses pronounced the wound to have been absolutely mortal; but the court, disregarding their opinions, and, guided by the dictates of reason and common sense, acquitted the aggressor of homicide, simply because the deceased was not assisted, as she ought to have been, by professional men. Trephining might here, as in other cases of wounds of the head, accompanied by extravasation, have saved life. It is of no avail to say that the deceased was struck while out of the town in the evening, and that the gates were shut. Circumstances of this kind, although to the detriment of the patient, must not be charged to the responsibility of the aggressor. If a practitioner had been procured, then only, when the best means of art had failed in recovering the deceased, should we have been in a condition to judge of the mortality of the wound." (Vol. iii. p. 263.)

We are surprised how a doctrine of this kind can be set down as consistent with reason and common sense. Here is an injury inflicted, always serious, and commonly fatal, even under the best treatment timely rendered. The deceased did not seek for medical assistance, simply because it was impossible for her to do this, the murderous violence having deprived her of the power, by rendering her insensible. It was not to be expected that the murderer himself should fetch a professional man, and it cannot be supposed that medical practitioners are to be stationed about high roads, with their instruments, looking out for cases of this kind! Medical assistance could not then possibly be afforded to the deceased; but how can it affect the moral or legal responsibility of an aggressor in such a case as this, whether medical assistance be at hand or not? Our opinion is that the presence of a medical man with his instruments, when the wound is inflicted, does not diminish the crime, or lessen, in the smallest degree, the responsibility of the accused. Wounds of the carotid artery have not always proved fatal, owing to the timely application of a ligature. But if a man divide the carotid artery of another, and the wounded person die, is the aggressor to be acquitted of homicide because, had a surgeon been at hand, he might have tied the artery and saved life? Reason will show that, if such a position be once admitted, the responsibility of those who inflict murderous wounds may be, in most cases, easily frittered away by abstract medical speculations.

The characters of *vital* and *post-mortem* wounds are elaborately described. Some space is devoted to the subject of spontaneous combustion, a doctrine which our author seems inclined to favour (p. 285); but as he would not give an opinion in the affirmative until, among other proofs, it were shown that the burning could not have taken place from communication with ignited matter, we willingly join him in the degree of credit which he virtually attaches to this alleged phenomenon. The remarks on the chemical examination of blood-stains are practical and valuable. Some interesting cases are related, in which the importance of these enquiries are well set forth. The responsibility of aggressors, as it is affected by necessary surgical operations on the wounded—the nu-

merous circumstances which aggravate the effects of local injuries, and the means of identifying wounds by their cicatrices, are subjects ably treated in the remaining chapters.

We here close our notice of this work, having confined it to the strictly medico-legal portion. The last half of the third volume treats of contagion, and medical police regarding contagious diseases. We may reserve our remarks on this until another opportunity, when the subject of medical police comes before us. We have, in a great degree, anticipated our judgment of the whole treatise. If we regard matter, system in the individual parts, and comprehensiveness in the whole, we think it, without exception, the best on medical jurisprudence that has issued from the continental press. The subjects are treated with as much judgment and ability as the mind of any man can be supposed capable of bringing to a science which demands not only an extensive knowledge of every branch of medicine, but of the principles of jurisprudence. One rare merit this work possesses over those of many French and German authors—in no part have we met with anything like dogmatism. The writer states his opinions firmly and modestly; and this is no slight praise to him, considering that of all sciences there is none so apt to lead its followers to dogmatize, because they may, in many instances, dogmatize with impunity. An error propagated on great authority may, it is well known, remain many years undetected; and, in the meantime, lead to irreparable mischief. We have not hesitated to state freely our opinion of some of the doctrines of Professor Barzellotti, but we think this will be no ground of displeasure to one whose object has been evidently throughout to write for the sake of truth, and not for victory. We have endeavoured to do justice, at the same time, to our author and our readers; to show that what we considered faults did not escape our notice. But in executing this task we trust we have always borne in mind the noble observation quoted by him from his countryman Redi, "*Siamo uomini e per conseguenza soggetti ad errare.*"

Art. III.

1. *Statistical Report of the Sickness, Mortality, and Invaliding among the Troops in the West Indies; prepared from the Records of the Army Medical Department and War-office Returns.* Presented to both Houses of Parliament by command of her Majesty.—*London*, 1838. pp. 103, with a Supplement of 40 pp.
2. *Statistical Report of the Sickness, Mortality, and Invaliding among the Troops in the United Kingdom, the Mediterranean, and British America; prepared from the Records of the Army Medical Department and War-office Returns.* Presented to both Houses of Parliament by command of her Majesty.—*London*, 1839. pp. 172.

We have already recorded our sense of the value of these documents, and presented our readers with a view of a portion of their contents, calculated, we are convinced, to justify the opinion we expressed. Want of space compelled us to limit our extracts and remarks in the former Number mainly to the statistics of two classes of the diseases, which are

examined numerically in these volumes, fevers of the tropics, and pulmonary affections. These were selected at once for their paramount importance and the great value of the information produced regarding them; a value marked, we admit, more frequently by the subversion of former than the perfect establishment of new opinions: but we were well pleased to have advanced so far, regarding, as we must, the destruction of error as the first step, and this an essential one, in the advancement of truth. There are, however, other and very important matters in these volumes, treated with the same preciseness as to facts, and the same sobriety of deduction which characterized the portions we have already noticed; and of these we shall present a concise, but, we trust, comprehensive view to the reader.

1. *Diseases of the Liver*. On this subject, Major Tulloch makes the following remark, when treating of the maladies and mortality of the Windward and Leeward Island command in the West Indies: "Though this class of diseases is by no means so common as in the tropical regions of the eastern hemisphere, it is nearly thrice as prevalent as among troops in the United Kingdom, and occasions about five times as high a rate of mortality." On tracing the prevalence and mortality of this disease through the different West Indian commands, we find in that of the Windward and Leeward Islands there are admitted 1,946, of whom die 161, or one in twelve; and when we take the proportion to the mean strength, it is stated that 22 per 1000 are admitted, and 1·8 per thousand die. There are, in the different stations throughout this command, considerable varieties in the proportions in which the troops there have contributed to produce this general ratio. At Trinidad, for example, the proportion of mortality per mean strength from this disease is 1·1, in Tobago 2, and in Grenada 4·5 per thousand. This last very striking excess above the general ratio, the reporter acknowledges himself to be utterly unable to explain. The proportions stated refer to the white troops only. The blacks suffer from this class of diseases and from all those which principally produce the mortality in a much smaller proportion than the whites, if we except pulmonary diseases and eruptive fevers, to which classes of disorders they manifest a much greater proclivity than Europeans.

Continuing still to trace the mortality from diseases of the liver through the different commands, we find that in Jamaica, notwithstanding its high temperature, this is only 1 for 1000; and that, as compared with Britain, the ratio of admissions is as 10 to 8, and the deaths as 1 to $\frac{4}{10}$. Two of the stations, Fort Augusta and Spanish Town, present examples of striking exemption from these diseases, the mortality from this source being only 0·3 per 1000, or exactly the same as in civil life in England. We can discover nothing in these stations to account for this comparative exemption. Their temperature is not below the average of the island, in which the extremes in stations occupied by our troops are as follows: at Kingston, average maximum 85°, minimum 76½°; at Maroon Town, average maximum 80½°, minimum 68½°. At Fort Augusta, under the very walls of the fort, is an extensive marsh or lagoon, interspersed with small islands, covered with mangrove bushes, and abounding in every species of decayed vegetation, from which issue most offensive effluvia. In Spanish Town, the temperature during the day is much higher than

at the other stations; and the soil being clayey and tenacious, partial swamps are produced by heavy rains. "The country, as far as the foot of the mountains, being a dead level, and no artificial means employed to carry off the superabundant moisture, it remains until evaporated by the sun's rays; and when the land winds blow over the ground thus saturated, they are supposed to have considerable influence in the production of fever." There can be no doubt of the abundance of rum and the facility of procuring it in both these situations. We thus observe the reputed causes of disease of the liver; heat, marshy effluvia, and ardent spirits, existing in abundance, whilst the disease prevails to a very insignificant extent, without any assignable reason for the exception, but the vague and conjectural one, of some specific power in the climate counteracting their operation.

In the other West Indian commands, the ratio is deduced from so small a number of cases, as can hardly be considered to constitute a fair ground of reasoning: we consequently pass on to the numbers stated in Great Britain, the Mediterranean, and North American stations. In our own country, we find that the deaths from liver disease, in civil life, amount (the ratio being taken from subjects insured in the Equitable Office from 1801 to 1832,) to ·3 per 1000, and among the troops to ·4 per 1000. The mortality from this class of diseases is exceedingly low in certain descriptions of force, amounting* to 1 in 10,000 annually in the foot guards, amongst whom we had occasion, in our former article, to point out the extraordinary prevalence of consumption; whilst the returns from the West Indian depôts show the ratio there to be the same. In the dragoon guards and dragoons, 4 in 10,000 die annually of liver disease; and in the household cavalry, 5 in the same number. In passing to the Mediterranean stations, we find the mortality in Gibraltar, 4 per 10,000 annually, or the same as among troops in the United Kingdom; at Malta, 11 in 10,000, or nearly three times as high as in the former station; whilst among the civil population of this latter garrison, it is extremely low, being only 11 in 100,000, or one tenth of the proportion among the troops. A supposition of Major Tulloch's may serve at once to explain the great difference between the prevalence of these diseases in two stations, not materially differing from each other in climate, Gibraltar and Malta, and the still more striking discrepancy between its effect on the military and civil population in the latter station. He remarks that the troops in Malta may have contracted the disease during a previous course of service in climates where it was very prevalent. We think this very probable; but, however it may be, the fact of the great exemption of the Maltese is sufficient evidence that heat, for many months, little inferior to that of tropical regions, is inadequate to produce a prevalence of liver disease. In the Ionian islands, the rate of mortality from this malady is only 8 in 10,000, and this low annual rate pervades all the islands, Zante alone excepted, where 20 in 10,000 die yearly—a higher rate than exists in any other Mediterranean station: no explanation can be found of this discrepancy.

In British America, this disease contributes but little to the average mortality among the troops. In the Bermudas, 5 in 10,000 die of it

* For the sake of greater perspicuity, we state the proportion in integers.

annually, and in Nova Scotia and New Brunswick, only 2 in the same number; whilst in Canada, the admissions and deaths are in the same low proportion as in these latter colonies. Major Tulloch here makes the following remark, which is equally applicable to Canada, New Brunswick, and Nova Scotia:

"The prevalence of this class of diseases is almost the same as among the dragoon guards and dragoons serving in the United Kingdom, and the mortality is only half as high. This fact tends to throw very considerable doubt on the supposed influence of spirituous liquors in inducing affections of the liver, at least in a cold climate; for owing to the low price of these in Nova Scotia and New Brunswick, there are few stations where intemperance is carried to a greater extent; yet not only do the troops suffer less from diseases of the liver than at home, but the proportion of deaths is only two thirds as high as among persons insured in the Equitable office, who, from their rank in life, as well as the caution exercised in their selection, are by no means likely to be addicted to that vice." (*British-American Report*, p. 17, *b.*)

On surveying the returns of affections of the liver, we have certainly been impressed with the scantiness of their contributions to the general mortality of most of the districts where their influence has been traced; and it will occur to the reader, either that such affections figure much too highly in the opinion and language of certain medical men in this country, or that they are the most curable of human maladies. These returns, however, disprove the latter branch of the alternative, for we find the proportions of deaths to the number treated by no means insignificant. The fact appears to be, that throughout the western hemisphere, diseases of the liver are very rare in comparison of those of the lungs and of the stomach and bowels; and though in some localities they are more frequent than diseases of the brain, they contribute much less than these do to the amount of mortality. We regret that we have no East Indian returns from which to deduce a comparison regarding the prevalence of liver-diseases between the western and eastern hemispheres.

2. *Diseases of the Stomach and Bowels.* In the West Indian Report we find that in the Windward and Leeward command, 20·7 per thousand die annually of these complaints, the proportion attacked being 421 per thousand, of which one in twenty dies; whereas, in this country, the admissions for such diseases are only 95 per 1000, and the cases are so mild that only one proves fatal for 2000 of the mean strength. Dysentery and diarrhœa are the great sources both of admissions and mortality from this class in the West Indies. Acute dysentery is directly fatal to 1 in 26 of those attacked, and a very great mortality results from a considerable proportion of the cases passing into the chronic form, of which cases 1 in 5 is fatal. Diarrhœa is a very frequent disease, and is fatal to 1 in 42 of those it attacks.

In the Jamaica command, the annual admissions from this class of diseases amount to 238, and the deaths to 5·1 per thousand of the mean strength, the admissions being only about one half, and the deaths scarcely one fourth part as high as in the Windward and Leeward command. The diseases, dysentery and diarrhœa, which contribute most largely to the sickness and mortality, are less prevalent and much less fatal in Jamaica; for, whilst the admissions from the whole class of bowel-complaints amount only to 238 per 1000, acute dysentery is

fatal to 1 only in 40, and diarrhœa to only 1 in 123 of those attacked. The only discernible reason for this difference between the two commands is, that in Jamaica the troops have twice as much fresh provisions as those in the Windward and Leeward command, "which," as Major Tulloch justly remarks, "if it does not prevent the occurrence of the disease, may obviously have a very material effect in modifying its virulence, by supplying a more digestible and less irritating diet than salt meat."

Passing over the other two West Indian commands, as containing too small a number of troops to furnish *data* for a fair inference, we proceed to trace the prevalence of diseases of the stomach and bowels among troops in the United Kingdom. In the dragoon guards and dragoons, the annual admissions amount to 94 per 1000, and the deaths to 8 per 10,000 of the mean strength, a very small mortality, amounting to 1 only in 117 of those attacked; whilst in other descriptions of force, the proportionate mortality is still lower, being in the foot guards 7, in the household cavalry 3, and in the West Indian depôts 4 in 10,000.

When we turn to Gibraltar, we find bowel complaints considerably more prevalent and fatal than in the United Kingdom, the admissions being 186, and the deaths 2·1 in 1000 of the mean strength annually. The mortality mainly arises, as in the West Indies, from acute and chronic dysentery and diarrhœa; but, although these are the most prevalent and fatal diseases, they are so in a much less degree than in the West Indies, whilst the pure inflammations of the intestinal canal are of nearly equal prevalence and fatality in both these colonies and in Great Britain. The reporter refers to the returns from the civil hospital of Gibraltar, and finds that bowel complaints do not there bear so high a proportion to other diseases as among the troops.

"This is the only way in which we can make the comparison, and though it does not supply such accurate grounds for our deductions as could be wished, it certainly adds to the presumption that the tendency to diseases of the bowels at this station may be increased by the large proportion of salt meat issued to the troops, especially as the officers suffer so little from them, and there is nothing in the duty of the troops likely to have created such a peculiarity. Whatever may be the opinion in regard to the possibility of this diet creating such diseases, there seems little doubt that for those who may have been suffering under them, and have recently come out of hospital, it is most inappropriate, as having a greater tendency to induce a relapse than the milder and more digestible nutriment afforded by fresh provisions."(*Mediterranean Report*, 13, *a*.)

A confirmation of these views is derived from a comparison drawn by Major Tulloch, between the mortality arising from these complaints among the troops, and the civil population of Malta. Bowel complaints are more fatal among the troops in this island than in Gibraltar, the mortality being at the annual rate of 3·6 per thousand of the mean strength, whilst the admissions are only 155 per 1000, or considerably lower than in the latter garrison. An extract from a return of the deaths among the civil population of the island, for thirteen years, shows an extreme accordance between the troops and the inhabitants in the mortality resulting from these diseases, the deaths in the latter class being 3·8 per 1,000, or a small fraction higher than among the soldiers. The diet of the troops is good and ample, and their meat is fresh.

In the Ionian islands, the admissions from these complaints are 156, and the deaths 3·5 per 1000, being in the same proportions as in Malta; and this latter ratio is very equally preserved throughout the islands, with the exception of Santa Maura, where the mortality from this cause is considerably below the average, the deaths from bowel complaints being only 2 per 1000, whilst those from fever are more than double what takes place in any other island. This tends to confirm views much insisted upon by writers on the influence of malaria in tropical regions, that bowel complaints are there equally with fever the products of malaria; that, according to certain varieties in its intensity or composition, the one disease or the other is produced; and that, consequently, a great prevalence of the one disease necessarily excludes that of the other. Whatever may be thought of the more theoretical parts of this opinion, there can be no doubt that the occurrences at Santa Maura are in confirmation of the plain fact on which they are founded.

When we transfer our attention to British America, we find that in Bermuda, the admissions, on account of these diseases, amount to 415, and the deaths to 5·3 per thousand, the former ratio being as high as in the Windward and Leeward command in the West Indies, though the mortality is much smaller. In explanation of the number of admissions, it should be observed, that Bermuda is in latitude 32° north, and has a high temperature, its summers especially being very hot; and, besides, there being much difficulty in procuring a sufficiency of fresh meat in a group of very small islands, salt rations are much employed, and the water, moreover, is brackish. In the colder latitudes of North America, bowel complaints are much less prevalent and fatal. In Nova Scotia and New Brunswick, for instance, the admissions are only 94 and the deaths 1·5 per 1000; and when from this latter ratio we deduct the deaths in the Fifteenth Foot, which brought with it, from the West Indies, several invalids labouring under chronic affections of the bowels, the proportions of both the admissions and deaths are substantially the same as in the United Kingdom. So in Upper and Lower Canada, the admissions are 155 and the deaths 1·3 per 1000; but making suitable deductions, on account of the mortality in the Second Battalion of the Sixtieth, which brought with it, from the West Indies, many soldiers worn out by dysentery, we find the deaths in nearly the same proportion as in Nova Scotia, New Brunswick, and the United Kingdom.

These statistics confirm the opinion generally entertained, that bowel complaints are especially prevalent and fatal in warm climates, whilst in the colder latitudes they are comparatively unfrequent and very mild. They afford ample evidence too, that irritating and indigestible diet, particularly salt meat, cooperates very powerfully with the climate in producing these diseases. In the details of the returns, there is a fact manifested, which is calculated to impress very forcibly on any individual accustomed to observe diseases at home only, the extreme prevalence and fatality of diarrhœa in warm climates, especially where irritating diet is conjoined with the influence of climate. The accuracy of the military reports on this point receives confirmation from some statistical details of the Lisbon hospitals, which we recently examined,—Diarrhœa contributed in a surprising degree to the sickness and mortality in these institutions. Now we happen to know that the food of that class in

Lisbon, who are likely to be inmates of a public hospital, consists mainly, in addition to fruit and vegetables, of salt fish.

3. *Diseases of the Brain.* The statistics of this class of diseases furnish much interesting information. In the Windward and Leeward command in the West Indies, the admissions are 28, and the deaths 3·7 per 1000; and deducting the cases of *delirium tremens*, to which more than one half of the sickness and mortality from this class of diseases are owing, the admissions are 12, and the deaths 1·6 per 1000, or just double of what takes place from the same cause in the United Kingdom. In Jamaica the admissions are only 14, and the deaths 2·6 per 1000, the cases of *delirium tremens* being only one third as numerous as in the Windward and Leeward command. In the United Kingdom the admissions are 6 in 1000, and the deaths 8 in 10,000, or only half of what occurs in civil life, according to the tables in the Equitable office. In explanation of this very striking discrepancy, Major Tulloch justly remarks that this class of diseases is supposed to be a greater source of mortality in the upper walks of life, from which the subjects of the Equitable office are drawn, than among the lower orders, whence our soldiers are recruited.

In Gibraltar the admissions are 6 in 1000, or the same as in the United Kingdom, whilst the deaths are still fewer, amounting to only 5 in 10,000. In Malta the admissions and deaths are precisely in the same proportions as in the United Kingdom: viz. the former 6 in 1000, the latter 8 in 10,000. In the Ionian islands, the admissions are 10, and the deaths 1 per 1000. When we again turn to the Western hemisphere, we find in Bermuda the admissions 17 and the deaths 2 per 1000; in Nova Scotia and New Brunswick, admissions 11 and deaths 1·3; and in Canada, admissions 14 and the deaths 1·2.

From these proportions, we perceive that affections of the brain are generally more prevalent in warm than cold climates, but that the prevalence or paucity is by no means in proportion to the warmth or coldness of the climate. This will be observed if we compare Gibraltar and Malta with Bermuda, and the United Kingdom with Nova Scotia, New Brunswick, and the Canadas: in fact, we perceive throughout a predominance of brain affection in the western compared with the eastern hemisphere. It would be interesting to discover whether this preponderance is in diseases of the brain in general or in some special affection. The following facts evince that the difference is mainly owing to the extreme prevalence and fatality of *delirium tremens* among the troops in our American colonies. In the Windward and Leeward command, *delirium tremens* gives rise to four sevenths of the admissions from all brain diseases, and to the same proportion of the mortality from them. In Jamaica more than one fourth of the admissions and nearly one third of the deaths arise from the same cause. In Bermuda, more than one half of the admissions and nearly one half of the deaths are attributable to it; in Nova Scotia and New Brunswick half of the cases and one fourth of the deaths; and in the Canadas, three eighths of the cases and one fourth of the deaths. When we turn to the European reports we find in Gibraltar, that of brain diseases, little more than one ninth of the admissions, and less than one sixth of the deaths, are from *delirium tremens*; in Malta, less than one sixth of the admissions, and exactly one

sixth of the deaths; and in the Ionian islands, one fourth of the admissions and three sevenths of the deaths, being the largest proportions in any European colony.

There can be no doubt that the great mortality from this disease in our American possessions arises from the facility of indulgence in ardent spirits which they afford. In our European colonies, the facilities of indulgence in liquor are considerable, and there can be little doubt of our troops availing themselves of them, but the effect is less pernicious, probably from wine being the beverage generally employed, which, however injurious when taken in excess, is much less prone to produce the brain fever of drunkards than ardent spirits. How many lives might be saved, could the unfortunate tendency of the British soldier to drunkenness be corrected, may be conceived, when we consider that where the ratio of *delirium tremens* to other affections of the brain is the highest, there do these last exist in the greatest abundance and severity; so that these reports show that habits of intoxication are the most fertile source of affections of the brain in general. Regarding the influence of mere heat on their production, Major Tulloch is of opinion that it is not very great (*West Indian Report*, p. 9); but on the conjoint influence of high temperature and habits of intoxication, he makes the following striking remark:

"The proportion admitted at this station (Gibraltar) is exactly the same as among the dragoon guards and dragoons at home, and the ratio of deaths is even lower; it may be necessary to state, however, that in order to obviate the consequence of exposure to solar influence, a precaution is adopted in this command of confining the troops to barracks during summer from nine or ten o'clock in the morning till four or five in the afternoon. Cases of *delirium tremens* are comparatively rare, *not more than half as many have died by it in the course of nineteen years, as in the Mauritius during one year alone out of half the strength.* Though intemperance is by no means uncommon among the garrison, it seems neither to be carried to that extent nor to be productive of the same prejudicial effects, morally or physically, as in tropical climates, where consequently there is the more urgent necessity for restrictive measures to prevent an indulgence so pernicious to the health and discipline of the soldier." (*Mediterranean Report*, p. 15, *a.*)

4. *Diseases of the Heart.* These are not sufficiently frequent among soldiers to receive specific mention in the very luminous abstracts of the returns given by Major Tulloch; but we find them in the original returns, under various designations, some of them distinct, others sufficiently vague. We are impressed with their paucity, especially in comparison with the disease whence they so often in civil life derive their origin, rheumatism. Thus we find in the West Indies, that in the Windward and Leeward command there are 4202 cases of rheumatism and 11 of carditis, and 23 returned under the vague head of palpitatio. In Jamaica there are 1479 cases of rheumatism and 8 of carditis. In the United Kingdom there are 2244 examples of rheumatism, whilst the dragoon guards and dragoons furnish 24 cases, named carditis, the foot guards 5 cases of death from *morbus cordis*, and 2 from carditis; and the household cavalry one death from pericarditis, the deaths only, not the admissions, from these diseases being specified in the returns of the household troops. In Gibraltar, there are 2309 cases of rheumatism, 12 of carditis, and 8 of *morbus cordis;* in Malta, 1383 cases of rheuma-

tism and 4 of carditis; and in the Ionian islands, 2428 cases of rheumatism, 4 of carditis, and 20 of *morbus cordis.* In our North American possessions, there are in Bermuda 390 cases of rheumatism and one of carditis; in Nova Scotia and New Brunswick, 1310 cases of rheumatism and 6 of carditis; and in the Canadas, 2427 of rheumatism and 11 of carditis.

We have no means of proving it from the statistics of civil life, but we are convinced by these returns that disease of the heart is less frequent and bears a lower proportion to rheumatism among British troops than among the labouring class in this country from whom they are recruited. This admits a ready explanation in the greater care taken of sick soldiers, and the less haste in their return to active employment on the abatement of disease, than occur to persons of the same rank of society in civil life. This comparative paucity, however, of cardiac affection, and the great amount of rheumatism which these returns indicate, ought, we think, to convince M. Bouillaud that carditis is not an essential part of rheumatism—not the rule, as he terms it, but the exception, or we should find, notwithstanding every allowance for the very advantageous circumstances in which soldiers are placed in respect to medical care and treatment, the latter disease laying the foundation of the former more frequently than it does.

5. *Epidemic Diseases.*—A. *Yellow Fever.* The reports contain much valuable information regarding epidemics, and as these occasional visitations, besides their intrinsic importance, present many interesting points of comparison with the ordinary diseases of any given climate, we shall transfer a portion of it to our pages. The facts mentioned regarding the yellow fever of Gibraltar, though detailed with the utmost brevity and with a scrupulous abstinence from all intermixture of speculative opinion, appear to us admirably calculated, not to *settle* the perplexed question of the origin of epidemics, but to render the adjustment of the controversy, which has long raged respecting it, more probable, by presenting distinct facts at a period when the minds of men are disposed to contemplate them with more equanimity than when the feelings are excited by the recent occurrence of such a visitation, and the disquisitions to which it gives rise.

Whilst the admissions from all the ordinary fevers of Gibraltar amount in nineteen years to 8165, and the deaths to 140, being a mortality of one in fifty-eight, the admissions from the epidemic fever of 1828 amounted to 1522, and the deaths to 423, constituting a mortality of one in three two-thirds. The strength of the garrison was 3494, of whom nearly one half were attacked by the disease, and one eighth perished. Now, in ordinary years, so far from the garrison of Gibraltar having suffered in any remarkable degree from fever, the proportion of deaths is only about one half higher, and the admissions about double what occurs from the same cause in this country.

Major Tulloch gives a summary of the preceding epidemics of yellow fever in this garrison. In that of 1804, there perished 54 officers, 864 soldiers, 164 soldiers' wives and children, and 4864 civilians. In 1813 it destroyed 461 of the troops and 883 inhabitants; and in the following year it again broke out, and 114 of the former and 132 of the latter class perished. From this time till 1828 there was no epidemic.

In many respects there was an accordance among the various epidemics, that of 1828 included. Though in certain of them a few scattered cases may have appeared earlier, yet in the latter end of August or the beginning of September was the commencement of the marked epidemic prevalence; the mortality attained its *maximum* about the middle of October, and, though no marked diminution either in prevalence or severity could be distinctly traced to the occasional reductions of temperature, which took place during the continuance of the disease, yet these epidemics have never been known to make their appearance at this station during winter, and they have always declined in severity as this season approached. Of all the means resorted to for the purpose of checking the progress of the disease, removal to an encampment beyond the precincts of the garrison has been most invariably successful. The place selected for this purpose was the neutral ground, and "though it is composed of materials which are supposed highly favorable to the formation of malaria," yet the epidemic has uniformly been checked in bodies of persons, civil or military, who have been removed thither; indeed it is stated by the surgeon of the Twelfth Regiment, that of 92 women and 190 children encamped in the neutral ground, during the whole period the epidemic of 1828 prevailed, not one was attacked, though in constant communication with the soldiers of the corps, who went daily from that encampment to do duty in the town, and of whom many were attacked with the disease on their return. Major Tulloch's account of the geological formation of Gibraltar is not such as would lead to the supposition that it would prove unhealthy; neither is it ordinarily so.

"The rock of Gibraltar is principally composed of gray limestone. The upper part is almost entirely devoid of soil, except in the gullies, and a few spots where it has accumulated by the action of the rains. The level piece of ground on which the principal streets of the town are built is composed of red sand, and towards the south side there is some light fertile mould, but it is exceedingly scanty; every spot available for the purpose, is laid out in garden-grounds, and wherever an adequate supply of soil and moisture can be obtained, the produce is most abundant. About 200 acres of that portion of the isthmus, termed Neutral Ground, have also been brought under cultivation, and furnish an ample supply of vegetables for the garrison. The whole surface of the rock, particularly on the western side above the town, is much intersected by deep gullies, in which, during winter, water occasionally lodges; they are, however, always dry in summer. To the southward are several extensive tanks, containing nearly two million gallons of water, for the use of the troops and shipping; but in no part is there any ground which can be designated as marshy, and in general it is necessary to dig to the depth of thirty or forty feet before water can be procured." (*Mediterranean Report*, p. 3, *a.*)

It is more reasonable to look for the causes of an extraordinary degree of sickness in some unwonted state of the seasons, than in a geological condition, which is permanent. Investigations in this direction, however, leave the subject as obscure as before. "There was nothing remarkable," says the reporter, "in the atmospherical phenomena which preceded the last epidemic: the summer was cool and pleasant, and the easterly winds so much complained of at the station, were more rare than usual; there was rather less rain, and fewer rainy days than had occurred during the two preceding years, but a smaller quantity had often fallen in other

years, which proved remarkably healthy." Tables are given of the temperature, the barometric pressure, and the quantity of rain during a healthy year, 1827, and an epidemic one, 1828; and the greatest uniformity, in all these respects, existed between them; whilst, as regards easterly winds, there was a great advantage on the side of the epidemic year.

The question has often been mooted—Is yellow fever something *sui generis*, or is it but an aggravated form of remittent fever? To this, Major Tulloch declines formally to reply, though we think a very satisfactory reply is contained in the statistics of the two diseases; for whilst the former destroys at the rate of 1 in $3\frac{2}{3}$ of those attacked, and so many are attacked, that 443 of the garrison perish in four months; the other is fatal to 1 of 11 attacked, and the admissions are so sparing, that in nineteen years only 28 deaths arise from it. The following remark tends, we would say, to confirm the conclusion deducible from statistics:

"In Gibraltar the same individual has seldom been attacked twice, even though a long series of years may have elapsed since he first suffered from it; but in the West Indies, and on the west coast of Africa, a former attack of remittent fever secures no such immunity,—indeed it could be proved, by reference to returns from these stations, that in many corps every soldier must have been treated, on the average, twice or thrice for remittent fever, during the four years of his service there." (*Ib.*, p. 9, *a.*)

By a similar argument it might be shown, that the epidemic was not an aggravated form of common continued typhus, or any other of the ordinary fevers of the garrison.

The facts presented in the Report appear to us to prove:

1. That the cause of the disease was not diffused through the general atmosphere of the district, for the troops in the different encampments in the vicinity, and who had no communication with the town, were not attacked; but was confined within the walls.

2. That this cause was not engendered by any manifest distemperature of the seasons.

3. That the disease resulting was not a mere aggravation of any disease ordinarily existing in the garrison.

The points remaining to be investigated appear to be:—Was the cause operating within the garrison contagion, or was it what may be termed a local malaria; and, in either case, how was this cause introduced or engendered? With regard to importation, Major Tulloch rejects (and on good grounds) the opinion which we remember had a short vogue whilst the epidemic was existing, that it was introduced in a Swedish ship from the Havannah. It thus appears that the main points still remain to be answered, but the Reports have certainly narrowed the ground of controversy.

B. *Cholera.* Regarding another epidemic, cholera, these Reports contain information, which is, as usual, precise and succinct. It appears that among cavalry in the United Kingdom, of 171 attacked 54 died, or 10 in 32; of troops in Gibraltar, 459 were attacked, of whom 131 were fatal, or 10 in 35; in Nova Scotia, of 210, 59 died, or 10 in 35; in Canada, in 1832, of 259, 94 died, or 10 in 28; in the same colony, in 1834, of 97, 33 died, or 10 in 29; and in Honduras, among black troops, of 62, 20 died, or 10 in 31.

Major Tulloch appends the following remark, as a commentary to these returns: "Thus, under all the modes of treatment which may have been adopted on these different occasions, the proportion of deaths to recoveries has not varied above one fourth, showing that the remedial measures hitherto employed, can have had little if any effect in counteracting the fatal character of the disease." (*British-American Report*, p. 316.)

A reasonable demur might, we think, be raised to this conclusion from such premises. The subjects of the disease are all British troops, all receive medical assistance of the best description, and the equality of the deaths may be ascribable to the equality of the skill of the practitioners employed, not to the inefficacy of the treatment. This conclusion would have been justifiable, had he found a uniformity of fatality between groups of cases skilfully treated, and others left to the care of nature. A subsequent remark, however, is more calculated to justify such a conclusion.

"In both these years (1832 and 1834), when this epidemic prevailed, the native Indians suffered from it to the same extent as the white population. At three settlements from which returns were received, about a twelfth part of the population died in 1832, and about half that proportion when it again prevailed in 1834. Although their principal remedy consisted in swallowing large quantities of charcoal mixed with lard, almost exactly the same proportion recovered as among the white inhabitants of the towns, who possessed every advantage which the aid of medical science could suggest." (*Ib.*, p. 31, *b.*)

It should not be unremarked, in discussing the question of treatment or no treatment in cholera, that one of the ingredients in this nostrum of these "stoics of the woods," the charcoal, was much esteemed by the late Dr. Jackson, as a remedy for dysentery;* and there is sufficient analogy between this disease and cholera, to render it probable that what is useful in the one disease may be of some efficacy in the other.

Major Tulloch points out the real object to be aimed at in the treatment of cholera, the early application of means, when he says:

"In all situations, and under all modes of treatment, about one in two died of the cases in the civil, and one in three of those in the military hospitals; but from the surveillance exercised over the troops, nearly half of the cases among them were noticed in the premonitory stage, and consequently could be treated with greater prospect of success than in the civil hospitals, where the great majority of patients were far advanced in the disease before they applied for medical aid." (*Ib.*, p. 31, *b.*)

The great question of the contagiousness of this epidemic the reporter does not leave untouched, but, as others have done before him, he leaves it unsettled. He describes the course of the disease along the principal channels by which the tide of emigration and commerce flowed through the country. He presents a table, displaying its appearance at successive points along the banks of the St. Lawrence, and the lakes; and both in 1832 and 1834, the accordance between the time of its outbreak and the geographical position of the infected place, in reference to the mouth of the river, is (with one exception) so perfect, as to justify his exclamation, "this singular disease may be said to have travelled with post-like regularity!" This is a strong *primâ facie* case for contagion; but after

* Sketch of Febrile Disease, p. 271.

stating that quarantine regulations were in some instances apparently effectual, and in others of little avail, and that neither the physicians nor those in constant attendance on the sick, exhibited any peculiar liability to the disease, he adds:

"Of course it is impossible, in a limited Report of this nature, to enter fully on all the facts and arguments bearing on the important and much-disputed topic of contagion; we can only say, that all which has been adduced on either side seems to fall short of absolute proof; and even those who have had the best opportunities of forming accurate opinions, by watching the progress of the disease, are forced to admit that its origin is still involved in mystery, or, at least, that the contrariety of results can only be reconciled by supposing, that under some circumstances it may be contagious, while in others it may be the reverse." (*Ib.*, p. 32, *b.*)

The increasing fatality of the disease, with advancing years, is distinctly proved in all the reports. The following numbers we collect from the table given to illustrate this fact, in the Report of Nova Scotia. Of the total strength under 18 years of age, 18 in number, none were attacked; from 18 to 25, strength 502, 1 died, or 2 per 1000; from 25 to 33, strength 829, 30 died, or 36·2 per 1000; from 33 to 40, strength 158, 14 died, or 88·6 per 1000; from 40 to 50, strength 37, 4 died, or 108 per 1000: average total, 34·7 per 1000.

"Of 293 women attached to the different corps, 87 were attacked, and 16 died, being almost exactly the same proportion as among the soldiers. Children were remarkably exempt, for of 560 in the garrison, only 16 were attacked, and 6 died. The officers, also, suffered but little; out of a strength of 60, only 4 were attacked, all of whom recovered." (*Ib.*, p. 18, *b.*)

c. *Plague.* There has been no epidemic of plague in any territory whence the materials of these reports are derived, within the period they comprise. Two, however, are mentioned by Major Tulloch, that of Malta in 1813, and that of Corfu in 1815. Both were extremely fatal; for in that of Malta, of 5600 of the natives attacked, 4486 perished, or 80 per 100; and in that of Corfu, of 700 inhabitants attacked, 630 died, or 90 per 100. These last numbers are not official; but on the authority of Dr. Gregory, in his "Practice of Physic," Major Tulloch informs us, that of 28 soldiers attacked in Corfu, only 3 recovered; whilst in Malta there were 7 recoveries in the same number. The natives of northern climates displayed, throughout the course of this epidemic, much less susceptibility to the disease than those of the south of Europe, to which circumstance Major Tulloch remarks, the exemption of the troops may perhaps be attributed. On the question of contagion, Major Tulloch expresses no precise opinion; but in relating the progress of plague in the village of Marathea, in Corfu, he says:

"As soon as its existence was officially reported, measures were adopted for cutting off all communication between the infected districts and the capital; strong cordons of troops and police were posted round each village in which the disease had made its appearance; the inhabitants were shut up in their respective residences; no communication was permitted between them, and the approach to the capital was guarded by a double line of troops, through which no one was allowed to pass from the interior, without performing fourteen days' quarantine. Owing, it is supposed, to these precautions, the disease was confined principally to the upper and lower districts of Leftimo, in which it broke out, and the capital entirely escaped its ravages." (*Mediterranean Report*, p. 38, *a.*)

Our estimate of the great value of these documents has been so strongly expressed, and the correctness of this estimate has (we hope) been so amply confirmed by copious extracts from them, that we feel no doubt of all to whom they are accessible bestowing on the Reports themselves the attention they so amply merit. We consider medical science under deep and lasting obligations to Major Tulloch, to whom we think the greatest stickler for the honour of the medical profession may freely apply the self-glorying dictum of our neighbours,—changing a single word,—*s'il n'est pas* MEDECIN, *il doit bien l'être.*

ART. IV.

1. *Ueber Kuhpocken an Kühen.* Von E. HERING, Professor an der Königl. Thier-Arzneischule in Stuttgart, &c. *Mit einer colorirten Tafel.—Stuttgart*, 1839. 8vo, pp. 175.
On Cowpock in the Cow. By E. HERING, Professor at the Royal School of Veterinary Medicine, Stuttgard, &c. *With a coloured plate.—Stuttgard*, 1839.

2. *Praktische Abhandlung über die Wiedererzeugung der Schutzpockenlymphe durch Uebertragung derselben auf Rinder und andere impffähige Hausthiere.* Von Dr. CARL GOTTLOB PRINZ, Professor der praktischen Thierheilkunde, &c. zu Dresden. *Mit zwei buntgedruckten Kupfertafeln.—Dresden*, 1839. 4to, pp. 42.
A Practical Treatise on the Regeneration of the Protecting-pock (Vaccine) Lymph, by transferring it to Heifers and other domestic animals susceptible of inoculation. By Dr. CHARLES GOTTLOB PRINZ, Professor of Veterinary Medicine at the Royal Veterinary College, Dresden. *With two copperplate engravings.—Dresden*, 1839.

3. *Die Menschen- und Kuhpocken in ihrer Identität und Ruckbildung ersterer zur Vaccine, &c.* Von Dr. BASIL THIELE, Inspektor der Medicinalbehörde in Kasan, &c. &c. (Henke's Zeitschrift, Erstes Vierteljahrheft, 1839.)—*Erlangen*, 1839. 8vo, pp. 26.
On the Identity of Smallpox and Cowpox, and the Reproduction of the latter from the former, &c. By Dr. BASIL THIELE, Inspector of the Medical Board in Casan, &c. &c. (From Henke's Journal, No. I. 1839.)—*Erlangen*, 1839.

4. *Report of the Section appointed to enquire into the present State of Vaccination, read at the Anniversary Meeting of the Provincial Medical and Surgical Association, held at Liverpool, July 25, 1839.* —*Worcester*, 1839. pp. 98.

THE state of security into which, in this country and in many of the continental states, medical practitioners as well as the public had been lulled with respect to smallpox, had led, as is but too commonly the case under like circumstances, to the entire neglect or careless employment of the very measures by which that security was induced. It is only through the repeated warnings of the enemy at the door, and the reappearance of the dreaded pestilence, even within the very lines of our presumed defences, that we are at length aroused to a sense of the

dangers which threaten us, and to the study and investigation anew of those means of protection which have for a time availed us. The causes of the late renewed prevalence of smallpox are various; and, setting aside those cases in which no protective means whatever had been had recourse to, the number of instances of smallpox occurring, in which the operation of vaccination had been previously performed, is such as to demand a most anxious and searching investigation into the sources of failure. Among those which have been assigned there are three especially worthy of consideration: 1, a presumed failure in the protection afforded by the vaccine after an interval of longer or shorter duration, and a consequent renewal of the previous susceptibility to smallpox infection; 2, a presumed deterioration in the powers of the vaccine lymph, arising, it is thought, from repeated transmission through the human subject; 3, negligence or carelessness in the performance of the operation, whether in the selection of the lymph used, or in ascertaining the healthy state of the recipient and the subsequent progress of the vesicle. The gradual failure of the protective powers of the vaccine, after a certain period of time, is an opinion pretty generally entertained in several parts of the continent, and we have recently had occasion to consider this point at some length, in connexion with its remedy, revaccination (vol. vii., p. 186). The question of a deterioration of the lymph in use has of late engaged much attention, as well in France and in this country as in several of the German states; and endeavours have been made in consequence to discover anew the vaccine disease in the cow, and to introduce fresh lymph, as it is supposed of a more active quality, from this source. The third source of failure, that of carelessness or the neglect of due precautions in the performance of the operation, is the explanation urged by those who are the more immediate followers of Jenner, and, especially, in the Report of the Vaccination Section of the Provincial Association, drawn up by its chairman, Dr. Baron. Whether one or all of these causes may not have been in operation is a subject for grave and unbiassed enquiry, and we hail with great satisfaction the exertions making both in this country and abroad to develope the truth.

From the Report of the Provincial Association it would seem that, in this country at least, the actual amount of failure has been much exaggerated; the cases of smallpox occurring after vaccination being scarcely, upon the whole, more in number than those occurring after a previous attack of smallpox, and the resulting mortality so small in these partially protected cases, as to be of little or no account in the general mass: but at the same time we must observe that there is an apparent bias in the report which gives to it too much of the air of special pleading; and although we are not disposed to question the general accuracy* of the conclusions arrived at by the author from the materials

* It is with regret that we must here refer to a serious inaccuracy on this very question, already pointed out by Dr. Gregory in the Medical Gazette for October last (p. 130). Our own personal experience accords with the statement made in the report of the not infrequent occurrence of secondary smallpox, though certainly not to the extent therein stated. We cannot but lament, therefore, that the want of accuracy in the reference to Mr. Crosse (*Report*, p. 64), has been the occasion of throwing some degree of discredit over other statements in the same document, which we believe to be substantially correct. If Dr. Baron, who is so thoroughly versed in this subject in

placed in his hands, there is a want of precision in the summing up of the evidence, and of sufficient detail with respect to the individual questions circulated by the section, which render it, when considered as a statistical document, of less value than might have been expected. In other respects, however, information of the utmost interest and importance is brought before us, and more especially with reference to the actual nature of the vaccine, a subject worthy of every consideration, and to which we purpose to devote some attention in the course of the observations which we shall have to make upon the German works before us.

The treatise of Dr. Prinz is chiefly devoted to a consideration of the reproduction of the vaccine in the cow, and the means of effecting this object (retrovaccination), and of rendering the lymph thus generated applicable to the general purposes of vaccination. The work of Professor Hering, on the other hand, is intended to give a full account of the cowpock in the cow, and embraces the consideration of: I. The origin of the vaccine from other diseases. II. Retrovaccination. III. The spontaneous development of cowpock. IV. The influence of various external and internal conditions on the generation of the disease in the cow. V. The symptoms, process, &c. VI. The so-called spurious and anomalous cowpock. To these points of enquiry, taking the work of Professor Hering as our guide, we shall advert in succession.

I. *Origin of the Vaccine.* Four sources of the vaccine disease in the cow are mentioned by Prof. Hering: 1, the grease (Pferdemauke); 2, smallpox; 3, retrovaccination; and 4, spontaneous development. The last two of these we reserve for separate consideration, confining our attention at present to the presumed origin of the vaccine in the cow, first from the grease, or some other cutaneous disease in the horse, and, secondly, from the smallpox, as it occurs in man. The question, it should be observed, is perhaps the most important that can be entertained on the subject, as it involves the actual nature of the disease, its presumed identity with smallpox, and consequently the consideration of whether in its operation upon man, as a protective means, the action of the vaccine is that of an antagonizing power, more or less incompatible with the existence of variola, or a milder and safer modification of the same power, by which the tendency to the reception of variola is exhausted, in the same manner as by the introduction of the more severe and exquisite form of the disease.

It is unnecessary to enter into any lengthened discussion upon the origin of the cowpock from the grease, and more especially, since much confusion would seem to exist among continental writers as to the precise meaning of the term. Whether Professor Hering is correct in referring it to the disease to which he alludes under the name of *Pferdemauke*, we must leave to the veterinary surgeon to determine. It is now acknowledged that Jenner was mistaken in the name of the equine disease, to which he referred the origin of cowpock, though not in the

all its branches, should have been led into this serious lapse of memory, it need not perhaps be a matter of surprise that other members of the section may have likewise been deceived; but we believe that the majority of its members never saw the report until it was in print, their duties being to obtain and transmit information to the secretaries, for the use of the chairman.

circumstance that the horse is subject to a vesicular eruption about the heels (but also found in other parts of the limbs and body), identical in its nature with the vaccine, and capable of generating the genuine vaccine vesicle by inoculation upon man or upon the cow. "It is ascertained," says Dr. Baron, "that the horse is liable to a vesicular disease of a variolous nature as well as the cow; and that lymph taken from the horse and inserted into man will produce an affection in all respects like that derived from the cow, and equally protective. The error consisted in believing that this affection was *the grease*, and that it required to be transmitted through the cow to give it efficacy. A misapprehension of this kind may well be excused in the infancy of so complicated an investigation; the disease appearing for the most part on the thin skin of the heels of the horse, and the traditions among the farriers in the country, leading to the mistake. We now know that the vesicle may appear on other parts of the animal's body; and that the horse as well as the cow has, in different ages and in different countries, suffered both from the mild and malignant *variolæ*." (*Vaccine Report*, p. 16.)

The *Pferdemauke* is described as consisting in an erysipelas of the skin of the ball and pastern joint, which not unfrequently extends also to the back of the metatarsus or metacarpus, and at first forms small vesicles, by the bursting of which an acrid lymph of peculiar smell escapes. These vesicles, through neglect, want of cleanliness, and improper treatment, readily pass into tetter-like fissures (the chronic form of the disease), difficult to heal, and finally giving rise to various degenerations of the skin and subjacent cellular tissue. (*Hering*, p. 5.) The disease, however, is elsewhere described as affecting the thighs and other parts. Several instances of the inoculation of cows with the matter derived from the vesicles of this equine disease, in its primary stage, are given, in which the vaccine eruption was generated.

The origin of the cowpock from smallpox is a question of far greater moment, and deserves the closest and most sifting investigation. "According to theory," says Professor Hering, "the identity of these two diseases is much more probable than that of the equine disease (*Mauke*) and the smallpox; but," he adds, "experience has not established the expectation." The opinion of the identity of the vaccine with variola, as is well known, was entertained by Jenner, who gave to the former affection the name of variolæ vaccinæ, and has since been strongly advocated by Wedekind, Dr. Baron, and others. This opinion, however, until lately, rested upon very insufficient grounds; and numerous unsuccessful attempts having been made to inoculate the cow and other animals with smallpox by Sacco, Ring, Dalton, and others, it might be considered, as far as negative evidence could decide, as entitled to but little weight. Dr. Baron it is true, in his Life of Jenner, devotes a considerable portion of the first volume to an attempt at establishing the identity of smallpox and the vaccine; but we think that a careful examination of his arguments, and of the historical details upon which they are founded, do no more than show that cattle have at various times been attacked with an eruptive disease, possessing all the characteristics of variola, the question of the identity of this affection with the vaccine resting still upon the grounds of analogy and presumption,

rather than upon experimental proof, actual observation, or historical evidence. Dr. Sonderland, of Bremen, was, as far as we know, the first to point out a method of inducing the vaccine in the cow by the direct application of the smallpox virus. The object was effected by means of a woollen bed-cover, removed from the bed of a patient who had died in the suppurative stage of smallpox, and wrapped round the body of the animal. The account of Dr. Sonderland's experiment will be found in Hufeland's Journal for January, 1831 ; but the fact became disputed in consequence of repeated attempts subsequently made elsewhere to generate the vaccine in this manner having invariably failed. At Stockholm and Utrecht, however, a pustular eruption was observed to follow the application of the infected woollen cover on those parts of the body of the animal in immediate contact with it. This eruption, as observed at the former of these places, quickly dried up; at the latter, according to Numan, who watched the progress of the experiment, the vesicles appeared on the sixth day, and by the tenth had reached their full development, and contained a tolerably clear lymph, and then became incrusted with a brownish scab, which fell off in the course of two days. The affection was almost purely local, being attended with scarcely any constitutional disturbance. Four children were inoculated with the lymph with some appearance of success, but the result was not cowpock.

The results of attempts made to inoculate the cow with variolous matter, though contradictory, have proved, in some instances at least, more satisfactory. Dr. Gassner is said to have inoculated several cows with smallpox lymph, and in eleven of these cowpock was produced, from which four children were inoculated, with the effect of producing, not any modification of smallpox, but the genuine vaccine. But the most important of the results hitherto obtained respecting this question, are those elicited by the investigations of Dr. Basil Thiele, of Kasan, in South Russia, and Mr. Ceeley, of Aylesbury. Those of Dr. Thiele, although obtained some years since, were first published in January, 1839, by Dr. Henke, of Erlangen, in his excellent journal.

It appears that Dr. Thiele, after some fruitless attempts to induce the variolation of the cow, according to Dr. Sonderland's method, the failure of which is attributed to the experiments having been performed in the winter season, and without due regard to keeping up the temperature of the stall, in the spring of the year 1836 caused a cow to be inoculated with smallpox matter. The operation was performed by the assistant-physician, Fomin, and succeeded ; several children were inoculated with the matter thus generated in the cow, and the lymph was transmitted to Dr. Thiele, in whose presence other children were inoculated. The progress and external characters of the eruption which ensued were altogether similar to the genuine vaccine, though accompanied with more severe constitutional symptoms. The matter thus obtained, at the time of Dr. Thiele's communication, had passed through seventy-five successive inoculations (*Impfgenerationen*), and had been employed in upwards of 3000 individuals. Dr. Thiele repeated the experiment, more immediately under his own personal inspection, in the spring of the year 1838, with similar success. The following directions are given as to the methods of procedure necessary to be followed, and

are the more valuable, since by neglect of one or other of the precautions recommended, numerous experimental researches of the same description previously entered into were rendered abortive. 1. The cow should be a recent milker, between four and six years old, and should have a white udder, to admit of the pock being easily and clearly seen. 2. It must be kept in a warm stable (at about 15° Reaumur, = 66° F.), fed with its customary food, and regularly milked. 3. The hair must be shaved off, and the posterior part of the udder chosen for the insertion of the matter, that the animal may not be able to lick the spot. The incisions are to be made as in inoculating with cowpock, but deeper, and the part is to be bound up. 4. The lymph used may be taken either immediately from the pustule or from points charged not longer than from ten to twenty days previously; the pustules from which it has been procured must be clear, transparent, and of a pearly aspect, and the lymph itself limpid. "The greater or less malignity of the epidemic," says Dr. Thiele, "and of the individual case from which the lymph is taken, has no essential influence upon the vaccine generated; for in a case in which the smallpox eruption was confluent, became black, and the child died, a perfectly genuine vaccine was generated by the transmission." (p. 17.) With respect to the general and local symptoms in the inoculated cow, it is stated that, on the third day, a hardness was remarked in the cellular tissue of the udder; and on the fifth, a pustule resembling the vaccine was formed, which from the seventh to the ninth day contained a clear lymph, and had a central depression or pit; from the ninth to the eleventh day it began to dry up, a crust being formed, which, on falling off, left a small smooth cicatrix. From three to six incisions usually gave rise to only one or two pocks. Acceleration of the pulse and increased heat were noticed between the fourth and seventh days, but the general health and appetite were but little disturbed. The pocks generated in children from this lymph presented an appearance perfectly similar to the genuine vaccine, differing only in being somewhat more intense in the first inoculations.

These researches of Dr. Thiele do not however satisfy Professor Hering, who considers the identity of smallpox and the vaccine as not yet proved. He appears to attach far too much weight to the negative evidence derived from the repeated failures, although all such evidence ought to yield to one well-authenticated and unexceptionable fact. It is with much satisfaction that we are enabled to refer to the Report of the Vaccination Section for the confirmation and further extension of these researches by those of Mr. Ceeley, of Aylesbury.

"On the first of February, 1839, he (Mr. Ceeley) inoculated with smallpox matter (*variola discreta*), of the seventh or eighth day, three young heifers; a fourth was at the same time vaccinated. We shall not mention all the particulars connected with these examples, but select the first as an illustration of the admirable skill and judgment with which the process was conducted. Mr. Ceeley made seven punctures and introduced fourteen points near the left *labium pudendi*, and on the same day inserted two setons with matter from the same subject. On the ninth day after this process, he vaccinated the same animal on the right *labium pudendi*, with fifth, sixth, and seventh days' lymph from a child, in seven punctures with fourteen points; and below the pudendum in four punctures with eight points. On the tenth day after the insertion of the variolous matter, one of the punctures near the posterior margin of the left *labium pudendi* had assumed the form of the natural vaccine vesicle. By gently removing

the central irregular crust, and carefully puncturing the cuticle, he was able, in the course of an hour, to charge thirty-eight points with lymph, and on the same and subsequent days to use part of it on children and adults. On the thirteenth day the smallpox vesicle was more inflamed and florid; this was the fifth day after the insertion of vaccine lymph, at which time all the eleven punctures were converted into effectual vesicles; from these he took fine clear lymph, and used it on children and adults. Both the variolous and vaccine vesicles subsequently ran nearly a parallel course; so that on the twenty-sixth day of the former, and the seventeenth day of the latter, the scars of both appeared perfectly similar. To obviate objections which might arise from the insertion of the vaccine lymph on the ninth day after the inoculation with the variolous matter, Mr. Ceeley reinoculated a sturk on the 15th of February with smallpox matter, of the seventh or eighth day, on the *labium pudendi.* He made eight punctures, which were deluged with the variolous fluid from capillary tubes. On the fifth day the four upper punctures were enlarged and elevated, the other four were less so. On the sixth day all presented the appearance of the vaccine vesicle. From one of them he took lymph with difficulty, and scantily charged thirty-nine points. On the eighth day he again took lymph from the vesicle opened on the sixth. On the ninth day the vesicles were enlarging, and he again opened carefully the first vesicle and charged twenty points. On the tenth day the four lower vesicles were increasing, and from them he charged twenty-seven points. After this time the brown crusts appeared, and the disease gradually declined. The animal was subsequently inoculated both with variolous and vaccine matter, but no result followed." (*Vac. Report*, pp. 26-8.)

Mr. Ceeley's experiments were conducted in the presence of five medical men and one veterinary surgeon; and he has twice succeeded in accomplishing his object after many previous fruitless trials. Portions of the lymph were transmitted to Dr. Baron, and the vesicles produced from it in every respect corresponded with those delineated by Jenner. "The correctness of the vesicle formed by it," says the report, "exhibits a marked contrast to that which we have seen produced by other virus now in use; and we fear that the local as well as general disturbance occasioned by the latter, so far from being a source of protection, will be found to be the reverse." (p. 26.) We have only to observe that these experiments, as far as we are able to judge of them from the accounts before us, appear to be highly satisfactory and conclusive; and we look upon the investigations of Dr. Thiele and Mr. Ceeley as the most important in their bearings upon the general subject of the vaccine which have been developed since the original researches of Jenner himself.

II. *Retrovaccination.*—The production of cowpock in the cow by inoculating with vaccine lymph taken from the human subject, has been had recourse to with different views at different periods since the first introduction of the vaccine. The essay of Dr. Prinz, as we have before remarked, is devoted exclusively to the consideration of this subject, and is divided into two parts:—the first comprising a general investigation respecting the regeneration of the cowpock lymph, the advantages to be derived by vaccination from the practice, with an historical account of its introduction; the second pointing out the means of generating the vaccine disease in the cow by inoculation from man, and the measures necessary to be adopted in the removal, preservation, and application of the lymph thus generated for the purposes of further vaccination.

The presumed advantages to be derived from the reproduction of the cowpock, by transference of the vaccine from man to the cow, are stated

to be :—1, That it affords a means of proving the genuineness and efficiency of the lymph employed; 2, of increasing the supply of vaccine matter; 3, of keeping up the supply for future use; and 4, a method whereby the cowpock lymph may be renewed, whenever the same may become necessary. The uncertainty of success which has hitherto attended attempts at retrovaccination, together with the consequent expenditure of time and labour, are impediments to the practice upon any but the last of these grounds. With respect to the first, it may be stated that we have more practicable and equally sure modes of testing the efficiency of the vaccine lymph, where it is thought necessary, by revaccination, or by inoculation with smallpox matter; and, although the third of these presumed advantages has found an advocate in Numan, who assures us that he had succeeded in preserving the cowpock lymph, by inoculation from cow to cow, for sixteen weeks, we are informed by others that the success of the practice cannot be depended on beyond the fourth transplantation, and that, consequently, as a means of increasing or keeping up the supply of the vaccine, it must prove nugatory. There remains therefore for consideration only the supposed restoration of the vaccine lymph to its original purity and efficiency by the retransference of the lymph to the cow, and the consequent retransmission of the disease through the animal from which it was, in this form at least, originally derived. Dr. Prinz contends for the practice of retrovaccination upon this ground alone, and considers that the lymph thus attains renewed efficiency as a preservative against smallpox. He seems, with many other authorities at the present day, to be impressed with the opinion of a gradual deterioration of the lymph in use, and, although he does not consider it to have yet entirely lost its efficiency, he suggests the possibility that the powers of the lymph may become still further impaired, the vesicle produced gradually diminishing in size after repeated vaccinations, and the protection afforded to the constitution becoming less. This deterioration and loss of power in the lymph would seem, under certain circumstances, actually to occur; thus Meyer, among others, perceived that the vesicles produced from his original stock of lymph became, from year to year, smaller, affording an impoverished and less copious supply of lymph, the vaccination from which was not always successful; but having at length succeeded in procuring fresh matter from the cow, the resulting vesicles were large and turgid with lymph, and left a cicatrix perfectly resembling that of the original cowpock inoculations, while the vaccination succeeded in all cases, or at least with very rare exceptions. (*Prinz*, p. 9.)

The advantages of retrovaccination, however, are not to be estimated by those resulting from a recurrence to the cow for original or primary cowpock lymph, and, as it appears to us, Dr. Prinz and others of our continental brethren are reasoning and drawing inferences upon erroneous grounds, when they place the primary vaccine, no matter how generated, and the retrovaccine upon the same footing. Granting that the vaccine lymph in its repeated transmission through the human body has lost power, or acquired certain deteriorations impairing its original efficiency, it by no means follows that by the transference of this deteriorated or impoverished lymph to the cow, it should thereby become purified and increased in power, so as to resemble primary lymph. If also, as the experiments of Dr. Basil Thiele, confirmed and extended as they are by

those of Mr. Ceeley, tend to show, the vaccine is in reality nothing more than a mild form of variola, and the lymph derived from the primary vaccine pustule in the cow generates a form of variolous disease in man, which protects, not by a presumed antagonism, but by its identity of action upon the constitution, a renewed transmission of the virus through the cow should, if it have any effect at all, tend to render the vaccine still milder in its operation, while the impurities presumed to be derived from the repeated transmissions of the lymph through the human subject, would probably only produce further modifications in the disease as it affects the cow, and become complicated with others derived from constitutional or acquired peculiarities in that animal. In order to establish any claim to our notice for this operation of retrovaccination, it must be shown, first, that the repeated transmission of the vaccine through the human body is actually and necessarily attended with a modification of its properties and a corresponding loss of its efficiency; and, secondly, that the retransmission of the humanized or deteriorated lymph through the cow is capable of removing the acquired impurities and renovating the weakened powers. The first of these propositions there may be some grounds to entertain ; but we would remark that hitherto neither of them, and least of all the last, has been made the subject of proof. The possibility of the vaccine becoming milder by repeated inoculation may be inferred from an analogous amelioration which is known to occur, as Professor Hering observes, in two other nearly allied forms of eruptive disease, smallpox and chickenpock. It would also seem to acquire some quality in its transmission by which it becomes assimilated to the human constitution, since the difficulty of inducing successful vaccination with primary cowpock lymph is much greater than with lymph which has already been passed a few times through the human body, though the constitutional and local effects of the primary lymph, where it succeeds, would seem to be more marked and severe. That a further reduction of its power or alteration of its nature takes place, where the necessary precautions have been used, does not however appear so decided : for although the cases of failure reported are, from whatever cause, numerous, still it must be borne in mind, that the lymph now in use in this country has passed, not through three or four, not through hundreds of individuals, but through thousands, and tens of thousands, while yet we hear of skilful and careful vaccinators expressing themselves, like Mr. Dodd, of Chichester, who states "that he has *not seen a single instance of smallpox in a patient whom he had vaccinated, though he had resided at Chichester ten years.*" (*Vaccination Report*, p. 48.) That the vaccine however may, through carelessness or want of skill, become deteriorated, does not admit of question. The salutary cautions, so earnestly enforced by Jenner, of attending to the state of the skin of those submitted to vaccination, more especially with respect to herpetic eruptions, bear closely on this point. The modifying powers of itch, whether in man or in other animals, are equally insisted upon by Professor Hering, yet from the work of Dr. Heim, noticed in our Seventh Volume, there seems to be a laxity of opinion in regard to this point which, if carried into practice, may possibly go far to explain many of the failures among continental vaccinators.

The question, however, still remains as to the efficacy of retrovaccination in correcting these evils. As it appears to us, the practice, until

further experience has shown to the contrary, must be held of very doubtful utility; while under any circumstances the variolation of the cow, performed according to the methods so skilfully devised and clearly pointed out by Dr. Thiele and Mr. Ceeley, would seem to afford a preferable mode of obtaining lymph of renewed activity and power. Professor Hering thus expresses himself respecting retrovaccination:

"Opinions differ as to the value of the vaccine thus renewed; some at once admit the cowpock generated by means of retrovaccination to be of the same value as that primarily developed in the cow; others think that there is no difference in efficiency between this renovated vaccine and that hitherto in use, while Ritter even considers the inoculation of the cow from vaccinated children as a doubtful proceeding, since, according to his experience up to this time, lymph so obtained does not equal in activity that of spontaneous origin, and at the same time we are exposed to the danger of obtaining modified pocks in the cow, and in this manner disturbing the purity of the original source. If the vaccine during its transmission through so many individuals, who may be variously affected with the germs of cachexy, scrofula, or other diseases, shall really have lost in intensity or protective power, it remains to be proved whether, by a single transmission through a cow, it shall be again so far purified and strengthened as to be capable of being placed on a par with primary cowpock. Where, however, this last (primary cowpock) is not met with, and the vaccine hitherto in use shows a falling off of its essential properties, I hold a renovation of the same by means of retrovaccination to be advisable, and by no means participate in the fear of thereby giving rise to spurious pocks." (p. 23.)

Retrovaccination has been practised, as we are informed by Dr. Prinz, in his historical account, first, with the purely scientific aim of establishing the fact of the reproduction of the vaccine in the cow, as by Woodville, Coleman, Valentin, Frank, and other of the earlier cultivators and advocates of cowpock inoculation; secondly, with the view of keeping up and increasing the supply of lymph, as by Junker, Sacco, Magliari, and others; and, lastly, in later times, by many of the German inoculators especially, in the hope of renovating the powers of the vaccine. Very full directions are given by Dr. Prinz for the performance of the operation, and the precautions necessary to ensure success are carefully pointed out. These relate to the selection of the animal, its age, general state of health, condition of skin, &c., to the characters of the lymph employed, to the time of year, &c. Yearling cow-calves, young heifers, and young cows in calf three or four years old, with well-developed udders, the skin of which should be, if possible, without hair and colourless, are recommended as the fittest subjects for the operation. (*Prinz*, p. 31.) The lymph should be carefully selected from well-developed vaccine vesicles, and transferred immediately from the arm of a healthy child to the animal, and the most fitting season is from the commencement of the month of March to the end of June. The best mode of performing the operation seems to be by making longitudinal incisions, either upon the udder or on the teats, from a third to half an inch in length, the skin being previously placed upon the stretch with the left hand of the operator, and of such a depth that the edges of the wound may gape a little; the lymph is then carefully applied over the whole surface of the wound, and the animal properly secured. It is not necessary to enter into any account respecting the subsequent removal of the lymph, which may be generated in the pustules, and its preservation for

use, for which full directions are given by Dr. Prinz, as these will readily suggest themselves to every experienced vaccinator, and by none other can the operation be attempted.

III. *Spontaneous Development of Cowpock.* Professor Hering remarks, in relation to the presumed connexion of the genuine cowpock with the *Pferdemauke* or grease, or rather with the peculiar eruptive disease in the horse, which was at one time mistaken for that affection, that this connexion is, on the continent, of very rare occurrence, and that almost all the instances of cowpock observed in the cow have arisen independently of any such affection. After the discovery of Jenner was promulgated, it was found that the vaccine disease not only showed itself in many places on the continent, but that also its occurrence among cows had been before noticed, and even that its protective powers against smallpox were known, although no practical inference had ever been drawn from the fact. In the Göttingen *Allgemeinen Unterhaltungen* for the 24th of May, 1769, it is said that the disease was not infrequent around Göttingen, that it attacked the milkers, and that such persons were considered to be protected against the smallpox. (*Hering*, p. 24.) Professor Hering quotes from the chronicles of Bishop Marius, of Lausanne, who lived in the sixth century, a passage which he thinks refers to cowpock: "*Anno 570. Hoc anno morbus validus cum profluvio ventris et variola Italiam, Galliamque valde afflixit. Et animalia bubula per ea loca maxime interierunt.*" However this may be, not long after the original discovery of the cowpock in Gloucestershire by Jenner, the vaccine disease was also observed in many of the southern and midland counties of England; in Ireland it is known to have occurred in the county of Cork, while at various times it has been noticed in most of the continental states of Europe. In the year 1800, Sacco saw the disease in the north of Italy, in cows which had been brought from Switzerland into the plains of Lombardy. In the year 1812 primary cowpock occurred in many places in northern Germany, and was observed by Dr. Bremer at Berlin, and by Dr. Fischer in Lunenburg. The disease has also been noticed, both before and subsequently to this last date, in Sleswich and Holstein, in Brunswick, Wirtemberg, Holland, Switzerland, Piedmont, Rome, and lastly in France. In May, 1812, Dr. Bremer, as we have stated, observed the disease among the cows in the neighbourhood of Berlin, and found the teats covered with brown horny crusts; he could discover clear lymph in two only of the vesicles submitted to his inspection. Two girls also had become infected with the disease. Dr. Bremer vaccinated twelve children with complete success from one cow and from one of the girls, and continued the inoculations with the regenerated lymph. Dr. Fischer, of Lunenburg, describes the cowpock which he observed in the cow in April and May of the same year as follows:

"The eruption showed itself on the teats of milking cows as small, blueish, or more correctly blackish-brown, glistening pustules, which, even from the commencement, were hard to the touch and covered with a thick skin, but about the eighth or ninth day became incrusted with a scab gradually increasing in thickness and hardness. These hard vesicles, which felt like small hazel nuts deeply seated in the skin, increased so much in from four to six days, that the largest of them, if isolated, attained the size of a *Sechsers* or a

Groschen (about that of a sixpence) in circumference. Their figure was round or oval; their contents at the commencement clear and resembling lymph, becoming more consistent at a later period. The cows suffered more or less severely (?); they were very sensitive during milking, but showed no constitutional disturbance." (*Hering*, p. 26.)

In Sleswich and Holstein, scarcely a year passes without the occurrence of cowpock among the cows. According to the report of Professor Ritter it becomes epidemic among the large herds of cattle, in a manner similar to the smallpox in man. It was however with difficulty, notwithstanding frequent opportunities of seeing the disease in the cow, that he could observe it in its perfect form, sometimes from receiving the information of the outbreak of the eruption too late, sometimes from the pustules having been ruptured in milking and degenerating into ulcers, or from their having been subjected to local treatment, by which the character of the eruption was destroyed. He thus often saw large herds of cows affected with the disease, without being able to find one from which he could obtain clear lymph. Inoculations with primary lymph were, however, successfully accomplished in children, in the years 1824, 26, 29, 30, and 32, which were accompanied by local effects of more severity than usual, but without any danger or injurious consequences. In March, 1836, a woman at Passy, in the vicinity of Paris, became infected from a cow with an eruption on the hands and face, which was recognized by M. Bousquet as the vaccine. In the same year genuine cowpock in the cow is said to have been observed also at Amiens and Rambouillet. The investigations carried on by the Académie Royale de Médecine, in connexion with the occurrence of the cowpock at Passy, and the circumstances attending the recent discovery and introduction of primary vaccine lymph, by M. Estlin, are already sufficiently known to our readers.

The instances of cowpock in the cow which have come under the notice of Professor Hering, in Wirtemberg, either personally, or through the medium of the official reports, are very numerous, and are classified by him as follows: 1. Instances of genuine cowpock in the cow, with successful transference of the same to man by inoculation. 2. Instances of probably genuine cowpock, the inoculation from which either failed or was not attempted. 3. Cases of accidental infection of man from primary cowpock. Of the first class, it appears that within the ten years from 1827 to 1837, there were sixty-nine instances of the occurrence of genuine cow-pock, in which eighty-four head of cattle are reported as being affected. From these 126 children, besides one young female, were inoculated with success, in thirty-six of whom, however, the further inoculation failed. In addition to these there were twenty-two successful and two unsuccessful inoculations, in which the precise number of children operated upon is not given. Of the second class, in which the inoculation either failed, or was for various reasons omitted to be performed, there were 152 instances, the number of cows attacked amounting to 208. In 107 of these the inoculation was attempted but failed; in forty-five the reporters were either unable to obtain lymph, on account of the pustules having proceeded too far, or abstained from the inoculation on account of the characters of the eruption varying, more or less, from the descriptions given by Jenner and Sacco. The

instances of the third class reported were twelve in number, in addition to three others included among those belonging to the first class. In the able summary which the author gives of these cases, as well as in the more general notice of the instances of cowpock in other countries, which he refers to as being of spontaneous development, he has frequently pointed out the entire want of connexion with the *Pferdemauke*, or any other disease in the horse. We must, however, observe, that he has strangely overlooked the more important question, of ascertaining whether there may not have been traces of a connexion between smallpox in man and the occurrence of these instances of cowpock in the cattle of the respective localities in which it has been observed to occur.

IV. *Influence of various External and Internal Conditions on the Production of the Disease in the Cow.* From a careful collating of the cases of original cowpock belonging to the first and second classes of the special reports, Prof. Hering deduces some valuable results.

1. The influence exercised by the geognostical characters of the soil and elevation of surface, as far as these conditions extend within the limits to which the reports refer, appears to be null. The geological formations chiefly occurring, we may observe, are the Muschelkalk, Lias, and Jura limestones, and the Bunter and Keuper sandstones, both belonging to our new red sandstone series. With respect to elevation, it has been thought that from the prevalence of cowpock in Holstein and Sleswich, and other situations of like character, that it is a disease of the lower lands, an opinion to which the greater number of cases observed in the Neckarkreis, an extent of country, for the most part lying lower than the other three departments of the Danube, the Jagst and the Schwarzwald, (see *British and Foreign Medical Review*, Vol. VII., p. 187,) lends some countenance; but upon closer examination of the district reports, this conclusion is not borne out, since it is not the lower districts of the Neckarkreis, but those the situation of which is considerably elevated which afford the greater number of cases of cowpock in the cow. The author concludes, therefore, "that the more or less frequent occurrence of primary cowpock does not depend upon the geological formation, and that the disease is not more prevalent in the lower-situated and warmer districts; at the same time it is also not infrequent upon the mountains and in the most uncultivated (*rauhesten*) parts of the country." (p. 106.)

2. The author next considers the effect of in-door or out-door foddering, the latter of which is most usual in the north of Germany, and in our own country, even throughout the winter. The case would seem to be different in Wirtemberg, where, in consequence of the severity of the winters, the cattle are only kept out of the stall in summer and autumn; while, in many thickly-populated districts, the practice of out-door feeding has been long discontinued altogether. Many reasons, which it is unnecessary for us here to consider, have contributed to this practice of stall-feeding, and in the vineyard countries especially (the valley of the Neckar and several lateral valleys,) the cows only leave the stall for the purpose of being led to drink. It is in these places, says Professor Hering, that the greatest number of cowpock cases have been observed, while, in others, in which a greater number of cows are kept, which are out in the more favorable seasons of the year, hitherto only a

few cases have been known. The stall fodder consists usually in summer of grass and clover, but among the cattle which are fed with the refuse of the breweries and distilleries, the eruption also occurs, as Professor Hering has himself observed; and he remarks that the cows of the great milk establishments in London, among which Woodville and Pearson had observed the disease, are fed especially with grains from the great breweries. A change of fodder from dry to green has already been assigned by Jenner as a cause of eruptions on the udder. A dry, and, usually towards the end of winter, sparing supply of fodder, observes the author, naturally tends to diminish the secretion of milk; if now in the spring a greater quantity of better and more succulent food is quickly obtained, there is a determination of fluids to the udder, and this it is chiefly which gives rise to the eruption. This peculiar state of the udder is further brought about by the practices of fraudulent dealers, who abstain from milking cows brought into the market with the express purpose of inducing distention of the udder, while the change of fodder, consequent upon the transfer of the animal to another owner, assists, as the author thinks, in the production of the eruptive disease. (p. 108.) Jenner, however, was of opinion that the eruptions thus generated were not genuine cowpock; for, according to his observations, the milkers who became infected from pustules so induced, were still susceptible of the attack of smallpox. With regard to the presumed effects of stall-foddering in the Neckarkreis, the author remarks that in that department, in consequence of the number of small proprietors who themselves attend to the cows, and to whom the government premium offered for the detection of primary cowpock is an object, the instances of eruptive disease in the cattle are readily brought forward, whereas in the Donaukreis, where the cattle owners are possessed of a greater number of cows, and intrust their herds to the care of children and labourers, the disease is not only less likely to come under their notice, but the reward offered for its detection is a matter of too little moment to induce them to make known its existence.

3. The following table exhibits the number of instances of genuine, and probably-genuine cowpock in the cow, reported from the year 1825 to 1837, inclusive:

Year.	Cases of Genuine Cowpock.	Probably-Genuine Cases.
1825	0	1
1827	2	3
1828	1	2
1829	14	24
1830	5	26
1831	7	24
1832	6	12
1833	5	9
1834	6	12
1835	7	12
1836	8	17
1837	8	10

Professor Hering attributes the apparent prevalence of cowpock, in the years 1829-30 and 31, to the circumstance of the public mind having

been, in the first of these years, then only fully awakened to its existence, and instructed in its nature; and to the effect of the government premium in bringing these cases into notice. Subsequent to this period it will be observed, that the annual number of cases reported of each class has fluctuated but little, though considerably less in amount than those of the years specified. Whether, in the years alluded to, there was any epidemic influence in action appears doubtful, as it is not improbable that, after the first impression among the proprietors of the cattle had subsided, many instances of the eruption might occur without being brought before the notice of the district reporters.

4. The effect of season is shown by the subjoined table, which proves that the spring and commencement of summer, as Jenner had before remarked, are the most favorable to the development of the disease.

Months.	Genuine Cases.	Cases presumed Genuine.
January	2	5
February	3	5
March	5	11
April	6	21
May	18	23
June	13	18
July	6	9
August	5	13
September	2	20
October	2	10
November	2	7
December	5	10

The average monthly number is 5·75 of the first class of cases, and 12·66 of the second, or, taking both together, 18·4; consequently, the months of April, May, June, and September are those which are in excess of this average, while January and February are greatly below it. Were a similar result to obtain over a sufficiently extended number of instances, it might be inferred that temperature exerted a very decided influence upon the prevalence of the disease, the cold of the winter and the heat of the middle of summer being both opposed to its spread.

5. The effect of age upon the frequency of cowpock is very difficult to estimate with certainty; and from various causes it is not always possible to ascertain with precision the facts necessary to establish it. Hitherto it has been thought that cows calving for the first time were the most readily affected by the disease, and consequently also the best fitted for experiments upon inoculation. The results of such of the Wirtemberg reports as give the age of the cows, which include about one third of the number of instances observed, would seem rather to militate against this opinion. Of 108 cows affected, 3 were two years old; 26 between two and a half and three years (*primiparæ*); 13, four years; 17, five years; 24, six years; 13, seven years; and 12, from eight to ten years: but it is probable that some included within the later periods were more advanced in age than is stated in the reports. The period of milking would seem to be that during which the cows are most subject to cowpock on the udder; and it is obvious that the same cause which has been before alluded to as predisposing to attacks of eruptive disease, namely, a de-

termination of blood to the udder, must operate here. Woodville and Ritter both assert that cows not giving milk are not liable to cowpock, and the Wirtemberg reports show that the eruption appeared, in a majority of instances, from four to six weeks to three months from the time of calving, but that instances were not infrequent in which old milkers, that is, from four to nine months from the time of calving, were also attacked. Exceptions however are mentioned to the general rule of milking cows only being liable; a cow in calf for the first time was attacked with genuine cowpock, probably however by infection from an older cow which had previously had the disease; another case occurred in a cow not milking, in which the disease is thought to have been spontaneously developed, and another in a two-year old, from which children were vaccinated with success. Upon the average, about two thirds of the affected cows, that is, those in which the time of milking is specified, were in the first period of milking, or from the time of calving to the third month inclusive; the remaining third were old milkers.

It will be observed that, in preparing the foregoing abstract, we have restricted ourselves to the matter of our text. It is, however, obvious, that if we admit even the *possibility* of the derivation of the disease in the cow from human smallpox, (and we presume that much more than this must be now admitted,) the admission not merely greatly diminishes the value of the statistical details given, but introduces a new element into the proposition which alters the whole aspect of the case. All future enquirers into the statistics of cowpox in the cow must make the contemporaneous prevalence or non-prevalence of smallpox in man a principal subject of attention.

v. *Symptoms. Progress of the Disease, &c.* Genuine cowpock in the cow is thought by some authors to be attended by constitutional symptoms of some severity, so that it has been attempted to draw a diagnosis between the genuine and spurious eruptions by the presence or absence of febrile and other constitutional symptoms. The distinction is not however found to hold good, since spurious eruptions are frequently found to be attended with constitutional symptoms, and the genuine cowpock, as Jenner himself observed, may be attended with little or no disturbance. The opinion however would seem to prevail so strongly in Wirtemberg that, in many cases of primary cowpock, the physicians state that, from the want of fever and of general symptoms, or from some irregularity in point of time in the outbreak of the eruption, they have been induced to consider the disease as spurious, and consequently to refrain from experimental trials of inoculation therewith. At the same time it is to be remarked, that in several cases of inoculation from cowpock eruptions, not agreeing in these respects with Sacco's description, successful results have been obtained. In many of the reports nothing positive is given upon this point, but in seventeen cases of the first class, (namely, that in which the genuine nature of the disease was confirmed by subsequent inoculation, and the production of correct vaccine vesicles,) it is stated that the fever was wanting, while it is expressly stated to have been present in twenty-four cases only, and is commonly described as slight. Loss of appetite was remarked in thirty-six cases. Among the instances belonging to the second class (that in

which the attempts at inoculation failed), there were only fifteen in which fever was observed to be established; in nine cases it was doubtful, and in thirty is said to have been altogether wanting. Rumination without food (*Wiederkauen bei leerem Maul*) was observed in only six of the cases; dimness of the eyes, restlessness, and other symptoms mentioned by Sacco, were as rarely, or even more rarely noticed. The most frequent constitutional symptom mentioned is a diminished and sometimes vitiated secretion of milk; but even genuine cases are reported in which the secretion of milk had been in nowise disturbed. Little dependence, however, as Professor Hering remarks, is to be placed upon the reports in this respect, since in many cases the information given could only be derived from the cow-proprietors, or their servants, the accuracy of whose testimony the temptation of the offered premium tends much to bring into question. The author concludes therefore that, since fever, loss of appetite, diminished secretion of milk, &c., are not always present, but, on the contrary, are often altogether wanting in the genuine cowpock, the existence of constitutional disturbance can no longer be received as a criterion of the genuine character of the eruption.

To come now to the local symptoms: the eruption, according to the Wirtemberg reports, was found, in by far the greater number of cases, to be seated on the teats, but in several instances on the udder also; in a few it was confined to the udder alone. This differs somewhat from the opinion of Jenner, who mentions only the teats as the seat of the genuine pustules, and still more from the assertion of Ritter, "that the pustules never break out on the udder, but always on the teats only, the papulæ which appear on the udder forming no cellular pustules, but only vesicles of a whitish or yellowish colour, which empty themselves entirely on being punctured." The number of the pustules was commonly of little importance, with the exception of a few instances in which from twenty to thirty pocks are mentioned as having occurred. The severity of the local inflammation, and the consequent sensibility of the part, were pretty generally found to vary with the greater or smaller number of the pustules. The size of the pocks is most frequently described as equalling that of a pea; sometimes it is compared to that of a lentil, or vetch. In a few cases, the pustules are said to have been as large as a horse-bean, a kreuzer, pfenning, or groschen, (that is, about equal in circumference to a sixpence,) and in one case as large as a hazel-nut. The form of the pustules was most frequently observed to be round and level-topped, with a slight depression or pitting in the centre. Deviations from this however occurred, the central depression being often scarcely to be seen, and frequently wholly wanting. In the place of this depression, hitherto considered as characteristic, in a few cases a mere dusky point was observed; but in others the pustule was raised in the centre and consequently conical, remained flat, or, by a change taking place in its progress from the flattened form, at length became pointed. The cellular structure of the pustule is always mentioned as occurring in the genuine affection, and would seem to be the most constant criterion by which to distinguish this from the spurious eruptions, which are usually simply vesicular. From this character Sacco drew the inference, that the genuine cowpock was seated in the subcutaneous cellular tissue,

whereas the spurious pocks being easily lacerated by the touch, and discharging their contents at once, were, he presumed, to be merely in the epidermis. "The structure of a pock," says Professor Hering, "may be best compared to the pulp of a lemon, the cells of which must be lacerated or cut before they will discharge their contents. Hence it also happens that, although the primary cowpocks appear to contain but little lymph, when they are opened, or the crust is removed, more flows out than had been expected." (p. 122.) Some variation appears to have existed with respect to the colour of the pustules, without there being any reason on this account to suspect their genuine character. This, we are aware, is contrary to the opinions of some high authorities, and we are willing so far to concede to these opinions as to say, that, until this and some other points of difference in the characters of the more recent cowpock eruptions from those assigned to the Jennerian pustule have been more closely investigated, lymph should by no means be taken from such pustules for the purposes of vaccination. It is even possible that some of the recent failures in the protecting powers of vaccination may have arisen from a want of due attention to the scrupulously correct characters of the pustules or vesicles from which lymph has been taken. The Wirtemberg reports describe the colour of the cowpock pustules as being in some cases blueish-white, or blueish; in others silvery, or with a lustre resembling mother-of-pearl, and lead-coloured; instances are however not unfrequently mentioned in which the pustules were white, whitish-yellow, pale-yellow, or of the colour of pus; and the author remarks that pustules which at the first glance appeared to be white or yellow, became silvery or pearly, on putting the finely wrinkled skin on the stretch. (p. 123.) The remarks which we have made with respect to the colour of the eruption will equally apply to the state of the areola, which, in several of the Wirtemberg cases, was observed to be very pale, or was altogether absent. Inoculation was however performed from these cases, and it is said with successful results. The author observes as follows, in reference to this point:

"In place of the erythematous inflammation in the most superficial layers of the skin is frequently found a somewhat deeper-seated inflammation of the cutis, and probably even of the subjacent cellular tissue and neighbouring glands. In this case, little or no redness is perceived, but, on the contrary, a hard and painful tumefaction (*Wulst*) is felt surrounding the pustule, and even a distinct hardened knot in the substance of the udder. There is here most probably a circumscribed inflammation of the udder, but which, if many such pustules be thronged together, may be sufficiently important to give rise to symptoms of fever, to affect the secretion of milk, and to render the milking of the animal almost impossible." (pp. 123-4.)

It is a matter of some difficulty to determine with sufficient accuracy the progress of cowpock, since in most cases the animal is seen only once, and the reports of the proprietors and herdsmen are of course not to be depended upon. In cases of retrovaccination and of inoculation of the cow with primary lymph, the first signs of the success of the inoculation became apparent on the sixth day; in the accidental infection of healthy cows through milking, the first trace of the pock is said to

have appeared on the third or fourth day, and is compared to the mark of a fleabite. Pustules thus generated attained their full development on the eighth, ninth, or tenth day, then became turbid and formed a crust, which remained to the third or fourth week, leaving a whitish cicatrix on falling off. The contents of the genuine pock are a clear, inodorous lymph, the consistence of which is sometimes perfectly limpid, sometimes a little clammy. The lymph, though at one time thought to retain its transparency uninjured, is said to become turbid and puriform after the full development of the pustule. It would seem, however, to be essential to the success of vaccination that only the clear colourless lymph should be employed. In the Wirtemberg reports only two exceptions are mentioned to this rule, in one of which the lymph was bloody, and in the other is described as being opaque and curdled (*klümprich*). It should be remarked that when the pocks are ruptured and irritated from repeated milking, they are longer in healing, but, according to the reports, do not give rise to malignant or fretting ulcers, as mentioned by Jenner. The formation of the scab commences in the central portion of the pustule, and, as the edges of the pock gradually contract, the crust is of course smaller in circumference than was the pustule the place of which it occupies. It is usually pretty thick, has a darkish-brown colour, the external layer being the darkest; it is thick, horny, and transparent at the edges, and the inner surface is of a pale or yellowish-brown colour. The cicatrix, as far as it has been examined, presents nothing characteristic.

Before quitting this subject, it is necessary to refer to the irregularity observed in the appearance of the pocks. The simultaneous development of the whole eruption has hitherto been held to be an essential characteristic of genuine cowpock; but here, as in some other particulars, the Wirtemberg reports are at variance with received opinions. In very many instances, it is said, that individual pustules were as much as from eight to fourteen days later in their appearance than the general eruption, and that, notwithstanding, these pustules were as available for the purposes of inoculation as those which first appeared. Among the sixty-nine instances belonging to the first class, in no less than twenty-three, or one third of the number, was the eruption of the pustules not simultaneous. In one case nearly four weeks elapsed from the first outbreak of the eruption, during which fresh vesicles from time to time made their appearance. We need scarcely point out that the same grounds for caution in the employment of lymph from sources of this description exist, as from those in which other hitherto presumed irregularities take place.

It is unnecessary to dwell upon the unusual symptoms which were remarked in a very few instances, and only as occurring in rare exceptional cases. Among these, however, it may be stated that the rumination with the mouth empty, sometimes observed before the coming out of the pock, is attributed by the author to the presence of a vesicular eruption within the mouth. Among the irregularities is also mentioned an instance of genuine cowpock occurring in a cow, which, according to its owner, had already, some years before, suffered several times from a similar eruption. Eleven months later, the same cow was again attacked

with an eruption, the nature of which, however, through failure of the experimental inoculation, could not with certainty be established; we may mention that the characters of this second eruption, as given in the detailed report, are to us by no means satisfactory.

VI. *Spurious and Anomalous Cowpock.* In the preceding abstract, which we have condensed from the treatise of Professor Hering, it has been our main object to lay before our readers the substance of the Wirtemberg reports, so far as they relate to the symptoms and progress of cowpock in the cow. The author places in a fourth section those cases reported upon, which, on account of their deviation from the characters assigned to the genuine disease, cannot be classed with any variety of cowpock, and must be considered as either spurious and anomalous in their nature, or essentially different affections. Among these he successfully describes: 1, The *Spitspocken*, *Nachpocken*, or *Euterseuche* (miliary-pock, after-pock, and udder eruption). These he considers to be varieties of the same affection, and differing from genuine cowpock, not only in the characters of the eruption, but in the far greater rapidity of their progress, by which at all times they may be readily distinguished. These eruptions, especially the two former varieties, which are named by Professor Hering respectively, *variolæ vaccinæ miliares* and *variolæ vaccinæ secundariæ*, are sometimes found to occur in connexion with the genuine cowpock, and we may observe that the *v. v. secundariæ* would seem to bear the same relation to the true vaccine in the cow which the miliary eruption, sometimes observed to precede and accompany the development of the vaccine vesicle in children, bears to the vaccine in man. 2, The *Stein-pocken*, or *Warzen-pocken* (stonepock or wartpock), *v. v. tuberculosæ*, and *v. v. verrucosæ* of Viborg. There are also varieties of the same affection frequently occurring together, and essentially distinguished from genuine cowpock by the form and structure of the eruption. The pocks are dry, hard, and remain for weeks or months partly in the form of warts, with brownish, rugged, cracked tops, partly as firm fatty tubercles. 3, The *Wasser-pocken*, or *Wind-pocken* (waterpock, or bladderpock), *variolæ vaccinæ albæ* of Jenner. These are simple elevations of this cuticle, and are readily characterized by their purely vesicular nature, as essentially different from the cellular pustule of genuine cowpock. Other eruptions noticed are the *v. v. herpeticæ* of Viborg, and the amber, blue, and black pocks of Nissen. These last have been already referred to in our Seventh Volume, and need not detain us here; as to those who attentively study the characters of the true vaccine, the marked deviations exhibited by these eruptions from the correct cellular pustule will always afford a sufficient warning to avoid them as sources of lymph.

The close analysis we have given of the work of Professor Hering is sufficient in itself to mark our estimate of its merits, and we cordially recommend it to the careful study of all who are desirous of contributing to the elucidation of the many important questions connected with the vaccine, as containing a vast store of valuable materials for reference and study.

Art. V.

1. *The Nervous System of the Human Body; as explained in a Series of Papers read before the Royal Society of London. With an Appendix of Cases and Consultations on Nervous Diseases.* By Sir Charles Bell, K.G.H., F.R.SS.L. & E., &c. &c. Third Edition.—*Edinburgh*, 1836. 8vo, pp. 501. With Plates.
2. *Narrative of the Discoveries of Sir Charles Bell in the Nervous System.* By Alexander Shaw, Assistant Surgeon to the Middlesex Hospital.—*London*, 1839. 8vo, pp. 232.
3. *Documents and Dates of Modern Discoveries in the Nervous System.* —*London*, 1839. 8vo, pp. 172.

Few controversies are more bitter than those which relate to priority of discovery; few present more temptations to fraud and misrepresentation; none are more profitless to the world. We can excuse a man for not feeling satisfied as to the reception of his discovery. We can justify him for his efforts to induce a more correct appreciation of it; because in so doing he is labouring for others as well as for himself. But what, save a purely selfish motive, can induce him to endeavour to raise himself upon the reputation of another? The *knowledge* is that which the world wants; of what practical importance is it by whom that knowledge has been first attained?

We do not think, however, that the world should be left in ignorance on the subject; because, if the real claimants do not make known their pretensions, there will always be found upstarts quite ready to seize the reward, without the slightest title to it. It seems to us to be the duty and the best interest of a discoverer, to present to the public a simple history of his progress towards the truth; not disguising his early errors, or endeavouring to clothe with more precise form the vague ideas with which he started; but recording every fact and opinion as it presented itself, with a scrupulous attention to dates. If he do this faithfully, the very air of *vraisemblance* which his history will present, will carry conviction along with it; and he may safely leave it to the public to decide between his claims and those put forward by others. Further than this, we cannot think that he should go. Angry recriminations can serve no good purpose, and only tend to keep alive the worst feelings of our nature, besides affording strong temptations to violate truth and sincerity. Nothing in such a history carries more weight than the candid acknowledgment of assistance supplied or of corrections received from other sources. On the other hand, nothing tends to excite suspicion so much as the exclusive arrogation to oneself of the whole merit of the discovery; since it can seldom happen in a lengthened enquiry that the conductor of it has not been indebted for facts or suggestions to others.

We have been led into these remarks by certain late events of much interest in the history of physiological science; and more particularly by the fresh discussion which has recently commenced as to the respective claims of Sir Charles Bell, Mr. Mayo, and M. Magendie, to the discovery of the double character of the spinal nerves, and of the true

functions of the fifth and seventh cranial nerves—unquestionably one of the greatest discoveries which the present age has witnessed. It is the object of the present article to examine the whole subject; and to award to every claimant what we consider his just right, without favour or affection. The works from which our materials will be derived are principally the three whose titles stand at the head of this article. That of Sir C. Bell appears to be a reprint of the Memoirs which he has presented at different times to the Royal Societies of London and Edinburgh; with an introduction, containing a general view of the functions of the nervous system, and an account of his own early enquiries in regard to them, as well as a history of the opinions previously entertained on the subject. We say *appears*, because we are bound to state that the most important of all these memoirs, in a historical point of view, has been so greatly modified, without due intimation of a change, as to be almost valueless for the purpose of reference. To this point we shall have to return hereafter. The history of the author's early enquiries is vague and unsatisfactory. It does not contain a single date. It omits all the errors into which the author was at first led, either by previously formed ideas, or by misinterpretation of observed facts. And it includes no acknowledgment of assistance or correction from others.

Now, it will be seen from our subsequent history that we entertain as high an opinion of Sir C. Bell's merits as a discoverer, as it would be easy to form by the examination of his successive publications. In fact, there is scarcely any point in the *later* history in which we do not coincide with his historian, Mr. A. Shaw, who, from his connection with him, might be suspected of partiality in his favour. But it is in the *early* history that both are deficient; and, as Mr. Shaw enters fully into this, for the purpose of exposing what he asserts to be the misrepresentations of others, the omission of the first errors of Sir C. Bell—in fact the positive contradiction of them—is a piece of seeming disingenuousness for which we cannot account, and which, if we are correct in our estimate of it, is in the highest degree discreditable to him. The remainder of Mr. Shaw's book is, on the whole, so candid and correct, that we should not have in the least suspected him of acting in this manner; and, if we had not come into possession of the document in question, we should have accepted his quotations from it as a fair statement of the opinions there expressed,—the fact really being, that the erroneous ones are kept back. Putting aside this point, we have little fault to find with the performance. Mr. Mayo is, we think, very justly reprobated for his attempts to strip Sir C. Bell of all the merit of priority, in favour of M. Magendie, when he could substantiate no claim for himself; and the mode in which he brought forward the corrections which he made upon Sir C. Bell's early views is shown in a very different light from that in which he has himself presented it. To these points we shall hereafter return; and we can here only express our regret that such differences have arisen between two individuals possessing high and independent claims to eminence.

The "Documents and Dates" are far from supplying a complete history of Neurology. Such a work, executed by one who should be at the same time impartial and well qualified, would be both interesting and

useful. Regarding the impartiality of the present editor, we cannot say much. The collection is so evidently designed to exhibit the supposed anticipation of Sir C. Bell by Mr. Alexander Walker, that we cannot have much doubt as to the source whence it has originated. We shall hereafter have occasion to enquire minutely into this new claim, and also into the editor's qualifications for accurately representing the opinions of other writers. The chief value of the book is its containing *the whole* of the important document we just now referred to;—namely, the unpublished tract of Sir C. Bell, distributed among his friends in 1811. Our subsequent strictures are based upon the supposition of this being a correct reprint; and, from the comparisons we have been able to make, we have no reason to think it otherwise.

We entered at some length, on a former occasion, into a review of the present state of our knowledge regarding the physiology of the nervous system. This review, however, had a special bearing on one particular department—the functions of the spinal cord, which was the subject at that time under discussion. At present, we propose to take a more general and less detailed survey, in which we shall introduce some modifications of our former views, rendered necessary by late additions to our knowledge. Our purpose in so doing is to enable our readers, in following us through the subsequent history, to carry with them a clear view of what may be at present regarded as the highest generalizations on the subject, so as to be able to form a more correct appreciation of the contributions of each discoverer towards the science of Neurology. It is not a little remarkable, that most of what is definitely known regarding the special functions of the nervous system has been ascertained within the last thirty years. We shall find that many *speculations* had been previously offered in respect to them; but few of these could be regarded as more than vague and ill-supported hypotheses. Many correct observations of phenomena had been made, and sagacious inferences had been erected upon them; but all were wanting in precision, until the guiding principle was established, which combined and directed the efforts that were previously unstable and desultory.

The ancient notion of the operation of the nervous system in producing its diversified phenomena was, apparently, a very simple and comprehensive one, but was really of no value whatever. It consisted in regarding the central organ as a sort of gland in which "animal spirits" were elaborated, and the nervous trunks as the channels by which these were conveyed to all parts of the system for their respective purposes. It was very convenient, therefore, to refer every manifestation of nervous agency, which could not otherwise be explained, to the operation of the "animal spirits;" and, however absurd and unphilosophical this may appear, it must be remembered that many physiologists at the present time are in the habit of seeking in the "vital principle" a similar refuge from their difficulties. We shall perceive that the progress of neurological research has been manifested in the more complete separation and classification of its varied phenomena, and in the assignment of these to distinct parts of the nervous apparatus. In the same manner, then, the philosophic physiologist will seek to explain the other phenomena of life by the properties of the organs manifesting them; and these may or may not be analogous to those manifested in the inorganic world.

Our present knowledge of the functions of the nervous system enables us to view it under three different aspects:—1. As the instrument of the mind; by which it acquires a knowledge of the external world through the medium of sensation, and operates upon it by an exercise of volition. 2. As the means by which various movements are excited in the bodily structure, which are immediately necessary to the performance of the organic functions, and to its protection from injury. 3. As the means by which the organic functions themselves are harmonised and controlled, and influenced by emotional states of mind.

Now, we find in Vertebrata three evident divisions in the nervous apparatus, to which these classes of functions may be respectively assigned. These are, the brain and nerves proceeding from it, the (true) spinal cord and nerves proceeding from it, and the (so-called) sympathetic system of isolated ganglia and nerves. The nerves proceeding from the brain are generally united with those connected with the spinal cord, so that the distinction between them cannot be traced without considerable difficulty; but in many invertebrate animals the distinction between the nerves of the two systems is more easily demonstrated.

In each division of the nervous system two kinds of structure are evident: the one consisting of continuous fibres, the other of a plexus of blood-vessels in which the fibres appear lost, and of an apparently confused mass of granules. Of the first kind the nervous trunks are exclusively composed; and as these are known to be simply *conductors* of whatever changes take place in the central or peripheral organs, there is good reason to believe that the fibrous structure, wherever it exists, has the same function. The fibres are of two classes; those proceeding from the circumference to the centre, or *afferent* nerves; and those proceeding from the centre to the circumference, or *efferent* nerves. The fibres of both kinds seem to communicate at their central terminations with the vascular plexus and granular substance. This constitutes what is termed the *cortical* substance of the brain,—the *gray matter* of the spinal cord,—and the *nuclei* of the ganglia. The *afferent* fibres of the first two systems, which commence on the general surface or in special sensory organs, have a corresponding substance (though its elements are somewhat differently arranged) at their peripheral origin. It is evident, both in the retina, the expansion of the auditory and olfactory nerves, and in the papillæ of the skin and tongue, that the vascular system is there peculiarly brought into relation with the nervous fibres; and a granular structure is always present, which seems intermediate between them. The peripheral termination of the *afferent* nerves, however, is different; they form a plexus in the muscles upon which they act; and in this no free extremities are discoverable.

It is well known that the active influence of the vascular system is essential to the production of any changes in the nervous apparatus. If the circulation of blood through the brain be suspended for an instant, insensibility supervenes; that is to say, the connexion of the mind with the external world is suspended. The spinal system may still be excited to action. But if (as in syncope) the circulation through the spinal cord be also weakened, its power of producing motions in respondence to impressions is diminished in like proportion. In the same manner the production of impressions on the peripheral origins of the afferent nerves

appears equally dependent upon the active influence of the vascular system. Every one knows that cold, which retards the circulation of blood through the skin, diminishes also its sensibility; and obstruction to the circulation by any other cause, such as pressure on the arterial trunks, produces the same effect. We have no opportunities of observing such affections of the special sensory organs, except in general syncope; and then, as the brain is also affected, there is no proof that their impressibility is diminished, though it can scarcely be doubted that this is the case. It is evident, therefore, that the changes which take place at the peripheral origins of the afferent nerves bear considerable analogy with those which occur at the central origins of the efferent; and the striking analogy of structure already pointed out would thus lead us to regard the gray matter of the central organs as the true seat of all the changes which take place in them. This belief is strengthened by various facts, of which some will be presently mentioned.

Less is known of the structure of the third or sympathetic system. Its fibres seem less perfect and distinct than those of the two former. It has been maintained by some that the gray matter is not confined to the nuclei of the ganglia, but is distributed through the trunks. The investigations of Remak* and others, however, appear to have shown that the gray colour is that of the fibres themselves, and that the vascular and granular structure is confined to the nuclei; so that there are, probably, afferent and efferent fibres in this system also, although they cannot be demonstrated by experiment.

1. The *cerebral*, or, as it has been termed, *sensori-volitional* system, bears a less proportion to the rest in the simpler tribes of animals than in the higher; and it cannot always be distinguished from that which corresponds with the spinal cord of Vertebrata, and which (for reasons to be presently given) we shall call the *excito-motor system*, or *system of reflexion*. Wherever the mouth is placed on a prominent part of the body, and is accompanied by organs of special sensation which determine the movement of the trunk in that direction, we find a ganglion, which appears to have a presiding or controlling influence over the rest; and this may be regarded as the chief, if not the only seat of sensibility, and as the originator of the voluntary movements: in other words, the instrument of whatever *mind* these animals can be regarded as possessing. In the radiated tribes, however, we find the mouth in the centre of the body, and the nervous system forming a ring around it; the ganglia on this ring (each of which, in the asterias, is connected with what seems to be an eye at the end of the corresponding ray) are evidently all equal in their endowments, and progression takes place indifferently in any direction.

Where, however, the cerebral ganglia are distinct, as in most of the Molluscous and Articulated tribes, we find that, however small they are in proportion to those of the excito-motor system, they send nerves to every part of the body supplied by the latter; for the purpose, it would seem, of controlling, harmonising, or antagonising its actions. These nerves proceed as connecting trunks from the cerebral ganglia to the other centres; and then divide into filaments, which unite with those proceeding from them

* See B. and F. M. R., Vol. VII., p. 501.

to the several organs. Each organ, therefore, receives four sets of fibres: an afferent and efferent set, which connect it with the cerebral ganglia, and are the channels of sensation and of the influence of the will; and an afferent and efferent set which connect it with its particular ganglion, and serve to convey the stimulus of impressions which produce motions by reflected influence.

In proportion as the special sensory organs are developed, and the animal less completely governed by *mere* instinct, we find the cerebral ganglia and system of nerves more predominant. This is especially the case in vertebrata. Here it is united more closely, however, with the excito-motor system (for the sake, perhaps, of the protection which the osseous structure is adapted to afford to both); but it is still distinguishable. The fibres which form the crura cerebri are continuous with the fibrous structure of the spinal cord, and form part of each root of the spinal nerves, by which they are distributed with those of the spine itself. Above, they may be traced to the cortical substance, or to the insulated tracts of gray matter. The medullary substance of the cerebrum seems to consist in part of afferent and efferent fibres, but principally of fibres which serve to unite the different parts of the cortical substance with each other, constituting the various transverse and longitudinal commissures. These fibres of communication are especially abundant in the human brain, to whose bulk they contribute in large proportion.

2. The *true spinal* or *excito-motor* system consists, in the Vertebrata, of a continuous ganglion with nerves proceeding from it, which unite, as just stated, with those derived from the cerebrum. Its ganglionic character is shown by the existence of gray matter in its centre, in which the nervous fibres are lost, just as in the cortical substance of the brain, and in the nuclei of the ganglia of the invertebrata. Both roots of the spinal nerves are connected with it;* and each is thus made up of fibres from the cerebral system, and of fibres from this spinal ganglion. The spinal cord, or prolonged ganglion, of the vertebrata, differs greatly in its functions in different parts. That portion which is termed the medulla oblongata is the centre of the respiratory movements, and of those of deglutition. The lower part is concerned in the reflex movements of the general locomotive apparatus, and of the sphincters of the rectum and bladder, as well as in other actions. Now, in the invertebrate animals we find these different ganglionic centres more or less disjoined. In the Mollusca we have generally a pedal ganglion, a respiratory ganglion, a stomato-gastric ganglion, and sometimes a palleal ganglion for regulating the motions of the mantle, which are partly concerned in respiration and partly in progression. These are generally quite insulated from one another, although each is connected with the cerebral ganglion, as formerly mentioned; and as the connecting trunk from a more distant centre sometimes passes through or over a nearer one in its way to the head, an appearance of communication between these is occasionally presented where none really exists. In the Articu-

* This was pointed out first by Bellingeri, and afterwards by Mr. Grainger (B. and F. M. R., Vol. V., p. 496); and has been subsequently in part confirmed by Remak (Ibid Vol. VIII., p. 504).

lata, as in the vertebrata, there is a repetition of similar parts in the different segments; and there is, accordingly, a repetition of ganglionic centres and corresponding nerves. Thus the double chain of ganglia, which runs along the ventral aspect of the body, is evidently a repetition of the pedal ganglion of the mollusca, and corresponds with the lower part of the spinal cord in vertebrata.* The respiratory ganglia are repeated in like manner; and the stomato-gastric system often possesses three, or even five distinct centres. Some of these, however, appear to belong in part to the sympathetic system. In the vertebrata all these centres are fused into a continuous mass. The term *spinal system* is a convenient one, if it is understood to include the scattered analogues of this system in the invertebrate animals.

The functions of this spinal system may be stated to consist in the reception of external impressions, which, being conveyed by the afferent nerves to the central organs, excite respondent motions by an impulse transmitted to the muscles through the efferent trunks. The nature of these has been fully discussed on a former occasion;† and although we did not then admit that the phenomena justified the attribution of them to a system of nerves distinct from the cerebral, as we now accord with Dr. M. Hall in representing them, we have nothing to alter in the account we there gave of the general character of the phenomena. That *sensation* is generally produced by the impressions which excite reflex actions is fully explained by what has been stated of the joint distribution of the cerebral and spinal systems of nerves; and it appears to us more clear than ever that sensation does not participate in actions of this class, in the higher animals at least, though it usually accompanies them. In regard to the lower tribes, however, there is more obscurity; since there sometimes appears, as already stated, little or no difference in the endowments of the different ganglia.

Several different groups of muscular actions are performed by this system; and all of these are closely connected with the maintenance of life. We find, however, that, as we ascend the scale of animal creation, many of them are gradually subordinated to those of the sensori-volitional system; and the same thing may be observed in the advancing growth of the new-born human infant. Thus the prehension of food by the lips, or mandibles, seems to be an action as completely reflex in its character, in many of the lower tribes, as that of respiration; we know that it is so in the young of man and other mammifera; yet in their adult state it becomes a voluntary act. The movement of deglutition is always a reflex one, however, and cannot be produced by a direct effort of the will. In the lower tribes, again, in which respiration is a mere organic function, and is not subservient to speech and other voluntary actions, it seems to be little affected by the cerebral system; whilst in man, we know that it is much controlled and modified by the will. This is still more true of the locomotive organs, whose reflex actions in man are entirely guided by the will, and are, in fact, scarcely distinguishable; whilst, in many of the lower tribes, the will appears merely to control and direct them.

* See Dr. Carpenter on the Nervous System of Invertebrata, p. 58.

† See Review of Hall, Grainger, Mayo, &c., in Vol. V.

There is a class of movements in which both these systems appear to participate;—those, namely, which result from *emotions* of the mind, and which are particularly manifested in the respiratory apparatus. They would appear to originate in the cerebral system, since we know that *sensations* are necessary to excite these emotions; and there is reason to believe that, if the cerebral lobes be removed, no emotions can be excited. But they would seem conveyed rather by the spinal than by the cerebral nerves; for we find them retained, with the reflex actions of respiration, when the muscles are paralysed to voluntary influence; and if the respiratory nerves do not act in their usual manner, the power of expression is lost. The subject is at present involved in considerable obscurity.

As far as we are acquainted with the functions of the *cerebellum* (we eschew, for the present, the questions of *phrenology*), it would seem that this organ is concerned in regulating and controlling the movements of the body in such a manner as to maintain its equilibrium. From its connexions it would seem to belong partly to the cerebral and partly to the spinal system; and it is by no means impossible that it may, to a certain extent, regulate and combine the actions of both. It is generally found to be the largest in those animals which execute the greatest variety of movements; particularly if these be such as require a nice and continued equipoise of the body. Thus it is scarcely developed in fishes, whilst it is large in birds and mammalia. It is more highly developed in birds which hover long on the wing, as the kite, than in those which simply fly; and it is much larger in proportion to the spinal cord in man (who walks erect) than in any other of the mammalia; the semi-erect apes approaching nearest to him in this respect.*

3. The *sympathetic* or visceral system of nerves is more closely united with the spinal system in the lower classes than in the vertebrata. But even in the latter it is not entirely distinct. Late enquiries have shown that filaments derived from the cerebro-spinal nerves exist in those proceeding from the sympathetic ganglia; and that, on the other hand, fibres analagous to those of the sympathetic exist in the spinal nerves. The enquiries of Remak tend to prove that these fibres terminate in the ganglia, on the posterior roots of the spinal nerves, which may therefore be regarded as a part of the sympathetic system. It can scarcely be doubted, then, that these ganglia, and the fibres proceeding from them, have an influence on the nutritive processes; and this will account for the injury to these processes occasioned by section of the cerebro-spinal nerves, which have been usually regarded as having a less direct connexion with them.† Regarding the general functions of the sympathetic system, we have nothing to add to what we stated on a former occasion. It seems to us to possess the same kind of concern in the organic sympathies as the spinal system has in the evident sympathetic movements; and just as, in the latter case, muscular contraction may be excited in insulated parts without the influence of nervous agency, so do we believe

* See the Comparative Tables in Serres' Anatomie Comparée du Cerveau, tom. ii., pp. 173 and 423.

† We now partly agree, therefore, with Dr. M. Hall, in our view of the functions of these ganglia; though, for the reason formerly stated (Vol. V. p., 521), we cannot, with him, regard nutrition as *dependent* on them.

that the individual acts of nutrition and secretion are independent of the sympathetic system, although harmonised and controlled by it. To use the felicitous illustration of Dr. J. Reid, " the movements of a horse are independent of the rider on his back,—in other words, the rider does not furnish the conditions necessary for the movements of the horse,—but every one knows how much these movements may be influenced by the hand and heel of the rider."*

If the preceding outline be regarded as giving a fair summary of the present state of our knowledge on the subject, it will be evident, from a slight retrospective glance at the mode in which it has emerged from its original chaos, that three principal eras may be recognized in the history of its erection. The *first* is that of the separation of the sympathetic from the cerebro-spinal system. It is not easy to say who was the first to point out the anatomical distinctness of the two; but there can be no question that to Bichat we owe the first definite assignment of their respective functions. We believe him to have taught very erroneous opinions as to the nature of the influence of the sympathetic system on the processes of nutrition and secretion; yet the term which he applied to it—the *nervous system of organic life*,—in contradistinction to the cerebro-spinal system, which he designated the *nervous system of animal life*, is by no means inappropriate. We shall return to his opinions hereafter. The *second* era is that of the discovery by Bell of the structural distinctness of the *afferent* and *efferent* fibres of the cerebro-spinal system. The difference in their functions had long been noticed, and had by some been attributed to structural distinctness; but other physiologists of the highest eminence denied this, and no certainty could be said to exist on the subject. By his separation of the respiratory series from the general system of cerebro-spinal nerves, he may be regarded as having approached the *third* great improvement, which consisted in the separation of the *true spinal system*, of excitor and motor nerves, from the cerebral system of sensation and volition. This is principally the work of Dr. M. Hall. Previous physiologists (as we formerly pointed out) had observed insulated phenomena, which they attributed to the independent action of the spinal cord; and some had expressed the opinion that neither sensation nor volition were concerned in their production. By classifying and grouping together these phenomena, and combining them with many others first observed by himself, Dr. H. was able to specify in a much more full and precise manner than had ever before been done, the particular functions of the spinal cord, as distinguished from those of the brain. This was his first step; and he may be regarded as then on a par with those who had *functionally* distinguished the afferent from the efferent nerves. But many of his experiments and observations led him to the belief that the spinal and cerebral nerves are really as distinct from each other as the afferent and efferent, although, like them, generally bound together in the same trunk. This view, however, was not received by many of those physiologists who accorded with him in his earlier conclusions, being regarded by them as unsupported by anatomical evidence. To his own mind this evidence did not appear necessary; the *functional* difference being with him suffi-

* On the Functions of the Eighth Pair. Edinb. Med. and Surg. Journ., vol. 51.

cient to prove the structural distinctness. But others were not so readily convinced; and this is not surprising, when it is remembered that the immortal Haller positively denied the structural distinctness of the motor and sensory fibres, although he admitted that their influence was conducted in opposite directions. The anatomical evidence was supplied with regard to the Vertebrata by Mr. Grainger, and with regard to the Invertebrata by Dr. Carpenter; both of whom acted on hints thrown out by Dr. M. Hall, in bringing facts, some of which were new, and some previously known, to bear upon his doctrines.

We have traced the mode in which Dr. Hall arrived at this discovery, and we have seen that he advanced from *general* to *particular* facts. Now, it is a little curious that, if Sir C. Bell's attention had been less exclusively directed to the respiratory system, he would probably have arrived at the same conclusion by advancing from *particular* to *general* facts. If he had noticed that, in the invertebrate animals, not only the respiratory apparatus has a centre of nervous action distinct from the cerebral system, but that the stomato-gastric and locomotive nerves, and sometimes others, have equally distinct centres,—and that all these are combined in the spinal cord of the vertebrata,—he could scarcely have avoided the inference that their actions are of the same character, though usually more under the control of the cerebral system. It is by his having pointed out this fact that Dr. Carpenter appears to us to have contributed evidence in support of Dr. M. Hall's views, beyond what the mere proof of their applicability to these classes of animals would have afforded. Sir C. Bell had in many instances spoken of the respiratory nerves as not only functionally but structurally distinct from those of general motor power, even when bound up in the same sheath with them; and has pointed out that one may be paralysed without the other. To those who received his doctrines regarding the respiratory system, therefore, the extension of their views to Dr. M. Hall's excito-motor system need not be difficult.

We are now prepared to enter into a more detailed examination of the general history of neurological science; in this survey we shall notice but briefly the subject of our former enquiry, directing our attention rather to those topics which concern the discoveries of Sir C. Bell, Mayo, and Magendie. We would preface our summary with the observation which we formerly made on a similar occasion: "The claims to *discovery*, on a question like the present, are very difficult to adjust, if all the vague thoughts which have preceded them respecting the subject they involve are allowed to be brought in evidence of previous right;—thoughts, too, which would perhaps have been forgotten, if the clear statements of the writer whose claims to originality are opposed had not revived them." After *any* grand discovery has been made, it is generally easy to find some glimpses of it in the writings of previous philosophers. But we never think of giving to *them* the credit of it, unless they have contributed something more than a *guess* that such things *might possibly be.* Who, for example, ever thinks of depriving Watt of the merit of inventing the steam-engine, because the Marquis of Worcester, Savery, or Newcomen had partially and imperfectly employed the same motive power, but on principles very different? And yet Watt's *first* engine was but an improvement on Newcomen's.

If he had then stopped, and any other person had carried forward the improvement on principles which he there applied, but whose full development did not occur to him, it is evident that the merit would have been divided But, with that remarkable sagacity which characterised him, he was enabled to *lead* for many years the course of improvement, and to bring his invention to all that perfection which *theory* could give, without receiving important assistance from any one. Yet, in perfecting his machine, he did make use of the inventions of others; as the introduction of the crank, the parallel motion, and other contrivances, fully testify. But these improvements had no relation to his principle; they merely enabled him to carry it into more advantageous operation.

Now we think it will appear that Sir C. Bell stands very much in this position. In his earliest statement on the subject he developes a principle of enquiry which had not previously been made the subject of investigation. It cannot be questioned that he was at that time far from seeing the full bearing of this principle; and that it gradually acquired importance in his mind as new applications of it suggested themselves to him. But it is to be borne in mind that these applications were made by himself; and that the progressive modifications which his views have undergone have resulted, in almost every instance, from his own researches. In some points it will appear that he was anticipated by others; but we shall find strong reason to believe that he was ignorant of their results; and that, if he had become acquainted with them, he would have been earlier led to the development of truth, without his merit as a discoverer being in the least degree diminished.* In the solitary instance in which an important correction was supplied by another enquirer, that correction was derived from experiments made without regard to his principle, and by an individual who was *at that time* strenuously opposing it. If Sir C. Bell had relinquished the investigation before the publication of his first memoir in 1821, it would perhaps be rather difficult to say what precise claim to discovery he could have set up: since, as we shall presently see, a difference of function in the two roots of the spinal nerves had been previously guessed at; and, although he was the first to attempt an experimental solution of the question, he had arrived at no very definite results. But we know, through subsequent publications, not only of his own but of the late Mr. John Shaw's, that he was engaged in following out this enquiry during the ten years which followed the first statement of his views; and we are enabled to gather from them the degree of perfection they had attained, long before his first enunciation of them to the world. And we can trace in Mr. J. Shaw's papers their gradual modification and maturation, even better than in his own memoirs; for, as we shall hereafter see, the idea of a separate respiratory system early occurred to him; and in following it out, he would seem, taking *his own* publications alone, to have somewhat lost sight of the original subject of his enquiries. We shall presently attempt to develope what appears to have been the

* We here principally refer to the data with which the anatomy of the roots of the fifth pair might have supplied him. Much more was known on the subject, as we shall presently show, than he was aware of.

progress of his views in this important investigation; and we offer these preliminary remarks, lest it should be thought that, in bringing together the expressed opinions of previous authors, we are attempting to deprive him of the credit he so justly deserves. A historical survey of this kind has many points of interest, and some of practical utility. The former we need scarcely advert to; but the latter deserve a brief notice.

Many persons deprecate the study of the older authors on scientific subjects as a waste of time and mental energy. To a certain extent this is correct. To seek in them for that knowledge which they had no means of attaining, would be obviously absurd. No one expects to find a microscopical description of the tissues of the organized body before the era of the invention of the instrument. But it will often occur that men of original views suggest thoughts which they have no power of following out,—which are, in fact, in advance of their time; and these may be profitably taken up at a subsequent period. Moreover in those departments in which the phenomena are constantly presenting themselves to inspection, an acute observer will frequently seize almost intuitively the essential details, and transmit to posterity accounts of them which may be highly valuable as bases for further enquiries. Thus the descriptions of diseases, founded on symptoms alone, left us by Hippocrates, were probably not surpassed by those of any other physician down to the time of Sydenham; and the descriptions of various species of animals, including not merely their external form but their internal structure, which were drawn up under the direction of Aristotle, if not actually by him, would, if attended to by subsequent naturalists, have saved them from many errors, some of them egregious ones. We by no means recommend the study of by-gone authors to those who desire merely to acquaint themselves with the present state of the science they are pursuing; since to them it would be generally a misemployment of time. But on those who are pursuing the path of original enquiry in any department upon which the means of investigation were within reach of their predecessors, we would urge a careful research into their contributions, whether of fact or opinion, as a matter of interest as well as of duty. They will frequently thus be able to start from a more advanced position; they will often receive valuable assistance in their progress; and, when they have completed their work, they will be able to reply more successfully to the attacks of those (and some, we fear, will always be found to cavil thus) who represent their discoveries as "nothing new."

With these views, then, we shall commence our historical retrospect. We by no means assert that it is a complete survey of the literature of this interesting subject. Many names and opinions might, we doubt not, be added; but we feel confident in the correctness of our statements on all essential points, and in the truth of the view they present of the *general* state of knowledge at each epoch.

The connexion of the nervous system with the functions of sensation and motion has been known from the most ancient times; and it would be difficult, if not impossible, to trace the discoverer of this relation. It appears, too, that there was very early a vague idea that different parts of the structure might minister to these two functions respectively. Hierophilus and Erasistratus, the first dissectors of a human body, made

a distinction between the Νεῦροι κινητικοὶ and the Νεῦροι αἰσθητικοί; but it does not appear that they gave any very definite statements as to the details of the subdivision. Aretæus and other ancient writers partook in this opinion, drawing strong evidence in its favour from the not unfrequent occurrence of loss of one function while the other remained unaffected. None of them, however, advanced so near to the truth as Galen; and to his writings, therefore, we may refer as giving the highest view of the state of knowledge among the ancients on this interesting subject. The following summary will, we believe, present a fair general statement of his opinions.

Galen certainly believed that the nerves of motion are *structurally* distinct from those of sensation, and that they are connected with different parts of the brain. He stated that the latter are all soft, and the former hard (which is pretty nearly true); that the latter arise from the anterior, and the former from the posterior part of the brain. It is on record that his attention was particularly directed to this subject by his having been called on by some of his contemporaries to account for the manner in which he had cured a partial paralysis of the finger, by applications made to the spine. In reply he told them that two sets of nerves went to every part; one to endow the skin or other organ with sensibility; the other to give the muscles a power of voluntary action. But he had no idea of any functional difference in the nervous trunks, beyond that arising from their texture. He imagined that the soft nerves were more susceptible of impressions, and the hard ones less impressible, but stronger, and therefore better fitted for action. He maintained, too, that a nerve which originates, as one of sensation, from the soft part of the brain, may during its course become condensed in its texture and assume the office of a motor nerve. It is evident, therefore, that he had no idea of any *essential* difference in the character of the various parts of the central organs, or in that of the nervous trunks as derived from them,*

The writings of Galen, as is well known, maintained their sway over medical opinion for nearly fifteen centuries; and men hardly dared to speculate, much less to avow opinions in opposition to those of their great master. With the revival of the spirit of original enquiry, however, new anatomical facts and pathological observations were brought together; and it might have been supposed that physiological inductions would have advanced in no unequal proportion. This was the case in almost every other department but neurology; and it is difficult to assign any other reason for its slow progress than the acknowledged difficulty of the enquiry. One can scarcely help being surprised that, when the functions of lacteal and lymphatic systems, and the double circulation of the blood, were being established, no progress was made towards the elucidation of phenomena so interesting as those connected with the nervous apparatus.

The difference between the motor and sensory functions of the nerves continued to be regarded as a question of speculative interest; and various hypotheses were framed to account for it. No one, however, seemed to think of putting any of these to the test of experiment;

* See Medical Quarterly Review, April, 1834.

indeed, they were generally such as no experiments could have been devised to meet. The names of Vesalius, Fallopius, Van Swieten, and their contemporaries, may be mentioned in this connexion. The attention of physiologists was now more directed, however, to the involuntary motions; and these were regarded by Willis (who was followed on this point by Vieussens, Boerhaave, Du Hamel, and others,) as taking their origin in the cerebellum, whilst the voluntary movements were supposed by them to result from the influence of the cerebrum. In this point an advance towards truth was evidently made; but it was not until some time afterwards that the true nature of the involuntary movements was explained. To Willis we owe much anatomical information regarding the nervous system. The numerical nomenclature of the nerves of the head was established by him; and though this has now had its day, there can be no doubt that it has also had its use. With more precise views in reference to the anatomy of the cerebral nerves, some advance in the knowledge of their uses could scarcely fail of being made; and we accordingly find that from this time the olfactory, optic, and auditory nerves were generally spoken of as nerves of *sensation* only (although it was usually imagined that they bestowed *common* as well as *special* sensibility on the organs to which they were respectively distributed), whilst the third, fourth, and sixth pairs were, from their exclusive connexion with muscles, recognized as specially, if not exclusively, *motor* nerves.* Still no attempt was made to account for the difference in function by more minute researches into their origin, and their connexion with different parts of the central organs.

So vague, indeed, were all the notions entertained upon this subject, that many writers seem altogether to have lost sight of the distinction just alluded to; and the old doctrine which regarded the brain as the

* Willis entered with much more correct minuteness into the functions of the cranial nerves than he has generally received credit for; his numerical nomenclature having been adopted in preference to his physiological. He distinctly states (*Cerebri Anatome*, cap. xxi-xxiii.) that the first and second pairs are nerves of *sensation only;* and that the two next are most especially subservient to motion (*motui potissimum inservire).* The third pair he distinguishes as performing the voluntary movements only of the eye; and he represents the fourth pair as the channel of the involuntary movements, and movements of expression, which he regarded the cerebellum as influencing. He notices the fifth pair as different from the first four, inasmuch as it ministers both to sensation and motion. He regarded all the branches, however, as equally possessed of this double endowment; and believed that the motor influence was subservient not only to volitional but to emotional impulses. He noticed, however, the special destination of the third branch to the masticator muscles; and states his belief that the sense of taste is due to this nerve also. The sixth pair he states to be a muscular nerve simply, and to have for its office the abduction of the eye for the purpose of gaining a backward view, as is done by animals under the influence of fear; whence he regards it as a sort of instinctive nerve. He states that in animals which have a nictating membrane, a twig of this nerve supplies the muscle which draws it across. The portio mollis of the seventh pair he states to be purely a nerve of sense; and the portio dura to be in reality a distinct nerve, although supplying the means by which the *action* of *hearing* may be best performed. He speaks of it as distributed *entirely to the muscles* of the face, and as designed to bring the various organs into co-operative action with that of the auditory sense. With regard to the ninth pair, he states most distinctly (chap. xxix.) that it is the nerve of the motions of articulation, whilst the fifth pair is that of the sense of taste; *and that the reason of this organ being supplied with two nerves is its double function.* As Willis is an author of whom England may justly be proud, it is rather strange that Sir C. Bell has said so little of him.

elaborator of the "animal spirits" which were transmitted by the nerves to every part of the body, still held its place in the schools, as the caustic ridicule of Tristram Shandy abundantly testifies. Even Haller, who contributed so largely to our knowledge of the true influence of the nervous system upon the organism at large, had very confused notions on the subject of the nerves themselves. Thus, although he clearly demonstrates that the first, second, and portio mollis of the seventh nerves were subservient to sensory impressions only, he tells us in another place that he knows not "a nerve which has sensation without also producing motion; the nerve which gives feeling to the finger is that which moves the muscles; and the fifth nerve of the brain branches to the papillæ of the tongue, and also to the muscles." He was perfectly aware that for a *sensation* to be felt, an impression must be propagated along the nerves from the circumference *to the centre;* and that, for a muscular contraction to be produced by a nerve, an impulse must be propagated along the trunk *from the centre* to the circumference; and yet he maintains that the same fibrils may convey either sensory or motor impulses, according to the direction in which they are transmitted.* "The same nerves," he afterwards states, "must evidently preside over both sense and motion; as *we cannot admit a distinction between the two systems of motory and sensitive nerves.* If sense sometimes remains after motion is destroyed, this seems to be because much more strength is required for the latter. Dying people hear and see, when they are incapable of motion." It is justly remarked, therefore, by Sir C. Bell (p. 4), that "Haller, who had traced the opinions of authors with the utmost diligence, gathered nothing from the ancients. The confusion in his mind, as well as in the minds of our most learned physicians and commentators, declared the necessity of having recourse to the volume of nature itself."

Having on a former occasion† given a full account of the opinions of Whytt on the subject of the sympathetic movements, we need not here go over the same ground. Further consideration and acquaintance with Prochaska's writings have even more fully satisfied us of the position we then took—that, in attributing these actions to the "sentient principle," Whytt did not regard *sensation* as necessarily participating in them. He was aware that the spinal cord furnished this sentient principle to the nerves connected with it, and had an action independent of the brain. And he was probably the first to prove, as a general fact, that "the vital and involuntary motions" are dependent upon a *stimulus* propagated to the central organs, and there exciting motor impulses which act through the efferent trunks upon the muscular apparatus. It is quite true that he assigned this as the mode in which some actions are excited (those of the heart and alimentary canal for example), which we now know to be mainly independent of the nervous system; but this error by no means detracts from his merit as having established by solid reasoning that sympathetic actions are not to be explained by "consent of parts or continuity of membranes," but by the interposition of nervous agency operating in the circle just described. Whytt is the first

* First Lines of Physiology, section ccclxxvii—Cullen's edition—Edinburgh, 1786.
† Vol. V., pp. 523-6.

author who is quoted in the " Dates and Documents," and the extract from his work is thus headed,—" Sensation the cause of a conservative motion in the whole or in the separated parts of animals." We must again repeat, that the true import of the term " sentient principle" cannot be understood, unless attention be paid to the phraseology of the time, and especially to that of the Stahlian school, of which Whytt was one of the last supporters. And when he distinctly states, that some stimuli produce vital movements *without consciousness*, we may surely take this as a definition of his meaning. Let it not be forgotten that many physiologists and pathologists, even at the present time, are in the habit of using the term *sensation* itself as synonymous with what is more correctly designated *impression*, and give the term *perception* to the act by which the mind becomes *conscious of sensation.* Now, we would ask, what clearer indication could be given of the intention of a writer to make this use of the term *sensation*, than to state that it is *independent of consciousness?*

The following experiment recorded by Whytt, in his " Observations on Sensibility and Irritability," is a very curious anticipation of those made by Dr. M. Hall. It is made use of by the author as a proof that " the soul" is not confined to the brain, but is present in the spinal cord also. " The late reverend and learned Dr. Hales informed me that, having many years since tied a ligature about the neck of a frog, to prevent any effusion of blood, he cut off its head, and thirty hours after, observed the blood circulating freely in the web of the foot: *the frog also at this time moved the body when stimulated;* but that, on thrusting a needle down the spinal marrow, the animal was strongly convulsed, and immediately after became motionless." Let it not be forgotten, however, what was his view of the operation of the soul on the living system. He regarded it as the universally operating " principle," which governed all its movements and actions; and every other " principle " was a subdivision of it, acting in some peculiar channel. Had the term " reflex function " been proposed to him for that portion of it which operated through the spinal cord, we are persuaded that he would have adopted it on account of its appropriateness, and set it down as an action of " the soul," just as many physiologists of the present day would do in regard to the " vital principle." The state of anatomical knowledge at that time did not enable him to limit the participation of the nervous centres in the actions of this class to the spinal cord and medulla oblongata, as we can now do; but no one who dispassionately reads his writings with due allowance for his phraseology, can fail to allow that, in all essential points, we have given a fair representation of his opinions; and these, although comparatively little known elsewhere, have been annually made the subject of prelection in the Edinburgh School, from the days of Cullen to the present time.

During the latter part of the eighteenth century, many important contributions were made to the anatomy and physiology of the nervous system. The discovery of the general fact that the ganglia of the spinal nerves are formed on their posterior roots only, and that the fibres derived from the anterior roots pass over them, seems to have been made, or at least first enunciated by Dr. Monro, *primus*, in his valuable work on the nerves. As to the functions of these ganglia, great difference of opinion prevailed.

By Dr. Johnstone it was conceived that they were interposed for the purpose of cutting off sensation in those operations of the nervous system which produce the involuntary movements. A remark of Dr. Monro's on this hypothesis shows his complete ignorance of the respective functions of the two roots. "That ganglia do not serve," he says, "to render motions independent of our will, as an ingenious author has supposed, is evident, without observing more than that all the branches of the fifth pair, and the posterior half of all the spinal nerves of the voluntary muscles, pass through ganglia." On this Sir C. Bell justly remarks—"If I had ascertained nothing more than that no motor nerve passes through a ganglion, the observation would have been important towards the true doctrine of the nerves."

Dr. Monro's mistake regarding the fifth pair was corrected by other anatomists. Santorini and Wrisberg had observed its two roots; Prochaska and Soemmering noticed that the anterior root passes by the ganglion, and enters the third division of the nerve; and both were struck by its conformity in this respect with the spinal nerves. Paletta went further; for, tracing the third branch principally to the muscles of the jaw, he conceived that the anterior root, which enters it alone, is a purely muscular nerve; "and when," says Sir C. Bell, "we should have expected that he was about to discover the truth, he acknowledges that he does not know what to make of the other branches of the fifth nerve." It appears to have been the general opinion that a plurality of nerves in any organ was simply to provide it a good supply of nervous agency, so that if one failed another might supply its place. Soemmering seems to have partaken of this idea; yet he points out that one nerve might give motion to the tongue, and another sensation: whence a man might lose his taste and yet move his tongue as before.* Scarpa dwelt with great minuteness on the anatomy of the roots of the spinal nerves, and had an idea that there might be a functional difference between them. But his idea was wrong. He asks, "Is the posterior root a proper and peculiar kind of nerve, belonging exclusively to the spinal marrow, whilst the anterior root is a cerebral nerve?" So far as this speculation was regarded, then, it might truly be said that this illustrious anatomist and surgeon "left the system doubly confounded."

We see, then, that those who examined the anatomical relations of the nerves with the greatest minuteness were ignorant of the physiological bearing of the facts they revealed; or that, at most, they derived from them one or two inferences which had only a very limited application. On the other hand, those who held the notion that the sensory and motor nerves are distinct, were content with the mere speculation, and did not attempt to support it either by the evidence of anatomical relation, or by that with which experiment might have supplied them. Pouteau, whose posthumous works were published in 1783, revived, on pathological grounds, this ancient opinion; he remarked, however, that it had been long abandoned by anatomists. He supposed that the nerves of sensation come from the cerebrum (or rather proceed to it), and those of volition from the cerebellum—an opinion which is not without its advo-

* This statement, like that of Willis, Sir C. Bell seems to have overlooked in his generally candid, though deficient, historical summary. It will be found, in the *Lehre von Hirne und von den Nerven*, p. 255.

cates at the present time. But he went no further than this vague surmise. Dr. Darwin, in his remarkable Zoonomia, points out that a morbid sensibility to warmth is occasionally observed in paralysis, although the sense of touch be not morbidly acute, or be actually impaired; and he thence infers, not only that there are nerves of sensation distinct from those of motion, but nerves for the sensation of temperature distinct from those of common sensation. But here his speculations ended.

During the period we have been just considering a very important change took place in the views of anatomists, in regard to the sympathetic, ganglionic, or more truly the *visceral* system of nerves. This had been formerly described as a kind of off-set from the cerebro-spinal, descending from its origin in the fifth and sixth cerebral nerves, and deriving reinforcements from the spinal nerves. Previously, however, to the time of Bichat, juster views of its nature were prevalent; and its title to the character of a distinct system was generally recognized. Availing himself of this, and of a certain superficial analogy between the chain of ganglia it presents and that which constitutes the central apparatus in the articulated classes, he propounded his specious view of its functions as the nervous system of organic life. This, to a certain extent, is correct; since we are fully assured that its operation is confined to the organs concerned in the purely vital functions. But we cannot agree with Bichat that all the changes to which these organs are subservient are regulated by it, as he supposed, whilst the cerebro-spinal system has no participation in them. And further, the untenable nature of his theory is evident from the very analogy he adduced in its support, since he attributes to what he regards as the analogue of the ganglionic system in the articulated classes, the functions of sensation and voluntary motion which they possess in a higher degree than any other invertebrata. With all his errors, however, Bichat did much to fix attention on the important fact that there is a plurality of function and a corresponding plurality of structure, in the nervous system.

We must not omit to point out, also, the advance made by Prochaska towards the truth, in relation to the excito-motor system of nerves. Our readers will remember that his ideas on this subject were only brought to our notice after our former historical summary had been printed. At the conclusion of the same number (Vol. V., p. 623,) we gave from the original the whole chapter relating to it; and we shall take this opportunity of making a few observations on its import. In the first place we shall repeat the remark we there made, that Prochaska does not "notice the subject merely incidentally or as a matter of no importance, but that he clearly and explicitly announces the phenomena as depending on a distinct and important property of the nervous system, to which he devotes a whole chapter of his treatise." We desire that it should be borne in mind that the "complete anticipation" of which we there spoke related to the doctrine of the "reflex function of the spinal cord," which was the whole extent of what we were then inclined to admit as true in Dr. M. Hall's views; and we think it will appear that almost all that is deficient in the views of Prochaska on this subject is supplied by the discovery of Sir C. Bell. Now we by no means insinuate that Dr. M. Hall was acquainted with Prochaska's doctrines, when he commenced his researches. In fact, we believe his to be one of the cases in

which a little more knowledge of what had been previously taught would have greatly assisted him, without depriving him of his claim as a discoverer. But having the work containing them at his command, and professing in his second memoir to give a full account of the doctrines of other physiologists, we are surprised at the omission of all reference to them. We must not be understood as asserting that the *general* state of knowledge on the subject was much affected by Prochaska's publication. In fact, it seems to have excited little attention at the time, and to have been subsequently altogether lost sight of.

Now in his case, as in that of Whytt, we must adopt *his own* explanation of his terms, or we shall be altogether misled. The title of this chapter is an enquiry into the seat and functions of the *sensorium commune*. In modern writings this term is applied to the part of the nervous system where *impressions* become *sensations*; and if we set out with this notion we should find it difficult to interpret any part of his doctrines; but he takes care to define his meaning in the very first paragraph, where he explains the *sensorium commune* to be that part of the nervous centres at which *external impressions*, conducted to it by different nerves, give rise to *certain and determinate motions* by *respondent motor nerves.** To avoid confusion, we might substitute the term *centre of reflexion* for sensorium commune; our author having distinctly stated that he applies the latter term in accordance with received notions, to the *locus* in which *impressions are reflected* through the motor nerves.

This centre of reflexion he expressly limits to the *spinal cord*, *medulla oblongata*, *crura cerebri*, and *cerebelli*, and *part of the thalami optici*. The cerebrum and cerebellum he states to be the instruments which the mind immediately uses for its peculiar actions. He adverts to experiments on decapitated animals as proving the independent power of the spinal cord, which he speaks of as the *seat of the consent between the sensory*† *and motor nerves.*

Prochaska then goes on to point out that this *reflexion of sensory into motor impressions* does not follow mere physical laws, but *peculiar laws written as it were by nature on the medullary pulp of the sensorium;* and these laws he shows to be the preservation of the body from external injury, by the production of motions in respondence to external impressions, *tending to ward off and remove the source of injury;* and also the conservation of the body by the reflection of external impressions into motions tending to its benefit. Three examples are given, than the first two of which none could be more applicable; the motion of the respiratory muscles in sneezing, produced by irritation of the schneiderian membrane and directed to remove the source of irritation; the cough produced by irritation of the wind-pipe by a crumb of bread or a drop of liquid;—and the *involuntary* closure of the eyelid when the finger is brought *near* the eye‡, which is rather of the *emotional* character.

* All the phrases in italics are literal translations of Prochaska's expressions.

† That we are not to understand this term as involving *sensation* will be obvious from what follows. It is manifestly equivalent to *afferent*.

‡ One of Sir C. Bell's cases is a very interesting illustration of the double provision made for the protection of this important organ. The patient had amaurosis of one

He next states that, as the principal function of the sensorium commune consists in the reflexion of sensory into motor impressions, it is to be remarked *that this reflexion may take place whether the mind be conscious or unconscious of it.* Now it is quite true that, like Whytt, he seems to have attributed the movements of the heart and alimentary canal to reflexed action; and he has used these as illustrations of the above position. But he does not speak certainly on this point, and puts it as a query whether these actions are not the result of impressions reflexed through the visceral ganglia. This is a curious anticipation of Mr. Grainger's doctrine formerly referred to; but he does not rest his assertion upon these, for he refers in proof of it to the motions of apoplectic patients, *in whom all consciousness is suspended;* also to the convulsive movements of epileptic patients, *in which the sensorium commune acts without consciousness of the mind;* and to those which are witnessed in profound sleep, as well as to those of decapitated animals.* "All these actions," he continues, "arise from the organization and physical laws proper to the sensorium commune, and are *spontaneous and automatic.* Those of which the mind is conscious are either such as it cannot control by an effort of the will, or such as it can restrain or prevent at pleasure. The former, as they are ruled by the sensorium commune alone, in as far as it does not depend on the mind, are not less to be regarded as *automatic* actions than those which are performed unconsciously; such are sneezing from a stimulus applied to the nostrils, cough from a stimulus applied to the trachea, vomiting from irritation of the fauces, or from an emetic, tremor and convulsions in chorea S. Viti, and in the paroxysms of intermittent fever, &c. But the actions which the mind directs and moderates by its power, although the sensorium commune has its part in producing them, we call, nevertheless, animal, not automatic."

It is perfectly evident, then, that Prochaska had a very distinct idea of the *function of the spinal cord and medulla oblongata as a centre of reflected actions;* and that he specified the principal classes of these, referring them all to the excitement of impressions. We do not see what essential difference there is between this doctrine and that of the reflex function of the spinal cord as *first* propounded by Dr. M. Hall. The *subsequent* idea of a system of spinal or excito-motor nerves, distinct from the cerebral or sensori-volitional, is unquestionably due, we again repeat, to the latter gentleman alone. Indeed, it may be said to be more original with him than the idea of the distinctness of the sensory

eye, and anæsthesia of the other. When the finger was brought *near* the blind eye, no effect was produced on the muscles; but when the surface of the eye was *touched,* a reflex action through the afferent portion of the fifth pair and the portio dura excited the closure of the lid by the orbicularis. In the other eye, winking was occasioned by the *approach* of the finger; but, if the lid were held open until the surface of eye was *touched,* no contraction of the orbicularis was excited—the ophthalmic branch of the fifth being paralysed. The first, however, is rather an emotional action, intermediate between the voluntary and the simply reflex. The second is unquestionably reflex only.

* We do not see that Dr. M. Hall brought any further *proof* of this position, until he was able to adduce cases of paralysis in the human subject, in which reflex actions could be excited without sensation. Sir C. Bell had previously shown that the respiratory movements could be excited in muscles which were powerless as instruments of the will.

motor and motor nerves was to Sir C. Bell, since the latter had been frequently brought forward as a speculation; and as we have just seen, was distinctly stated (as a speculation only, however,) by Prochaska.

We must not forget to state that this chapter from Prochaska is quoted in full in the "Documents and Dates" and is accompanied by a translation which is generally faithful, though occasionally deficient in clearness, seemingly from the desire to keep as close as possible to the words of the original.

The doctrine of plurality of functions in the nervous system was carried to a much greater extent by Gall. Whatever may be the opinions of anatomists and physiologists respecting phrenology, it cannot be questioned that we owe much to Gall, and to those who worked under his guidance, for the rapid advance which was made about this time in the anatomy of the nervous centres, and in the knowledge of their true relations to each other. Gall had a strong conviction of the distinctness of the motor and sensory nerves; and used this as an illustration of his peculiar doctrines In the first volume of his Anatomy of the Nervous System, published in 1810, he urged this upon his adversaries; and refers to the fact of the fifth pair, which confers both sensibility and motive power, having three distinct roots, as an argument in favour of his doctrine of *plurality*. Perhaps the most important contribution which he made to our physiological knowledge was his demonstration of the true nature of the cerebral ganglia. By his predecessors, the brain had been regarded as the great centre of nervous energy; and the spinal cord as a sort of appendage or prolongation of it, possessed of some amount of independent power. By his researches in Comparative Anatomy, Gall was led to the belief that the spinal cord is really the central organ, and that the cerebellum and cerebral ganglia are developed upon it in proportion to the required predominance of their respective functions. He succeeded to a great extent in unravelling the structure of the latter; proving that they consist in part of fibres connecting the cortical substance with the medulla oblongata, and partly of commissural or connecting fibres proceeding in all directions, and uniting the different portions of the cortical substance to others in the same and opposite hemisphere. At the same time Reil was occupied with similar enquiries, specially directed towards the structure and functions of the cerebellum, and these, according to Dr. Elliotson, (*Physiology*, p. 330,) were suggested by Gall, and prosecuted on his plan.

We now approach the period when Sir C. Bell commenced his researches. There can be no doubt that his views were formed in ignorance of a great part of what we have thus brought together, and especially of the accounts which had been given of the anatomy of the roots of the fifth pair, and the analogy which had been pointed out between this and the spinal nerves. It must be remembered that, at the period of which we speak, there was much less communication among men of science than at present. A discovery of any kind was a long time in finding its way from one country to another; and the small facility given by the periodical press of those times to the diffusion of knowledge tended still more to restrict it within narrow limits. That Sir C. Bell was unacquainted with the statements and opinions of Paletta, Soemmering, and Prochaska, no one can have any reasonable doubt;

since the knowledge of them would not only have materially aided his enquiries, but would have saved him from the only serious error in which he has not corrected himself. That these statements had not excited general attention is sufficiently evident from the fact that Meckel took no notice of them in an elaborate memoir which he composed on this nerve; and that even he omitted to mention its origin by a double root. John Hunter, too, made the same omission, in a description which he had occasion to give of it. "In short," says Mr. Shaw, "I may observe, that I have not succeeded, after a careful search, in finding any single work in the English language, in which allusion is made to the true structure of the origin of this nerve, before Sir C. Bell commenced his investigations." That he should know nothing of Gall's Anatomy is not surprising when we recollect the distrust with which phrenology was at first received.

But we must not yet enter upon the history of Sir C. Bell's discoveries. A claimant remains to be disposed of, whom many of our readers perhaps never heard of but as the author of "Intermarriage," "Woman Physiologically Considered," "Theory of Beauty," and other works of similar erotico-physiological character. His title to discovery is thus set forth in the "Dates and Documents:"

> "To Mr. Walker's papers, then, we first proceed; as, whether they be right or wrong as to function, direction, or course, they are certainly those of by far the earliest writer who speaks of *nerves, spinal columns, and cerebral masses of sensation,—of the continuation of the action thence originating through the cerebrum,—and of other cerebral masses, spinal columns, and nerves of volition,—thus forming the great circle of nervous action and influence.*—This appears to be at least the first part of the discovery, 'the conception or idea,' as described by Professor Whewell." (p. 13.)

The first part of Mr. Walker's claims is that of assigning a function to the cerebellum, which it seems he did in a "Table of a Natural System of Medical Science," published in 1808. The cerebrum is here set down as the organ of perception, and the cerebellum as the organ of volition. This dogma, enunciated as it is without the support of a particle of evidence, is nothing more than a revival of the doctrine of Pouteau. We do not mean that Mr. Walker must necessarily have been acquainted with this; or that, if he had proved it by additional facts, he would not have been entitled to the merit of the discovery; but in its insulated form it is a mere speculation, and must rank as such.

We are next presented, however, with a series of extracts, from a paper published by Mr. Walker in April, 1809; and upon some parts of these we must bestow a little more attention. The first passage we shall quote will serve as a specimen of the philosophy of its author. "*That sensation* belongs entirely to the organs of sense, is *clearly proven by this, that several animals possessing a brain retain sensation* (*in this case improperly termed irritability*) *unimpaired long after it is removed.*" (p. 20.) We are afterwards informed that "*all the other intellectual functions are modifications of sensation.*" Again, "as sensation exists without volition, and as almost all nerves arise by distinct filaments, I am of opinion that, wherever a part, having both sensation and motion, is supplied from one nervous trunk, that trunk envelopes both a nerve of sensation and one of motion." Many had

anticipated Mr. Walker in the enunciation of that opinion; and as long as it remained unsupported by evidence, no advance was made. In a little work now before us, entitled Anatomia Britannica, and published in 1808, we find the following passage, which, succeeding as it does an account of the two principal terminations of the nerves—in muscles, and in sensory organs,—is to our minds quite satisfactory as to the opinion of its author. "Though the fibres in a nervous cord are firmly connected, and frequently nerves join into one trunk, or into the same ganglion, yet the sensation of each part of the body is so very distinct, and we have so much the power of moving the muscles separately, that, if the nerves are principal agents in these two functions, we have reason to believe that there is no union, confusion, or immediate communication of the proper nervous fibrils, but that each fibre remains distinct from its origin to its termination."* We have quoted this passage with a view of also showing that the statement made by Sir C. Bell in his first pamphlet,—as to the prevailing doctrine of the anatomical schools being that *the same* nerves which convey sensory impressions *to* the brain transmit motor impulses *from* it,—is rather too sweeping a generalization. But this is a matter of comparatively little importance; since neither one doctrine nor the other was ever *proved.*

We return, however, to Mr. Walker. His next assertion is that the *cerebellum and medulla oblongata* constitute the *organ of volition.* This opinion he states to be supported by "numerous observations;" but we find none which bears closely upon it, and no reference to experiment. Of the novelty of the dogma regarding the cerebellum we have already spoken; and the absurdity of limiting the function of the spinal cord to volition is self-evident. We are afterwards told that "the only apparent difference between the nerves of sensation and those of volition is, that their motions take place in different directions. The latter, therefore, may be said to resemble the arteries; the former the veins." The first part of this passage is a mere hypothesis; the second, which is printed in capitals on account of its supposed importance in establishing Mr. Walker's claims to discovery, contains nothing new. Hartley had long anticipated him in the speculation. In his Essay on Man may be found a beautiful hypothesis (of which this is the basis), plausibly enunciated, and pursued into extensive application. This we should recommend to Mr. W.'s perusal, as it may furnish him with some valuable hints for his next work.

A second paper by Mr. Walker next presents itself among the Dates and Documents, of more pretensions than the former one. The title of this is a little remarkable—"New Anatomy and Physiology of the Brain in particular, and of the Nervous System in general." It is contained in the Archives of Universal Science; and the date of its publication is July 1809. This cannot be passed over as summarily as the other; for it certainly does contain a very remarkable *anticipation* of Sir C. Bell's views;—an anticipation, however, which was dogmatic only, which was never subjected to the test of experiment, which never led the author fur-

Vol. iii., p. 238. Since this paper has been in type we have learnt that Mr. Walker is himself the author of the Anatomia Britannica. He was, however, by no means singular in the opinion there enunciated.

ther, and with which we have no proof that Sir C. Bell had become acquainted, when he commenced his more successful researches. It is due to the author, however, to present it in full.

"The medullary matter may be traced as continued from the portions of many nerves which join the two anterior columns of the spinal marrow, upward through these columns to the inferior fasciculi of the medulla oblongata, forward through the crura cerebri, outward and upward through the corpora striata, and the hemispheres of the cerebrum. From the hemispheres it passes posteriorly, backward, inward, and downward through the thalami, backward through the striæ inferior to the nates and testes, and backward and upward through the processus cerebelli ad testes, or the anterior peduncles of the cerebellum to the substance of the cerebellum itself. Lastly, from the cerebellum it passes downwards by the corpora restiformia, superior fasciculi of the medulla, processus cerebelli ad medullam, or posterior peduncles of the cerebellum to the posterior columns of the spinal marrow, and the remaining portions of the numerous nerves which join it. Thus the medullary fibres form a most remarkable circle, of which this is the direct course." (pp. 32-3.)

In this description, which is by no means anatomically correct, Mr. Walker was anticipated by Gall, who had been for some years in the habit of demonstrating the course of the fibres of the brain in a manner generally similar. Let it be observed that he had nothing but speculation to guide him in assigning to the two sets of fibres an *ascending* or *descending* course; and that his speculation was wrong. It is in his physiological explanation that we meet with the chief novelty, that of assigning motor and sensory functions respectively to the two origins of the spinal nerves; and here, too, we find the basis upon which the assignment was founded.

"But it may be questioned by which nerves, columns, and cerebral masses, the action ascends to the brain, and by which it descends to the muscles. Fortunately, here nature also directs us. Several nerves of mere sensation join the anterior masses; hence they must be the ascending: one nerve of mere locomotion proceeds from the posterior masses; hence they must be descending—for sensation, as already said, must ascend to, and volition must descend from, the sensorium commune.

"Thus, then, it is proved to us, that medullary action commences in the organs of sense; passes, in a general manner, to the spinal marrow, by the anterior fasciculi of the spinal nerves, which are, therefore, nerves of sensation, and the connexions of which with the spinal marrow or brain must be termed their spinal or cerebral terminations; ascends through the anterior columns of the spinal marrow, which are, therefore, its ascending columns; passes forward through the inferior fasciculi of the medulla oblongata, and then through the crura cerebri; extends forward, outward, and upward through the corpora striata, and reaches the hemispheres of the cerebrum itself. This precisely is the course of its ascent to the sensorium commune. From the posterior part of the medulla of the hemispheres, it returns by the thalami, passing backward, inward, and downward; flows backward in the fasciculi under the nates and testes; backward and upward through the processus cerebelli ad testes, or anterior peduncles of the cerebellum; and thus reaches the medulla of the cerebellum itself. From the cerebellum, it descends through the posterior column of the spinal marrow, which are, therefore, descending columns; and expands through the posterior fasciculi of all the nerves, which are, therefore, the nerves of volition, and the connexions of which with the spinal marrow or brain must be termed their spinal or cerebellic origins." (pp. 35-6.)

Now it cannot for a moment be questioned by any one who reads this

passage, that its author had the *idea* of a difference in the functions of the fibrils of the same nervous trunk, depending on a difference in the part of the central organs with which they are connected; and that this difference is manifested in the spinal nerves by the connexion of each at its root with two distinct columns of the spinal cord, one a prolongation of the cerebrum, and the other of the cerebellum; whilst in the head there existed nervous trunks separately appropriated to these functions, and directly connected with the two central organs. But the *value* of the idea depends upon the evidence adduced in its support, and the use subsequently made of it. Let us enquire what amount it can claim on these grounds. Mr. Walker's idea of a distinctness in the functions of the roots of the spinal nerves is founded upon two suppositions. First, that the anterior and posterior columns are continuous with the cerebrum and cerebellum respectively. This, every anatomist now knows to be incorrect; both columns being connected with each organ. Secondly, that the cerebrum is the organ of sensation, and the cerebellum of volition. This opinion has now been abandoned by every physiologist of reputation; the few who maintain that such an antagonism exists between the functions of the two masses, being of opinion that the cerebellum is the organ of sensation, and the cerebrum of volition; and the greatest number adopting the opinion of Rolando and Flourens, that the cerebral hemispheres are the organs both of sensation and volition, whilst the cerebellum is the organ by which the separate motive impulses (whether voluntary or involuntary) are harmonized and controlled, so as to produce the more complex actions. Whatever be the shades of difference on the latter point, there is little or none on the former. If *either* of the two positions on which Mr. W's. hypothesis was founded, be proved to be untenable, the doctrine falls to the ground, since one support is nothing without the other; but when *both* are shown to be fallacious, not a shadow of a proof upholds it.

Still the *idea* may be regarded as an ingenious speculation; and had it been subjected to the test of experiment, it might have conducted its author to the truth. But has it been thus fruitful? So far from it, that the author, relying upon its self-evident merits, took no pains to perfect and substantiate his supposed discovery; and nothing more of him or it (as far as we know) was heard by the scientific world in this country, until the publication of his work on the subject in 1834, in which he accuses Sir C. Bell, and all who have been engaged in the same enquiry, of having stolen his discovery from him.

We have shown then, that his *idea*, however ingenious as a speculation, had no real value either in itself, or as having led to other discoveries. We will suppose for a moment, that Bell, Magendie, Mayo, &c., had never existed, and that Mr. Walker's opinions had gained the public credence to which their author considers them entitled: unless some more acute physiologist had corrected the error, we should have been now in a worse position than when everything was regarded as uncertain; and we should have had more difficulty in finally attaining truth, since every one knows that the overthrow of old doctrines is often a far more laborious process than the establishment of new ones. The *only* way in which it seems to us possible that Mr. Walker's speculations can have contributed to the advancement of science, is that they

may have suggested to Sir C. Bell the course of investigation which he has followed up with such gratifying success. But, independently of other considerations we feel assured by the internal evidence of Sir Charles's first pamphlet, that his views were entirely original.

We now enter upon the period embraced by Mr. Shaw's history, of which we shall in future make frequent use, commenting freely both upon its statements and its deficiencies. The first point we shall advert to, is one which has already come under our review, and which we shall now merely notice. It is the statement of the author, coinciding with that of Sir C. Bell, as to the degree of previous ignorance on the subject. We have shown that many physiologists entertained at different times the opinion that the motor and sensory fibrils were distinct, and terminated in different parts of the nervous centres; and that this was expressed in an anatomical work published at that very period. We do not think that it derogates in the least degree from Sir C. Bell's merits as a discoverer to make this admission; and we think it better, that it should be stated broadly and fully, in order that claims to more complete anticipations may be at once refuted. It is only by gaining clear notions on the state of previous knowledge that the true merits of any discoverer can be appreciated. That the floating opinions to which we have just referred could not be regarded as *knowledge*, is evident, from the great diversity which existed amongst them. One speculation, however, had evidently more influence on Sir C. Bell's mind than he was at first aware of; it is that of John Hunter, that the nerves have different functions, and that the complexity of their distribution will be explained when these are thoroughly known. Sir Charles has duly acknowledged his obligation to his great master, in a note to the second paper on the nerves of the orbit. (3d edition, p. 188.)

We entirely concur with Mr. Shaw in the statement which he has very properly prefixed to his historical details, that Sir C. Bell's chief merit as a discoverer consisted in directing attention to the *roots of the nerves*, as the part of the whole nervous system most adapted to afford *information to the experimenter*. If it be true that these have different functions, the physiologist is enabled to ascertain them by rigid tests; and he may thus more clearly determine the functions of the mixed trunk into which they unite, and by tracing further back, infer the office of the portion of the nervous centres with which they are respectively connected.

> "Here then is a simple explanation of the principle on which all these new discoveries have been based. It consists, I repeat, in supposing that, to investigate the functions of the nervous system successfully, we must devote our attention, not to the trunks, as was formerly done, but to the roots of the nerves. Accordingly, whoever was the first to suggest and follow out that new method of prosecuting the subject, must be declared the true originator of the recent improvements in this department of physiology. It is by the test of who did the most to establish this law? that we must decide to whom we are indebted for these discoveries." (*Narrative*, p. 7.)

Now here we feel that Sir C. Bell can fairly meet Mr. Walker. Did the latter enunciate this principle? It may be said that it flows self-evidently from his doctrine. But we know that the most self-evident inferences often remain undeduced. Was this the case? We answer without hesitation that it was. During *twenty-five years'* investigation of the

subject, Mr. Walker made but *one experiment*, the results of which coincided with those obtained by Sir C. Bell; and finding that this method revealed facts inconveniently opposed to his opinions, Mr. W. very judiciously abandoned it.*

Some confusion exists as to the time when Sir C. Bell first propounded his new views. In his own volume, as well as in Mr. Shaw's history, this date of the *printing* and *circulation* of the "Idea of a New Anatomy of the Brain" is fixed at 1811; whilst in Mr. J. Shaw's Essay on Paralysis of the Nerves of the Face,† it is stated as 1809; and this statement was repeated in the discussion with Magendie. This requires some explanation; but it is evident that, if there be any real uncertainty arising from the want of a date attached to the paper (which was never published), Sir C. Bell and his present historian have erred on the safe side. We have long had a curiosity to meet with this essay. The first ideas on which a grand discovery is founded are always interesting; and to trace the progress of their development is a most instructive exercise. Who that has read the account of the investigations which conducted Davy to the invention of the safety-lamp, has not felt a sympathetic delight in the gradual unfolding beneath his search of the important principle which is there so beautifully applied? We must confess that we should have been glad to see this essay prefixed to the collection of memoirs, read before the Royal Society. The very fact of its not having been published adds to the reasons why it should have appeared there; since, if it be appealed to for historical data wherewith to establish Sir C. Bell's claim to public reputation, the public have a right to know *all* that it contains. We still more regret that Mr. A. Shaw has not thought fit to publish it entire, but has contented himself with such extracts as suited his purpose. These extracts, we feel it our duty to state, unequivocally, *do not give a fair view* of the essay. When we rose from the perusal of this part of Mr. Shaw's history, it was with the feeling of unmingled astonishment that Sir C. Bell's original opinions could have been so boldly misrepresented, as it would seem from it that they have been. But when an opportunity was furnished us by the "Dates and Documents" of making ourselves acquainted with the whole paper, we at once perceived the sources of those statements, and saw with regret, as far as Mr. Shaw is concerned, and with satisfaction in regard to the individual whom he chiefly criminates, that they could scarcely be regarded as misrepresentations, although they erred in taking a one-sided view of Sir C. Bell's opinions. As to Sir C. Bell himself, we gladly state that the perusal of this paper, with all its errors and imperfections, has *not in the least degree* lowered our estimate of his merits as a discoverer,—and for this reason: the *principle* is there plainly and unequivocally enunciated which guided all his subsequent researches. They may fairly, therefore, date back to this period; and we also trace that philosophic spirit, so opposed to the dogmatism we have elsewhere encountered, which prevented him from hasty decision, whilst it also pointed out the road to more certain information. We should not do

* We make this statement on the authority of Dr. Fletcher, (*Rudiments of Physiology*, Part ii., *b*, p. 102,) whose statements, being referred to by the editor of the "Dates and Documents," may, we presume, be regarded as authentic.

† Medico-Chirurgical Transactions, vol. xii., p. 149.

him justice, if we did not quote a passage or two in illustration of this general character. The first is from his introductory remarks.

" When in contemplating the structure of the eye, we say, how admirably it is adapted to the laws of light, we use language which implies a partial, and consequently an erroneous view; and the philosopher takes not a more enlarged survey of nature when he declares how curiously the laws of light are adapted to the constitution of the eye. Thus, creation of which we are a part, has not been formed in parts. The organ of vision, and the matter or influence carried to the organ, and the qualities of bodies with which we are acquainted through it, are parts of a system, great, beyond our imperfect comprehension, formed as it should seem at once in wisdom; not pieced together like the work of human ingenuity. When this whole was created (of which the remote planetary system as well as our bodies, and the objects familiar to our observation, are but parts,) the mind was placed in a body not merely suited to its residence, but in circumstances, to be moved by the materials around it; and the capacities of the mind, and the powers of the organs, which are as a medium betwixt the mind and the external world, have an original constitution framed in relation to the qualities of things." (*Documents and Dates*, p. 41.)

These remarks are followed by some sagacious distinctions between the different kinds of sensation; the object of which is to show that these depend, not only on the difference of the organ in which the impressions originate, but on the different endowments of the nerves conveying them, and on the diversity of function in the portions of the central organ with which they are respectively connected. These observations appear intended to introduce and illustrate the main argument of the essay, that the brain, instead of consisting of parts having similar endowments, is composed of numerous organs, ministering to distinct offices; and they conclude with the following admirably-expressed and philosophical views:

" Thus we see that, in as far as is necessary to the great system, the operations of the brain, nerves, and muscles, are perfect from the beginning; and we are naturally moved to ask, might not the operations of the mind have been thus perfect and spontaneous from the beginning, as well as slowly excited to action by outward impressions? Then man would have been an insulated being, not only cut off from the inanimate world around him, but from his fellows; he would have been an individual, not a part of a whole. That he may have a motive and a spring to action, and suffer pain and pleasure, and become an intelligent being, answerable for his actions, sensation is made to result from external impression, and reason and passion to come from the experience of good and evil; first, as they are in reference to his corporeal frame, and finally as they belong to the intellectual privations and enjoyments." (*Ib.*, p. 47.)

The study of men's characters from their writings is a most interesting employment. When pursued with acuteness and discrimination, it often leads to far juster impressions than personal intercourse produces; such is our feeling in the present instance. What mind capable of appreciating such a passage as that just quoted would not say—" Set that man upon a right track, and he will be the discoverer of important truths?" Let us see whether its author succeeded in finding such a path.

The whole object of the essay is evidently the establishment of the doctrine of a plurality of endowments in different nervous trunks, and in different parts of the same trunk; these endowments being possessed

by them in virtue of their connexion with different portions of the central organs. In this manner it was conceived by the author that the complexity of the distribution of the nerves, their union with one another, and many similar points of interest and difficulty to the anatomist, might be satisfactorily explained; but as long as he confined himself to mere argument and speculation, it is manifest that he only advanced upon the doctrines of others in the additional reasons he adduced. This, however, was not all. Bell perceived the deficiency, and his sagacity pointed out the means of remedying it. Our next extract gives his own statement in full, and it is sufficiently apparent, that his mode of investigation was completely opposed to the dogmatic assumption of Mr. Walker, who reasoned downwards to the roots of the nerves from the false position with which he started. It will be observed that, in common with Mr. W., Sir C. Bell entertained an incorrect, or rather an imperfect, view of the connexion of the brain and spinal cord; and this, as we shall hereafter see, was one cause of the errors of detail into which he was at first led.

"In thinking of this subject, it is natural to expect that we should be able to put the matter to proof by experiment. But how is this to be accomplished, since any experiment direct upon the brain itself must be difficult, if not impossible? I took this view of the subject. The medulla spinalis has a central division, and also a distinction into anterior and posterior fasciculi corresponding with the anterior and posterior portions of the brain. Further, we can trace down the crura of the cerebrum into the anterior fasciculus of the spinal marrow; and the crura of the cerebellum into the posterior fasciculus. I thought that here I might have an opportunity of touching the cerebellum, as it were, through the posterior portion of the spinal marrow, and the cerebrum by the anterior portion. To this end I made experiments, which, though they were not conclusive, encouraged me in the view I had taken. I found that injury done to the anterior portion of the spinal marrow, convulsed the animal more certainly than injury done to the posterior portion; but I found it difficult to make the experiment without injuring both portions.

"*Next, considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connexions with the parts of the brain, I thought I had an opportunity of putting my opinion to the test of experiment, and of proving at the same time that nerves of different endowments were in the same cord, and held together by the same sheath.*" (*Ib.*, pp. 50-1.)

This is the grand principle of Bell. It seems simple enough when it is pointed out to us; but had any one even hinted at it before? Such importance do we attach to it, that we consider its author as entitled to the merit of whatever discoveries might be made by following it out, excepting, of course, that share which belongs to the ingenious and discriminating experimenter. Supposing that Sir C. Bell had died or had been prevented from pursuing his physiological researches, before he had himself established any detailed results, would it not have been fairly asserted that he was *de facto* the author of whatever improvements might be introduced into the science by the adoption of his method? To us it seems as if he had thus unlocked a secret spring which allowed the chest to be opened and its contents examined, or found the concealed thread by which the labyrinth of truth might be explored. But let us again refer to his essay.

"The spinal nerves being double, and having their roots in the spinal marrow, of which a portion comes from the cerebrum and a portion from the cerebellum,

they convey the attributes of both grand divisions of the brain to every part, and therefore the distribution of such nerves is simple, one nerve supplying its distinct part. But the nerves which come directly from the brain, come from parts of the brain which vary in operation; and in order to bestow different qualities on the parts to which the nerves are distributed, two or more nerves must be united in their course or at their final destination; hence it is that the first nerve must have branches of the fifth united to it; hence the portio dura of the seventh pervades everywhere the bones of the cranium to unite with the extended branches of the fifth; hence the union of the third and fifth in the orbit; hence the ninth and fifth are both sent to the tongue: hence it is, in short, that no part is sufficiently supplied by one single nerve unless that nerve be a nerve of the spinal marrow, and have a double root, a connexion (however remotely) with both the cerebrum and cerebellum. Such nerves as are single in their origin from the spinal marrow will be found either to unite in their course with some other nerve, or to be such as are acknowledged to be peculiar in their operation. . . . . .

"Understanding the origin of the nerves in the brain to be the source of their powers, we look upon the connexions formed betwixt different nerves, and the combination of nerves in their passage, with some interest; but without this, the whole is an unmeaning tissue. Seeing the seeming irregularity in one subject, we say it is accident, but finding that the connexions never vary, we say only that it is strange, until we come to understand the necessity of nerves being combined, in order to bestow distinct qualities on the parts to which they are sent." (*Ib.*, pp. 51-3.)

What can be conceived more clear and explicit than this statement of the *general principles* on which Sir C. Bell desired that subsequent enquiries should be pursued? Had he been desirous of preventing any others from entering upon it, he would have scarcely placed his "Idea" in the hands of those members of the profession in Great Britain and Ireland, whom he thought most likely to aid in its development. We can scarcely help feeling surprised at the apathy with which it seems to have been received; there being no evidence that any one partook in the subsequent enquiry but Sir C. Bell's own pupils. We shall presently show that he completed the *general structure* for which he here laid so good a foundation, without receiving any assistance from external sources. And as it was entirely raised upon this foundation, the commencement of the building (to carry out our metaphor) must unquestionably be dated from the time when it was thus established, although the progress of the work was for a long time concealed beneath the ground, and although the courses first laid were imperfectly fixed, and had to be removed and replaced by others.

Such being our view of the merits of this essay, it seems to us that Mr. Shaw has underrated them, in keeping back, as he has done, those parts of it which show that Sir C. Bell made in the first instance an imperfect and erroneous application of his own principle. These do not appear to our minds to derogate in any material degree from his claims as a discoverer; and, by partially suppressing them, he has thrown the worst imputation on an individual, who, with all his faults, is scarcely capable, we trust, of the misrepresentation with which he is thus virtually charged. Let us see how the case stands.

The only experiment on the roots of the nerves recorded in this essay is the following:

"On laying bare the roots of the spinal nerves, I found that I could cut across

the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but that, on touching the anterior fasciculus with the point of a knife, the muscles of the back were immediately convulsed. Such were my reasons for concluding that the cerebrum and the cerebellum were parts distinct in function, and that every nerve possessing a double function obtained that by having a double root." (p. 51.)

Now, *were this all* that is said on the subject, Mr. Shaw would be perfectly justified in saying, as he has done, that, whilst Sir C. Bell affirmed in sufficiently positive terms, that the anterior root was alone capable of exciting the muscles to contraction, he has refrained from saying directly to which root sensation belongs (p. 44). But we find passages in the essay, which show that he had formed an opinion on the subject; and these we feel it our duty to quote in full, slightly varying their order, that we may exhibit more clearly their bearing on one another.

"The cerebrum I consider as the grand organ by which the mind is united to the body. Into it all the nerves from the external organs of the senses enter, and from it all the nerves which are the agents of the will pass out.

"The nerves proceeding from the crus cerebelli go everywhere (in seeming union with those from the crus cerebri); they unite the body together, and control the actions of the bodily frame, and especially govern the operation of the viscera necessary to the continuance of life." (p. 54.)

Of these two classes of nerves an illustration had been previously given in the eighth and ninth pairs. "The first is a nerve of the class called vital nerves, controlling secretly the actions of the body; the last is the motor nerve of the tongue, and is an instrument of volition." The functions of the two central organs being thus assigned, the anatomical statements of their relations previously given would leave no room for doubt that similar functions would be assigned to the anterior and posterior roots of the spinal nerves, but the author has himself deduced the inference.

"From the medullary matter of the hemispheres there pass down, converging to the crura, striæ, which is the medullary matter taking upon it the character of a nerve; for from the crura cerebri, or its prolongation in the anterior fasciculi of the spinal marrow, go off the nerves of motion. But *with these nerves of motion*, which are passing outward, there are nerves going inwards, nerves of touch, and nerves of peculiar sensibility, having their seat in the body or viscera. It is not improbable that the tracts of cineritious matter which we observe in the course of the medullary matter of the brain, are the seat of such peculiar sensibilities, the organs of certain powers which seem resident in the body." (p. 56.)

"The secret operations of the bodily frame, and the connexions which unite the parts of the body into a system, are through the cerebellum and nerves proceeding from it." (p. 60.)

Now it is a little curious that, whilst these early ideas of Sir C. Bell's on the functions of the cerebrum and cerebellum should so nearly correspond with the present state of opinion respecting them, he should have been so greatly misled by his erroneous view of their anatomical relations, in assigning corresponding functions to the anterior and posterior fasciculi of the spinal cord. It is evident that he arrived at these conclusions by reasoning on the supposed termination of the

cranial nerves in these organs, and that he interpreted the uncertain result of his experiment by the views he had thus formed. We assert, then, that Mr. Mayo was *not* guilty of the misrepresentation imputed to him by Mr. Shaw, in stating, as he has done,* that "Sir C. Bell was carried by these experiments very near to the truth, but he failed at that time to ascertain it; he inferred from his experiments, indeed, that the anterior and posterior roots of the spinal nerves have different functions, but in the nature of these functions he was mistaken. Upon the anterior root he supposed both motion and sensation to depend: the posterior root he considered an unconscious nerve, the office of which was to control the growth and sympathies of parts." On the contrary, we assert, that Mr. Shaw has been guilty of a positive but no doubt unintentional *suppressio veri*, in omitting all reference to the passages we have quoted, when professing to give a full exposition of Sir C. Bell's early views, for the purpose of showing their inconsistency with Mr. Mayo's statement.

But, in the sentence immediately following the passage just quoted from Mr. Mayo's Physiology, we find the following assertion: "*Before Sir C. Bell published any other account of the functions of these nerves, M. Magendie had given to the world the true theory of their uses.*" This is *verbally* but not *actually* true, as the course of our history will now fully prove.

Experiments on the roots of the spinal nerves alone are not likely ever to lead to satisfactory results, especially in regard to their *sensorial* character. The causes of the difficulty, originating in the violent nature of the operations concerned, are fully explained in Mr. Shaw's work, (pp. 44-6); and it is evident that Sir C. Bell was strongly impressed by them, for he abandoned this mode of enquiry for one which promised more certain information, whilst still pursued *on the same principle.* We have already noticed the contrast which he drew between the very *regular* distribution of the spinal nerves, and the very *irregular* course of the cranial; and it has been seen that he attributed the latter to a difference in their characters, corresponding with that which be believed to exist between the two roots of the spinal nerves. Nothing was so natural, then, as his recourse to the cranial nerves for the determination of the question which he could not solve by experiment on the roots of the spinal; and that this was the plan he followed, we learn not only from his own history (prefixed to his memoirs), but from the papers published during the course of the investigation by his brother-in-law and assistant, the late Mr. John Shaw, to which we shall have frequent occasion to refer.

The experiment on the spinal nerves had given satisfactory indication that the anterior column of the spinal cord is the portion specially connected with *motion.* Following it up to the crus cerebri, it was perceived that the ninth, sixth, and third nerves, arise by single roots from it. These were well known to be purely *muscular* nerves in their distribution; and the belief thence arose, that the anterior fasciculi of the spinal cord were not only peculiarly but *exclusively motor.* "The

* Outlines of Physiology, fourth edition, p. 249.

inference then presented itself," says Sir C. Bell, "that the posterior column and the posterior roots were for sensibility. But here a difficulty arose. An opinion prevailed that ganglions were intended to cut off sensation; and every one of these nerves, which I supposed were the instruments of sensation, have ganglions upon their roots. Some very decided experiments were necessary to overturn this dogma." Thus arose the series of experiments on the fifth pair, and portio dura of the seventh, which not only led to the establishment of the true functions of the roots of the spinal nerves, but also contributed towards the doctrine of the respiratory system.

But before entering upon these, we may observe, that the passage just quoted harmonizes fully with what we have stated of Sir C. Bell's early opinions. A vague notion was prevalent, that the posterior roots were connected with the "Vital and involuntary motions," in which sensation did not participate; and that the cerebellum, which was supposed to be their anatomical centre, was also the centre of these actions. Now, in the absence of any decided experimental results, it is quite evident to us, that Sir C. Bell did not at first feel justified in attacking this opinion, but acquiesced in it. It is not, therefore, a fair representation of his views, to say, that he drew such an inference from experiment; the fact being rather, that his first experiments did not contradict it. But the first which he performed on the cranial nerves were especially directed to the overthrow of the dogma, which impeded the general reception of the inference at which his own mind had arrived.

Without entering into minute historical details, we shall enquire what were the opinions of Sir C. Bell on the functions of the fifth and seventh nerves, as expressed either by himself or by Mr. J. Shaw previously to the publication of Mr. Mayo's *first* Series of Anatomical and Physiological Commentaries, in August, 1822. The sources from which we gather them are Sir C. Bell's first Memoir on the Nerves, read before the Royal Society on the 12th of July, 1821; Mr. J. Shaw's Manual of Anatomy, published in September, 1821; an Essay, by the latter gentleman, "On the Difference in the Functions of the Nerves of the Face," in the Quarterly Journal of Science, December, 1821; another by the same, "On the Effects produced on the Human Countenance by Paralysis of the different systems of Facial Nerves, April, 1822;" and a paper, by the same, on "Partial Paralysis," in the Medico-Chirurgical Transactions for April, 1822. No one, we believe, who is acquainted with the circumstances of the case, will hesitate in regarding Mr. J. Shaw as Sir C. Bell's mouthpiece at this period, and in assigning to the latter all the opinions on the subject of the nerves put forth at intervals by the former. We deem it right to quote Sir C. Bell's own reason for the form in which the results of his enquiries were first presented to the public.

"In proceeding to give some account of these new observations, the author of this paper had conceived that it would be more suitable to the scientific body he had the honour to address, to lay the subject before them in the precise form in which it first presented itself to his enquiries, and to detail his observations and experiments in the order in which they were made; but he has been persuaded by some of the members of this Society to change the form, and to present the subject in the manner to which he was accustomed in teaching these

doctrines. They were pleased to say, that in this way a new subject would be more readily comprehended."

In a note to the present edition, the author adds:

"I believe that attention could not be raised to this great subject by the account of a system founded on anatomy, and on the minute distinctions in the origins of the nerves. It required the announcement of some distinct and remarkable facts, such as this paper contains, to excite enquiry." (p. 34.)

The adoption of this plan we believe to have been the source of much misconception; the author having, in his desire to direct attention to some prominent and novel points, left others unnoticed, and thereby given a handle to his opponents for the assertion that he was unaware of them. Thus he characterized the portio dura of the seventh pair as a "respiratory" nerve; and if he did regard it in any other light, he certainly did not so express himself. He almost passed by the question of the roots of the spinal nerves, referring to it only by the way; and it has thence been asserted that he left it as it lay after the first statement of his views on the subject. This is the very opposite of truth, as we shall now demonstrate.

The first point we meet with is the recognition of the analogy between the fifth pair and the spinal nerves. This, to be sure, is not new, since Soemmering and other anatomists had advanced the same doctrine. But it happens that it is very important to our argument to show that Sir C. Bell did not only recognize it at this time, but proceeded on the strength of it. For, if this be the case, we may safely assert that he regarded as true of the two roots of the spinal nerves that which he concluded from observation and experiment to be true of the two roots of the fifth pair. The double origin, the existence of a ganglion on one of the roots, the distribution both to muscles and integuments, and the connexion with the functions both of sensibility and voluntary motion, are dwelt on by Sir C. Bell and Mr. Shaw in support of this analogy; as also the similar connexion which they possess with the sympathetic. The anatomical description, however, of the origin of the fifth pair is very imperfect. Sir C. Bell states that "it receives roots both from the medullary process of the cerebrum and of the cerebellum. A ganglion is formed upon it near its origin, though some of its filaments pass on without entering the ganglion. Before passing out of the skull, the nerve splits into three great divisions, which are sent to the face, jaws, and tongue. Its branches go minutely into the skin, and enter into *all* the muscles, and they are especially profuse to the muscles which move the lips upon the teeth." It is evident, therefore, that Sir C. Bell was ignorant of the previous statements of Paletta, Soemmering, and Prochaska relative to the exclusive distribution of the ganglionless root to the muscles of the lower jaw; had he been acquainted with them, he would have been saved from the only error he committed in assigning the functions of the fifth pair.

In all the papers we have referred to, the fifth pair is spoken of as the exclusive source of the *sensibility* of the face. Although it is nowhere pointedly stated, it is evident from the contrast repeatedly drawn between the fifth and seventh nerves, that Sir C. Bell considered the former to possess a sensory character in virtue of its ganglionic root.

Of this we do not entertain the smallest doubt; and we think that no candid person who examines the evidence can avoid arriving at the same conclusion. Remembering, then, the analogy drawn by him between the fifth and spinal nerves,—bearing in mind the continuity which he believed to exist in the tracts from which these two roots respectively originated,—and the fact that he had recourse to the cranial nerves for the special purpose of clearing up what was left obscure in the spinal,—are we not justified in asserting that he thus *virtually* assigned sensibility as the function of the posterior roots of the spinal nerves? If this be admitted, it follows that the claim of M. Magendie, and of Mr. Mayo in his behalf, falls to the ground; since the experiments of the former were not published until August 1822.

If any doubt could remain on this subject, it is removed by Mr. Shaw's paper in the Medico-Chirurgical Transactions already referred to, and also by a statement contained in his Manual of Anatomy. Here the doctrine that the two origins of the spinal nerves are subservient to motion and sensation respectively is *most distinctly implied;* but, curiously enough, it is not pointedly expressed. On this subject our readers shall judge for themselves:

"I shall take the liberty of trespassing still more upon the time of the Society, by making a few remarks upon a very curious question, which has particularly excited the attention of physicians in all ages since the time of Galen. 'Why sensation should remain entire in a limb, when all voluntary power over the action of its muscles is lost, or why muscular power should remain when feeling is gone?' The attention of Galen was particularly directed to this question, in consequence of his having been called on by his contemporaries to account for the manner in which he had cured a paralysis of the finger, by applications made to the spine. In answer, Galen told them, that two sets of nerves went to every part; one, to endow the skin with sensibility, the other, to give the muscles the power of voluntary action. This opinion was probably founded on an erroneous theory; but the facts lately discovered, and the observations which have been noted in attending to the phenomena of disease, though they do not afford absolute proofs of the correctness of Galen's supposition, still go far to establish the fact, that every part of the body which is endowed with two or more powers is provided with a distinct nerve for each function.

"The form of the nerves, which at the same time endow the skin with sensibility, and the muscles with the power of voluntary motion, is such, that they appear to be single cords; but, if we examine the origin of any of these nerves, we shall find that it is composed of two packets of fibres, which arise from distinct parts of the spinal marrow. These origins are soon enveloped in the same sheath, so as to appear to a superficial observer to form a single nerve. It is not too much to suppose, that either of these origins may be affected while the other remains entire. To prove this by ocular demonstration will perhaps be impossible; and, therefore, the question will probably remain undecided. But we have already seen examples of the consequences of injury to a nerve that has a single root, viz., the portio dura; for, if we cut it, there will be only one set of actions paralysed, while, by dividing a nerve which has a double origin, viz., the fifth, we shall destroy two powers, namely, voluntary motion and sensibility. We know, also, that when we cut through the trunk of a nerve going to the hand, we destroy both sensibility and voluntary motion." (*Shaw's History*, p. 13.)

Mr. Shaw then refers to the early experiments of Sir C. Bell on the spinal nerves, and states the uncertainty in which they left the question

of the source of their sensibility. But, it being remembered that the object of his Essay was to explain the functional differences of the fifth and seventh nerves in accordance with their origins, he evidently implies that the doubt is in his mind cleared up, and he thus concludes:

"If the view which I have here taken of this question be correct, it may lead to this rule of practice. If only one set of functions of a spinal nerve be deficient, we should apply our remedies to that part of the system from which the nerve arises; but if both functions are impaired, we must then direct our enquiries to the state of the nerve in the whole course from its origin to its distribution, as the loss of power is probably owing to some affection of a part of the nerve, after the two sets of filaments by which it arises are united together." (p. 15.)

In his Manual of Anatomy, Mr. J. Shaw was still more precise as to the functions of the respective roots of the spinal nerves, and that his meaning was understood is evident from the following account of his views given by Magendie in his Journ. de Physiol. Oct. 1821: "Every nerve that belongs to the regular system arises by two separate roots, one from the anterior and one from the posterior column. . . . . There are thirty-two perfect, regular, and double nerves, common to man and the lowest animals, and each possessing motion on the one hand, and sensation on the other."

In concluding, then, this part of our history, we confidently assert that, at the period of the publication of his first memoir, Sir C. Bell had abandoned the notion of the functions of the roots of the spinal nerves with which he started in 1811; and that he had arrived at the conclusion, that the *anterior* roots of the spinal nerves and the small root of the fifth pair are *motor* in their function, and the *posterior* sensory. To any one who still objects that this *change is nowhere expressed*, we would only speak thus: The first essay not having been *published*, Sir C. Bell did not feel it incumbent on him to avow any change, however prudent it might have been to do so. He may still fairly refer to it for the enunciation of *his principle*, which has undergone *no change*. The whole internal evidence supports the view that he had by this time formed the opinion of the sensory character of the posterior roots of the spinal nerves. On the contrary, let any one read the papers we have referred to, with the opinion that the posterior roots of the spinal nerves had in Sir C. Bell's estimation *any other function than sensibility*, and he will be constantly unable to harmonise it with what he there encounters. A useful lesson may hence be learned by those who are desirous of making known their discoveries and securing their title to them. Let the previous knowledge on the subject be fully ascertained and detailed, and the supposed improvements systematically enunciated, without leaving anything open to doubt, excepting such points as are intentionally reserved for future examination. Had Sir C. Bell done this, instead of yielding to the wishes of his friends, much angry discussion would have been saved.

In regard to the functions of the *fifth pair*, no one who reads Sir C. Bell's original memoir can feel the smallest doubt that its author entertained erroneous notions. His ignorance of the minute anatomy of the roots led him to believe that, as in the case of the spinal nerves, the motor and sensory branches accompanied each other throughout. He

accordingly speaks of the nerve as supplying all the muscles of the face with motor influence, as well as the surface with sensibility. But it is clear that he regarded the action of mastication as the *special* object of its motor branches, since he speaks of the whole as the nerve of *sensation and mastication.* He was aware, too, that the supra-orbital branch does not possess motor powers, since he speaks of its having been divided, so as to cause loss of sensibility, without the movements of the eyebrow being affected. But *these*, as will presently appear, he regarded as respiratory or expressional in their character, and therefore in the province of the portio dura. That it was the only nerve of sensation belonging to the face, he adduced abundant proof. But in regard to its motor properties the experimental proof was incomplete. The only fact of this kind which he brought forward was the very one which induced him to uphold the opinion he had formed that the motor filaments are distributed to the whole face, being the experiment upon the infra-orbital branch in an ass, section of which *appeared* to deprive the animal of the power of moving his lip. The fallacy of this inference was subsequently exposed by Magendie and Mayo; and Sir C. Bell, as we shall see, has adopted the correction without due reference to the source from which it was supplied. We think there is good reason to believe, however, that, by the union of anatomical and experimental enquiry, Mr. J. Shaw would have speedily arrived at the truth by his own investigations. That the fifth pair had motor properties, however, was abundantly proved in Sir C. Bell's estimation by the origin of its smaller root from the *tractus motorius*, and by the persistence of some amount of motor power after complete paralysis of the seventh. So far as this relates to the masticator muscles, there can be no mistake; but we suspect that Sir C. Bell was at first confirmed in his idea of the *general motor* character of the fifth by the occurrence of cases in which the movements of respiration and expression are paralysed, while the voluntary power over the muscles remains. The latter we do not think he was *then* prepared to attribute to the seventh pair.

Everything in the original memoir leads us to the conclusion, that Sir C. Bell *then* regarded the seventh pair as solely concerned in the *respiratory* movements of the face. These, however, he made out to be a very comprehensive class, embracing not only the motions of expression, but many actions which are usually regarded as voluntary. It is quite certain that *no other* motor functions were there attributed to it; and there is an antagonism evidently alluded to (*Phil. Trans.*, p. 422) between the voluntary action of the fifth nerve, and the involuntary action of the seventh. Perhaps the strongest proof, however, that we are correctly stating Sir C. Bell's original opinion, is the alteration which all the passages have undergone in which the fifth pair is spoken of as the general nerve of voluntary motion, and the seventh, either directly or indirectly, characterized as simply a respiratory nerve. If he has not altered his opinions on the subject, why has he thought it necessary to alter so completely the expression of them? We are sorry to be obliged to add, that he has not marked these altered passages as he should have done; so that the reader of the last edition will not find, in the paper of 1821, Sir C. Bell's *original* opinions or experiments, but modifications of them rendered necessary by the subsequent progress of

his views. There could be no possible objection to his ingrafting these on his original memoir if he thought fit; but there should have been some intimation given of a change. We can very well imagine a student referring to this paper for the purpose of ascertaining the original opinions of Sir C. Bell, and being entirely deceived as to the degree of truth he had then attained on the points in dispute. The charge of unfair dealing has been raised against him by the editor of the Dates and Documents; and a considerable amount of evidence might be adduced in support of such a charge. We shall content ourselves with stating the fact, in the hope that Sir C. B. may see the propriety of not giving such a handle to his opponents, as is afforded by what certainly has the *appearance* of underhand dealing.

There is another curious point to which no one, we believe, has directed the attention of the public. In his original memoir (p. 411), Sir C. Bell speaks of the seventh nerve as distributed not only to the muscles, but *to the skin*, in conjunction with the minute vessels of the cheek. And in another passage (p. 415), he speaks of *an impression on the respiratory nerves*, in such a manner as to lead to the belief that he regarded the seventh pair as not only an efferent or motor nerve, but, so far as the function of respiration is concerned, an afferent or excitor channel. If he entertained this opinion, (which, however, we do not positively state,) though it receives strong confirmation from an expression of Mr. Shaw's, he soon abandoned it, for we find the anatomical assertion first qualified and then omitted, and the other expression modified. Whether or not this idea be correct, (and, like the other, it is borne out by the alterations which the author has made,) it derives support from the fact that the anatomical statement does not correspond with that of other authors, nor with that of Mr. Charles Bell in his Anatomy of the Human Body, (1802, vol. iii. p. 150-1,) where all that is said of its ultimate distribution refers to muscles only. A passage from the work just referred to may be thought a little curious by some of our readers. "As to the sympathies which physicians have thought fit to ascribe to the connexions of this with other nerves, as laughing, weeping, kissing, &c., they would be tedious to enumerate, and by no means instructive."

Mr. A. Shaw endeavours to show from his brother's papers, that, previously to Mr. Mayo's first publication, the *voluntary* character of the portio dura was understood by Sir C. Bell; but we do not think that he has succeded. He adduces its known concern in the actions of whistling, speaking, blowing, and inhaling, as proved by cases of paralysis; but all these actions belong to Sir C. Bell's class of *respiratory* movements, and are expressly alluded to by him as such; and in his eagerness to establish this entirely new system, he seems to have left out of view the share which volition has in producing them. The only action which could be otherwise represented is the closure of the eyelids, of which Mr. J. Shaw observed that a patient with paralysis of the seventh nerve was incapable. But even this Sir C. Bell tried to include in his class of respiratory movements; for in the first reprint of his Memoir, in 1824, he says, in relation to a case of this kind, "*Thus it appears* that the portio dura of the seventh nerve is the principal muscular nerve of the face; that it supplies the muscles of the cheek, the lips, the nostrils,

and the eyelids; *that is*, that it is the nerve which orders *all those actions which are in the remotest connexion with the act of respiration*. It is possible that those relations may not be apparent at first; but in the prosecution of the subject we shall discover the reasons of those links by which the respiratory organs are combined with the actions of the features."

We now come to the period when other physiologists present themselves as participators in the work of discovery. In August, 1822, M. Magendie published his first Memoir on the spinal nerves, in which he assigned distinct functions to the two roots. Now we have stated our full conviction that Sir C. Bell and Mr. J. Shaw had virtually done this in the year previously, and that it was so understood at the time. Moreover, it is quite clear that M. Magendie was well informed, through various sources, of what had been previously done in England. For details on this subject we must refer to Mr. A. Shaw's history; only stating that among these sources was a visit paid to M. Magendie by Mr. J. Shaw himself in the autumn of 1821; at which time the latter explained Sir C. Bell's views, performed in his presence experiments illustrating the functional difference of the fifth and seventh pairs of nerves, and presented him with a copy of his Manual of Anatomy, in which a sketch of these views is given. All this is acknowledged by M. Magendie. It would be impossible, then, to sustain the slightest claim to priority in discovery in his behalf, even if the doctrines he aimed at establishing were the same.

But they are not so. Even in his first Memoir (Aug. 1822), he hints at *a union of the two functions in each root*. His experiments, he states, "made it appear that the posterior roots were *more particularly* provided for sensation, while the anterior were *more particularly* provided for motion." In the next paper (Oct. 1823), he expresses this opinion decidedly, and he has continued to hold it to the present time. He can scarcely, then, be said to have made any advance upon Bell's original hypothesis; and it is preposterous to set up any claim to discovery, either of principle or of fact, in his behalf. Into the merits of his opinion we shall not enter at present. No one, we apprehend, will attach great weight to it; since his deficiency of almost every habit that characterizes the true philosopher renders him nearly incapable of correct reasoning on any complex subject. We again repeat that the true functions of the roots of the spinal nerves are learned, in by far the most unexceptionable manner, from experiments on the cranial; and these M. Magendie has almost entirely neglected.

On one point, however, he did make an advance towards the truth. It was he who first detected the imperfection of the experiments which led Sir C. Bell to attribute motor functions to the infra-orbital branch of the fifth pair; showing that section of it does not destroy the motor power of the lip. But, in a subsequent paper, he stated that the seventh nerve is possessed of sensibility; an error of great importance in relation to the principle of Bell that single roots give functions of a single class only; and this was exposed by Mr. J. Shaw, who showed that the supposed sensibility of this nerve is derived from filaments of the fifth pair. Moreover, he never intimated the opinion that the fifth pair has *any* motor power, nor did he pay the slightest attention to its double origin; but

he everywhere alludes to its sensory properties only; so that it may reasonably be questioned if he knew anything more respecting it. His strange idea regarding its participation in the functions of special sensation is well known, and valued as it deserves.

We now come to a more painful division of our history;—that which respects Mr. Mayo's claim. We cannot but deeply regret the position he has assumed in this question. He has not merely arrogated to himself, as we shall presently see, discoveries to which he has no right whatever, but he has supported his claim by unwarrantable means; and, influenced apparently by personal hostility to Sir C. Bell, he has attempted to deprive him in favour of Magendie of that to which he could not assert his own right. All this would be pitiful only in a man of no other pretensions to eminence; but it is something worse in one who has such well-earned and independent claims to notice.

Before proceeding to these particulars, we think it right to state generally (on the authority of Mr. A. Shaw) the relation which formerly subsisted between Sir C. Bell and Mr. Mayo. The latter was not merely an attendant at the Windmill-street School, where Sir C. Bell's peculiar opinions were made the subject of frequent prelection by himself and Mr. Shaw, but he was Sir C. Bell's house-pupil, and therefore enjoyed special opportunities of becoming acquainted with the progress of his opinions. Moreover, he had been employed by Sir C. Bell in making preparations illustrative of the difference between the roots of the cerebral nerves. Further, he had given other assistance to Sir C. Bell and Mr. Shaw, which was duly acknowledged at the time in three of the papers we have referred to. We quite agree with Mr. A. Shaw in attributing to these circumstances the degree of credence which his statements have received.

"It cannot be questioned that it has been owing to this gentleman's having been originally a pupil of Sir Charles Bell, and to its being generally known that he enjoyed, on that account, the best opportunities of knowing the true history of the discoveries, and likewise, I may add, to its being commonly felt that, unless he had been strongly assured of the correctness of what he delivered, he would have been unwilling to pass an unfavorable judgment concerning his teacher,—that his representations, however unsupported by proofs or arguments, have been readily accepted by the profession as infallible." (p. 24.)

The first part of Mr. Mayo's Anatomical and Physiological Commentaries was published in August, 1822. In *this* he took up a position decidedly hostile to Sir C. Bell, not on one or two points only, but as to *the whole system.* The title of the paper in question was "Experiments to determine the Influence of the Portio Dura of the Seventh, and of the Facial Branches of the Fifth Pair of Nerves." It might have been expected that he would have adopted the principles of his master; and, by additional experiments, have corrected or confirmed the functional details which had been imperfectly ascertained. On the contrary, he not only *neglects* but *opposes* the principle throughout. In giving a minute description of the anatomy of these two nerves, he omits all reference to the difference of their origins; but commences his account of their branches from the time of their emergence from the cranium. Moreover, he says that the "only unexceptionable evidence" respecting the functions of individual nerves is derived from the results of injuries

(accidentally or purposely inflicted) in their course. This is in evident opposition to Sir C. Bell's principle, which has been fully explained by Mr. J. Shaw in his paper on Partial Paralysis, which had been read to the Medico-Chirurgical Society five months before. But Mr. Mayo offers even more decided opposition to it than this. He quotes a passage from Sir C. Bell's memoir, in which the fifth pair is classed with the spinal nerves on account of its double origin, ganglion on one root, universal distribution,&c.—and the portio dura with the respiratory nerves, on account of their single ganglionless roots and peculiar distribution; and says of it "*As nothing is so prejudicial to the interests of science as the temporary adoption of an unsound theory*, I shall hazard a few remarks upon that of Mr. Bell."

It is evident, then, that Mr. Mayo cannot found on this paper the slightest claim to having assisted Sir C. Bell in the development of his *principle*. We shall now enquire what is the amount of its contribution to positive knowledge. In the first place, with the view of demolishing the "respiratory system," he performed experiments on the *par vagum*, for the purpose of demonstrating that it is a *common nerve* of motion and sensation. Some of these experiments were certainly important; but they by no means supported the inference erected upon them; nor have they, as is very well known, prevented the necessity for others of more varied and precise character. He then endeavours to prove that the portio dura is "*a common nerve of voluntary motion;*" if it be divided, the muscles which receive branches from it are "*completely paralyzed.*" Here he supplies a correction to Sir C. Bell, which was, we think, partly needed. The latter had, we feel no doubt, overlooked in the first instance the participation of the seventh pair in the *purely voluntary* motions of the face; and had endeavoured to show that all the movements it produces are connected remotely or directly with the act of respiration. But Mr. Mayo runs into the opposite extreme; for he asserts that this nerve is not merely the channel of *voluntary motion only*, which is certainly untrue, but that it supplies *the whole* of the voluntary power of the nerves to which it is distributed—a position still more decidedly false. This brings us to consider his statements regarding the *fifth pair*.

Neglecting, as we have stated, all consideration of the peculiar origin of this nerve, and, as it would seem, desiring nothing so much as to prove the fallacy of Sir C. Bell's statements, Mr. Mayo confined his experiments to the particular subject of Sir C. Bell's, namely, the infra-orbital branch: and succeeded in proving that muscular power had been erroneously attributed to it. But in this correction he had been anticipated, as we have seen, by Magendie, who had published the same correction ten months before. Mr. Mayo is represented by Mr. A. Shaw as leaving the question in this state, and as implying, if not asserting, that the fifth pair has no other function than sensibility. But this is not the case, as the following passage shows:

"I infer from the preceding experiments that in the ass the portio dura is a simple nerve of voluntary motion; and that the frontal, infra-orbitar, and inferior maxillary, are nerves of sensation only; and from the preceding anatomical details, that other branches of the third division of the fifth are voluntary nerves to the pteregoid, the temporal, and buccinator muscles." (*Commentaries*, p. 112.)

The concluding passage is a good specimen of the *animus* which characterizes the whole paper.

"It remains (says Mr. Mayo) for the reader to decide whether Mr. Bell's experiments are satisfactory, and bear out his inferences; whether the latter, coupled with my former observations on the five respiratory nerves of this author, leave his theory tenable; and, perhaps, finally to determine whether there exist in the whole of Mr. Bell's essay, after the deduction of his controvertible statements, *more than one correct inference*. I here allude to Mr. Bell's experimental confirmation of an opinion which, at the beginning of the eighteenth century, occurred to Dr. Blair, on his minute examination of the proboscis of an elephant, viz., that the infra-orbital nerves are nerves of touch."

Within two months after the publication of Mr. Mayo's first paper, Mr. J. Shaw obtained unequivocal proof, by experiment, that the *third branch* of the fifth pair has motor properties, and that it is the nerve which produces the masticator movements of the jaws. This he published in a paper entitled "Observations on M. Magendie's Experiments," in the London Medical and Physical Journal, for October, 1822. Thus, then, we may regard the *functional history* of this nerve as completed. We do not perceive, however, that any attempt was made by Sir C. Bell to obtain more accurate information regarding its origin; but if he had been aware of the description given of its roots by previous anatomists, he must at once have seen how perfectly conformable it was to the results obtained by experiment and observation. Throughout the paper we have just referred to, *Magendie's* experiments are the subject of criticism. The correctness of his statement respecting the absence of motor power in the infra-orbital branch is admitted; and the fallacy of the first inference explained. The paper concludes with a notice of "a late pamphlet," which Mr. Shaw adverts to with much unwillingness; "for, when engaged in such a delightful enquiry, it is most unpleasant to be involved in a dispute with one whose object seems rather to be an attempt to detract from the merit of his late master than a wish to promote the interests of science." It is perfectly evident, then, that Mr. Mayo has *no right whatever* to claim, as he has always done, the correction of Sir C. Bell's views regarding the fifth pair; and that no acknowledgment was due from Sir C. Bell *to him* in adopting the modified opinion; but the obligation to Magendie should have been more openly stated. We find it only in the *second* memoir; whilst the correction has been introduced into the reprints of the first paper, without any notice of the alteration.

We shall quote two other passages from this and the succeeding paper of Mr. Shaw, in confirmation of views we have previously expressed. The first will show *his* anxiety that Sir C. Bell's doctrine regarding the *spinal nerves* should be the first in order of publication: "The accumulation of facts will probably embolden Mr. Bell, in a short time, to give an account of his investigations into the anatomy of the spinal marrow, and the views he has deduced from them, which will lead to an entirely new doctrine of the anatomy of the brain and nerves. In justice to Mr. Bell, I should state that this has been a subject of constant contemplation with him for the last fifteen years. *He has thought it necessary to rouse attention to the subject* of the nerves, by first offering his papers on the respiratory nerves, and on those of the orbit and throat, before

that on the general system. It has been in vain that we have urged the danger of anticipation."*

In the first part of Mr. J. Shaw's General View of the Nervous System, in the same journal for December, 1822, we find the following passage, which confirms our opinion that the actions of the portio dura were still regarded as of a respiratory character *only:* "The observations which are already before the public, proving that the expression of the face, nay, of the whole body, has such an intimate connexion with the organs of respiration, afford sufficient explanation why the same nerve should combine the *motions of the eyelids and forehead* with the acts of respiration, and with the motions of expression performed " through the muscles of the nose and mouth."† And he speaks in the same paper of the portio dura as being a nerve "*as similar in function as it is in origin and form* to the branches of the *eighth pair*." This last sentence also tallies with the passage in Sir C. Bell's memoir formerly alluded to as indicating a belief that the portio dura is subservient to *impressions* connected with respiration, as well as to motor influence; since the par vagum has been known to have chiefly an afferent function from the time of Whytt. Neither Mr. J. Shaw nor Sir C. Bell have ever acknowledged any change in their views on this subject; but the question again returns—if their views have not been altered, why has the mode of expressing them been gradually changed? We do not think, however, that Mr. Mayo can claim much merit on this score; since the influence of the will may be distinguished from an instinctive impulse more by cases of paralysis in man, in which one function is lost without the other, than by any experiments that he had performed or could possibly devise.

Previously to the publication of the second part of Mr. Mayo's Researches, in July, 1823, two new memoirs had been presented by Sir C. Bell, to the Royal Society; one on the Nerves of the Orbit, and the other on the Motions of the Eye. These, however, do not contain any matter peculiarly relevant to the present enquiry, which refers only to the general functions of the nervous system, and the offices of the fifth and seventh nerves: the latter question being connected, as we have seen, most closely with the first. The chapter of Mr. Shaw's History devoted to the consideration of Mr. Mayo's second paper is thus headed: " Mr. Mayo's second memoir essentially distinct from his first, inasmuch as he abandoned his opposition to the principle of the discoveries, and adopted it for his own use, without acknowledgment." This appears fully justified by the contents of the paper.

We have seen that, in his first memoir, Mr. M. opposed the principle we have so often referred to as peculiar to Sir C. Bell, that the explanation of the functions of nerves is to be sought in their roots; and that, in his anatomical description, he omitted all mention of the origin of the fifth pair.

He *now* adverts expressly to its roots, and speaks of it as *well known* that the small root passes by the ganglion and enters the third division only of the fifth pair. He then informs us that he had discovered that this is distributed exclusively to the muscles of the jaws; and, reminding

* London Medical and Physical Journal, xlviii., p. 347.

† Op. cit., p. 463.

us that the remaining branches had been proved to be nerves of sensation, he continues, "*Thus it appears* that the fifth nerve consists of two portions: one of which has no ganglion, and is a nerve of voluntary motion, and probably of muscular sensation; and another, which passes through a ganglion, and furnishes branches which are exclusively nerves of the special senses." He then quotes Soemmering as having long ago pointed out the analogy between the fifth pair and the spinal nerves; and modestly tells us that "by this analogy he was led to conjecture the fact that the double roots of the spinal nerves have functions corresponding with those of the fifth," and that, "while he was engaged on experiments to ascertain the fact, M. Magendie's were published, which established the justice of the conjecture."

We scarcely know how we can decorously characterize the latter part of this claim. Suppose Mr. Mayo had been the discoverer of the accordance between the double function of the fifth pair and its double root, is it not perfectly clear that he had been led to this by the previous enquiries of Sir C. Bell, which had commenced with the spinal nerves, and afterwards been directed to the fifth as analogous to them, and as affording from its contrast with the seventh a more satisfactory object of comparative investigation? And is it not clear, also, that the functions of the roots of the spinal nerves had been correctly assigned two years before in Mr. Shaw's Manual, not to mention the assumption of them in Sir C. Bell's own memoir on the very ground specified by Mr. Mayo? It would have been *bad enough* for a bystander to have overlooked all this; but that Mr. Mayo, who had enjoyed such peculiar opportunities of knowing the real progress of Sir C. Bell's views, should have assumed that he was ignorant in June, 1822, of the true functions of the roots of the spinal nerves, we must say, is *too bad*. But this is not all.

Mr. Mayo claims the discovery of the anatomical connexion between the small root of the fifth pair and the muscles of the lower jaw; speaking, however, of the entrance of this root into the third branch exclusively as *well known*. Now we have before shown that the anatomists, who had stated the latter fact, which had been forgotten and neglected, also knew the former. How much credit, then, does Mr. Mayo deserve for this observation? We might have been disposed to award him a share, if he had not, in the same paragraph, passed by Sir C. Bell to give to Soemmering the analogy between the fifth pair and spinal nerves. But let it be clearly understood, that to Sir C. Bell is unequivocally due the first demonstration, that all the branches of the fifth are sensory, and that this is *the* nerve of sensation to the face; that after the correction of Magendie he had restricted its motor character to the third branch; and that Mr. J. Shaw had proved experimentally, which Mr. Mayo had not, that the third branch really does possess motor powers; and then let us ask, what share Mr. Mayo had in establishing *any one truth* regarding the functions of the fifth pair? Will it be credited that, in thus changing both his mode of enquiry and in adopting as the results of his own investigations the facts previously observed by Sir C. Bell and Mr. J. Shaw, he has not mentioned the name of either of these gentlemen from beginning to end of this paper, devoted as it is to enquiries connected with the roots of the cranial and spinal nerves?

We have yet to notice Mr. Mayo's Outlines of Physiology; but of

these we shall soon dispose. We have already noticed that statement which refers to M. Magendie's claim to the discovery of the true functions of the spinal nerves, as *entirely destitute of foundation.* We say *the same* as to Mr. Mayo's claim to having established the motor character of the ganglionless portion of the fifth, and the true function of the seventh. And we would say, if possible, still more, as to the claim which he here repeats of having *been led by these discoveries* to assign the functions of the roots of the spinal nerves at the time of the publication of Magendie's experiments. We have yet a graver charge against him. He quotes from the *first part* of the Anatomical and Physiological Commentaries, the experiments which led him to the inference that the infra-orbital branch of the fifth is sensory only, (in which, as we have seen, he had been anticipated by Magendie, yet he gives the French physiologist no credit for anything *he* could claim;) and he then continues his account of experiments and observations on the same subject, embracing those contained in his *second* Memoir, in such a manner as would lead any one unacquainted with the facts to the idea that he was all along recapitulating the statements in the *first*. The difference of a year is here very important; and in the only other reference he makes to his Commentaries, he speaks of them as published in 1822, without intimating that a second part appeared at any subsequent time. The readers of his Physiology, therefore, have no information of the existence of such a publication; and naturally attribute to the *first* part all that is quoted from it. We can speak from experience as to the effect of this and other misrepresentations of Mr. Mayo's, in giving rise to most erroneous impressions as to the share which Magendie and himself had in the late discoveries; and there is one most important *omission*, with noticing which we shall conclude our reference to him. He leaves so little for Sir C. Bell to claim, that one would have thought he would have been glad to assign him what he could. But no; the same *animus* prevails throughout; and, in professing to award to each their due, he passes by the fact that "to Sir C. Bell we are indebted for introducing the new mode of investigating the functions of the nerves, by examining the distinctions between their origins; and then indirectly claims that discovery for himself."

To sum up in few words the result of our historical enquiries, we may state that Sir C. Bell appears to us to have the exclusive merit of originating the mode of investigation which has led to the positive knowledge of the structural distinctness of the afferent and efferent nerves; that he was the first to give a correct explanation of the functions of the two roots of the spinal nerves; that he was the first to direct attention to the double function of the fifth nerve contrasted with the single function of the seventh, and to point out the connexion of the functions with the double and single roots of these nerves respectively; and that he was the first to assign clearly the whole function of the fifth pair. In thus speaking of Sir C. Bell, we include, of course, Mr. J. Shaw. We have no means of assigning to these gentlemen, individually, the particular share of merit which may be respectively due to them. Mr. J. Shaw always represented himself as following out Sir C. Bell's views; and Sir C. Bell has constantly taken opportunities of attributing

to his regretted friend and relative the highest merits which such an assistant could possess. We wish that this spirit had been more extended to others who were engaged in the investigation.

The only points on which we regard Sir C. Bell as having received assistance or correction in perfecting the system as we at present possess it, are the character of the infra-orbital branch of the fifth, as a purely sensory nerve; and that of the portio dura of the seventh as a nerve of voluntary power, independently of its connexion with the respiratory function. For the first he was indebted to Magendie; and the obligation has been expressed by him, although, as we think, insufficiently. For the second he may possibly be indebted, in some degree, to Mr. Mayo; but as that gentleman's view is as erroneous in its exclusiveness as Sir C. Bell's own, we are disposed to think that the modification of the opinions of the latter is the result of his own independent observations. We shall presently see that it has been very gradual. As to the anatomical information respecting the roots of the fifth, which Mr. Mayo claims to have supplied, we have reason to believe that he was anticipated, not only by Paletta but by Mr. J. Shaw. The decisive result of the experiment of the latter upon the third branch, and the correction supplied by Magendie regarding the second, would naturally lead him to examine the anatomy of their roots with more precision; and we find from a subsequent statement of Sir C. Bell that this was the case, and that it was then perceived that the motor root is only connected with the infra-orbital branch by cellular substance.

With regard to the respiratory or superadded system of nerves, no one has ever attempted to deprive Sir C. Bell of his claim of originality. But the doctrine has not been by any means generally received among physiologists. That the respiratory movements belong to a large class of actions excited through the afferent nerves, central organs, and efferent nerves, was pointed out by Whytt, and has been generally taught in this country, except by those who adopted the strange notion that they are voluntary. It was very properly urged, therefore, by Dr. Alison, in opposition to the " respiratory system," that these movements are in no respect different in character from those to which no such system has been assigned. We fully accord with that philosophical physiologist in this view; and on a former occasion we entered at length into the analogy of the respiratory movements with those of deglutition, defecation, &c. (See Vol. V. p. 511, &c.) The tendency of Dr. M. Hall's researches has been to harmonize the two opinions on a very remarkable manner. He agrees with Dr. Alison and others who have written on the same side, in placing the respiratory movements in the same footing with those which have been generally denominated *sympathetic*; and he includes Sir C. Bell's respiratory system of nerves in his own distinct spinal system, to which all these associated movements may be alike referred.

We have purposely omitted all reference to the researches of Bellingeri on the Fifth and Seventh Pairs of Nerves, because there is no proof that, although they were contemporary with Sir C. Bell's, and were published before them, they contributed anything to the advance of knowledge in this country. But our summary would not be complete without a notice of them; and we should have had a still better opinion

of Sir C. Bell's candour if he had mentioned them in his introductory history. These researches were published in an Inaugural Dissertation at Turin in 1818; but it does not seem that they received any attention in this country until 1834, when an abstract of them appeared in the Edinburgh Medical and Surgical Journal. The following statement we make on its authority: "The author sent a copy of the Dissertation to the Royal Society of London in 1819; and it was presented to that learned body on the 20th of January, 1820, as is proved by its being acknowledged at that date among the list of donations. There, however, it appears to have been buried in the inglorious oblivion of the multitude of works which find their way into that far-famed institution, or——but no, we cannot allow ourselves to give utterance to the alternative." Of that alternative we shall leave our readers to judge for themselves: we prefer not expressing an opinion of our own; but it is our duty to state facts.

Bellingeri first gives a minute anatomical description of the origin, course, and distribution of the fifth pair. He correctly describes its two roots, the formation of the ganglion upon the large one exclusively, and the entrance of the smaller one into the third branch alone. He states that few of the filaments of the first two branches are distributed to muscles, but that the third branch sends a larger proportion of filaments to muscles than either of the two others; and, from these muscles being those concerned in manducation, Bellingeri has given, with Paletta, the term nervus masticatorius to the smaller root. Amongst its branches he minutely describes the ramus buccinalis labialis, which Sir C. Bell took credit to himself in 1829* for having traced as the nerve governing those muscles of the lips which are concerned in mastication.

The portio dura of the seventh pair is described with equal minuteness. The chief peculiarity in the account of it is that, although the great majority are sent to muscles, some filaments are spoken of as proceeding *to the skin* of the cheek, lips, and neck. The correspondence of this with Sir C. Bell's early statement already noticed is somewhat curious.

We shall not enter into minute detail regarding his view of the functions of these two nerves, since a copious abstract of this part of his Dissertation is readily accessible elsewhere. The following is, we believe, a correct general description of them. Following the division of Bichat into the nervous system of animal life and that of organic life, he labours to show that the fifth pair belongs chiefly to the latter, controlling the growth and nutrition of parts, and being the medium of the *organic sympathies*, in which he includes not only what are now denominated such, but the sensible movements which take place without the control of the will, such as that of the iris, the internal auditory apparatus, &c. These he imagines not to be produced through the brain or spinal cord, but through the medium of nearer ganglia and plexuses. The motor powers of the third branch he shows to be connected with organic life, as ministering exclusively to the actions of nutrition. He attributes to the fifth pair the control over the secretions,

* Phil. Trans., Second Memoir on the Nerves of the Face.

both of the glands and membranes; and shows that it thus furnishes the *conditions necessary for the exercise* of the special sensory organs. In this he has anticipated very remarkably the correction which the subsequent dogmas of Magendie received.

Now we here find no trace of the function of sensibility attributed to the ganglionic division of the nerve by Sir C. Bell; and it cannot be denied that Bellingeri was so far carried away by his peculiar view of its character, as not to recognize this property. But it is equally clear that he has anticipated later discoveries in attributing to the fifth pair an influence on the functions of nutrition, secretion, &c., which it probably possesses in virtue of its ganglion. And the knowledge since attained of its character as an *excitor* nerve has enabled us to perceive that it does participate in many of the sympathetic movements he mentions, though not by directly acting on the muscles.

As to the portio dura, Bellingeri was nearer the truth. He distinctly states that it is the nerve of *voluntary motion* to the face, and also of the *animal sympathies.** He supports this statement upon experiments and pathological observations, which prove it as clearly as Sir C. Bell's. He was misled by the supposed distribution of the nerve to the skin, to consider it the nerve of sensation; notwithstanding that, in a case he details, sensation is spoken of as unimpaired when the paralysis of the muscles supplied by the seventh pair was complete, its trunk being compressed by purulent matter in the aqueduct of Fallopius. We have already adverted to a somewhat corresponding idea on the part of Sir C. Bell. The following is from Bellingeri's general summary: "We conclude, therefore, that the facial nerve *performs almost all the voluntary motions in the head,* presides over animal sensation in the face and neck, produces laughter, *expresses the emotions of the mind and the passions of the soul,* and is the agent of all the animal sympathies." His notion that it was the channel of sensation appeared to derive support from experiment. He found that when it was divided on both sides, the animal gave no signs of sensibility or pain when hairs were plucked out by the roots. If this be correct, we should explain it by the fact, that the animal was, by division of the nerve of expression and respiration, deprived of the power of making known its suffering; and Bellingeri's erroneous inference will thus appear to be precisely the converse of that equally erroneous one first drawn by Sir C. Bell in regard to the infra-orbitar branch of the fifth.

* Meaning those actions which are performed with the consciousness of the mind, and may be interrupted by a voluntary effort—the animal motions of Prochaska.

Art. VI.

1. *Des Etablissemens consacrés aux Aliénés en France, et des Moyens de les améliorer. Mémoire présenté au Ministre de l'Intérieur, en Septembre* 1818. Par E. Esquirol, &c. &c.—*Paris*, 1838.

Of Establishments for the Insane in France, and the Means of improving them: a Memoir presented to the Minister of the Interior in September 1818. By E. Esquirol, &c. &c.

2. *Maisons d'Aliénés.* (*Dictionnaire des Sciences Médicales.*) Par E. Esquirol, &c. &c.—*Paris*, 1838.

Lunatic Asylums. (*Dictionary of Medical Sciences.*) By E. Esquirol, &c.

3. *Essai sur les Distributions et le Mode d'Organisation, d'après un Système Physiologique, d'un Hôpital Aliénés pour quatre à cinq cent Malades; précédé de l'Exposé succinct de la Pratique Médicale des Aliénés de l'Hospice de l'Antiquaille de Lyon, depuis de* 1er *Janvier*, 1821, *jusqu'au* 1er *Janvier*, 1830. Par R. Pasquier, d.m. &c. &c. —*Lyon*, 1835. 8vo, pp. 52.

Essay on the Distribution and Organization of a Lunatic Hospital for four or five hundred Patients, according to a Physiological System; preceded by a succinct Account of the Treatment of the Insane in the Hospital of Antiquaille of Lyons, from January 1, 1821, *to January* 1, 1830. By R. Pasquier, m.d. &c.

4. *On the Constitution of Asylums, and the Mode of their Management.* By Sir W. C. Ellis, m.d. (*The Eighth Chapter of his Treatise on Insanity.*)—*London*, 1838.

5. *Total Abolition of Personal Restraint in the Treatment of the Insane. A Lecture on the Management of Lunatic Asylums, and the Treatment of the Insane, delivered at the Mechanics' Institution, Lincoln, on the* 21*st June*, 1838; *with Statistical Tables, illustrative of the complete Practicability of the System advocated in the Lecture.* By Robert Gardiner Hill, House-Surgeon of the Lincoln Lunatic Asylum.—*London*, 1839. 8vo, pp. 112.

6. *State of the Lincoln Lunatic Asylum.* (*Fifteenth Report.*)—*Lincoln*, 1839. 8vo, pp. 28.

7. *Sixth Annual Report of the Trustees of the State Lunatic Hospital Worcester* (*Massachusetts*). *December*, 1838.—*Boston*, 1839. 8vo, pp. 88.

8. *Nineteenth Annual Report of the Directors of the Dundee Royal Asylum for Lunatics.*—*Dundee*, 1839. 8vo. pp. 42.

9. *Medical Report to the Managers of the Lunatic Asylum of Aberdeen, for the year ending* 30*th April*, 1839.—*Aberdeen*, 1839. 8vo, pp. 16.

10. *Report of the Directors of the Montrose Lunatic Asylum, Infirmary, and Dispensary, for the year ending* 1*st June*, 1839. 8vo, pp. 22.

11. *State of an Institution near York, called the Retreat, for Persons afflicted with Disorders of the Mind.*—*York*, 1839. 8vo, pp. 19.

12. *Regulations of the Crichton Institution for Lunatics, Dumfries.*—*Dumfries*, 1839.

13. *Statistics of the Bloomingdale Asylum for the Insane.* By James Macdonald, m.d., Physician to the New York Hospital, late Resident Physician of the Asylum. (*New York Journal of Medicine and Surgery. October*, 1839.)

14. *Twenty-ninth Annual Report of the State of the General Lunatic Asylum, near Nottingham, delivered by the Vice-President and Visiting Governors, at their Anniversary Meeting, held in the Shire Hall, at Nottingham, on Tuesday, the 8th day of October*, 1839.

15. *Fiftieth and Fifty-first Report of the Visiting Justices of the County Lunatic Asylum at Hanwell, and Report of the Resident Physician, October* 31, 1839. 8vo, pp. 24-49.

In the construction and organization of a house for the habitation of lunatics, it would seem to be of the first importance to keep in mind that it is intended to be a place of security for human beings, brought by their lamentable malady to such a state that with the force and dexterity of adult age are conjoined the love of mischief and the helplessness of childhood. Humanity steps beyond the consideration of mere security, and adds much that is required for protection, for comfort, and even for enjoyment; and science, deriving salutary aid from these humane arrangements, adds others with the intention of securing relief and promoting recovery.

As every large establishment of such a kind must contain a number of patients whose incoherent or imbecile state, the result or sequence of the form in which the disease first manifested itself, has been found, by long and varied experience, to be little under the control of therapeutic measures; the well-doing of this considerable class of patients depends mainly on the regulation of all circumstances that have a general influence on the health of the body and mind. To breathe good air, to have good food in good quantity, to sleep comfortably, to be clean in person and decently dressed, to take exercise regularly, to be screened from vicious or exasperating excitements, and to be solicited by gentle and frequent efforts to self-control or to mental alacrity, are the points of most importance to such patients, and those to which their superintendent must systematically direct his attention; always remembering that this care is demanded for a class of persons who have the unhappiness to labour under the worst of human calamities. From these circumstances are to be deduced, without difficulty, all the principles that should be observed in the government of lunatic asylums; principles yet of recent acknowledgment, but of stability and truth; the blessed product of that enlightened and universal charity which, although it has not yet flourished equally in every age and clime, has its imperishable root in Christian institutions. The details flowing from these principles may vary infinitely, but the objects must ever be the same. By an observance of these principles a lunatic house may itself become what M. Esquirol has designated it—"an instrument of cure, and, in the hands of a skilful physician, the most powerful therapeutic agent against mental maladies."

But that it should be so implies well-devised arrangements, a careful classification, practical experience, various knowledge, humane dispositions, ample attendance, a superintendence ever watchful, and the

continual exercise of a philanthropy which nothing can weary, and a judgment which scarcely anything can disturb. In many of the private and public asylums of this and of other countries, the recommendations of the establishment necessarily fall below this standard; but the difficulty of managing lunatic patients in a private house, or whilst in the bosom of their families, and the want of a general familiarity with the phenomena and treatment of the different forms of insanity, often induce medical men, to whom such deficiencies are best known, to concur, with very little reflection, in sending patients to houses in which they cannot possibly enjoy the benefit of such various remedial means and such enlightened care as their peculiar affliction requires.

Scarcely credible does it now seem that, no longer back than twenty years ago, the condition of lunatic houses throughout France, and, with some exceptions, throughout Europe, should have been such as we find it described in M. Esquirol's historical review of those establishments. Prisons and dungeons were built and dug for them, and chains forged for them. To prevent their dying of sheer hunger was the sum of duty apparently thought due to them; and they were everywhere the victims of ignorance, of prejudice, and of terror. The patients (in France) were found by him covered with rags, lying upon straw, miserably fed, and grossly abused by ignorant and brutal keepers. Air to breathe and water to drink were almost equally excluded from them: they were chained like wild beasts, but in dens into which wild beasts would not have been put. In Germany, as described by Reil, by Joseph Frank, and by Max. Andrée, their condition was equally horrible. In Italy and Savoy the same enormities existed.

It is in vain that we would endeavour to represent even England as forming an exception; for, not to speak of the past mismanagement of Bethlem, the history of one of our provincial asylums, under the superintendence of eminent physicians, and the control of a committee of the first persons in one of the most considerable counties in England, was for the long period of thirty years the scene of every abuse that rapacity and inhumanity could crowd into a single institution. The mere recital at the present day would exceed belief. Suffice it to mention that, among the instances of mismanagement gradually brought to light, were the most aggravated neglect of all medical and moral treatment; every species of cruelty; much gross immorality; every practicable variety of shabby embezzlement and peculation; false reports, in which, especially, the *deaths* were concealed, even so many as one hundred at a time; the occasional disappearance of patients, supposed to have been murdered outright, and returned in the reports as dead, or removed, or cured; and, as a grand and appropriate finale, a very strong suspicion of the building itself being wilfully set on fire, in the hope of destroying some of the books or some of the patients. All these abuses were at length laid bare; but not until many honest enquirers had suffered every kind of insult and obloquy, and death had secured the principal offenders from human disapprobation: for, during the long series of years in which these enormities were practised and suspected, every abuse, every outrage, except the actual murders and burning, was defended by the officers of the institution, and screened from too prying eyes by the most dignified expressions of satisfaction from the committee, who were all the

time mystified by false accounts, kept in double sets for the purpose; and to whom, so heedless and confident were they, and to the nominal visiters, even some of the noisome *cells* forming a part of the establishment were actually *unknown*. Now and then, in this long period of blind confidence, a very delicate censure was extorted from them; but it should never be forgotten that for more than twenty years they obstinately and successfully resisted enquiry and reform; although during this time the deaths in the asylum were about 1 in 6.

The institution here alluded to was a kind of epitome of all the evils existing in all the lunatic asylums of the country; but a certain number of the enormities which made up the frightful mass at York were to be found, in various combinations, in almost every place throughout England in which lunatics were confined. M. Esquirol's Atlas of Plates will perpetuate the remembrance of a method of enchaining a poor officer of the navy in Bethlem, at a later period, and even eighteen years after Pinel had struck the chains from off the limbs of the lunatics in the Bicêtre. The poor man, when convalescent, is said to have threatened one of the physicians; and, as ordinary means of restraint were evaded by the patient, an ingenious apparatus of iron was brought from Newgate, and applied to him. This unhappy person is the subject of the last plate; and is represented sitting on a bed of wood and straw, his arms bound, his legs manacled, and a collar round his neck, by which he was fastened, by a chain ten inches long, to an upright iron pillar behind him. The weight of the apparatus was twenty-three pounds; and the patient (it ought rather to be said the prisoner) was unable to walk about or to stand upright, or even to lie down: and in this state he was permitted to remain nine years! Unless this mode of torture was customary at Bethlem, we should presume that this was the history of William Norris, laid before the Parliamentary Committee in 1815. Norris existed twelve years in a state very similar to this, at least; and was at length released by dying of consumption. During much of the time of his confinement he was well enough to read books of all kinds, and could talk quite coherently "on the events of the war." From the information laid before the same committee, we might take other horrible examples; but it would scarcely serve any useful purpose to do so. The only good effect of recalling any of these disgraceful transactions is to prevent too implicit a trust in institutions of the most inviting aspect, when jealously concealed from the public eye.

There is, in truth, scarcely an institution in the kingdom from which the shadow of former evils is altogether departed. We have learned to distrust the fairest outsides. Whole bookfuls of compliments to humanity signalized as unrivalled, and dexterity vaunted as matchless, do not move us one jot. All these things may coexist with great neglect, unseen in the show hours, but pervading every corner of the establishment, and saddening every patient's heart. Scanty diet; want of sufficient air and exercise; windows deeply encroached upon by iron panes, excluding both light and air, neglect of medical treatment in the early stage of the disorder; deficient aid in incidental sickness; early locking up in the evening for twenty-four hours of solitude, and miserable fancies, and burning thirst; Sunday neglect, that keepers and nurses may divert themselves abroad, their number being disproportionately

small to the number of patients, and their wages only attractive to the rejected of other occupations; coercion-chairs, in which poor irritable wretches are consigned to brutality of habits and stupidity of mind; frequent, and severe, and long-continued restraint, and not unfrequent blows: these things, thus collected, seem but horrible exaggerations; yet of each of these, singly taken, the lunatic asylum will furnish warm advocates, and few are the asylums in which some, nay, in which many of them, do not exist. Vainly do they boast of the neatness of their halls, and the silence and order of their wards, who have in some unfathomed corner of the house miserable creatures sitting strapped in chairs, with their restless hands in heating and constraining leathern muffs. More vainly still, if, as on too good grounds we fear to be the case, they have others in *chains*, and *kept in chains for years*. "They make a solitude, and call it peace," upon which we pray that the eye of authority may soon be directed. It is a refinement of the unprincipled to avail themselves of the benevolent feelings characterizing the time, for their own unworthy ends; and the spirit of gain, opposed to the Christian spirit by an everlasting incompatibility, may strive, we fear, in these times, to throw over these evils the fair cloak of employment, boasted of as profitable labour; under which cloak, however skilfully its folds are arranged, we shall find that, whilst many are subjected to fatigue, for which both their malady and their diet render them unfit, some are detained long after their convalescence is confirmed; detained because they are useful, and help to support an elaborate delusion. The spirit which can, in these days, prolong violent restraints, is not to be allayed, and therefore never to be trusted. They who have slept sweetly for many an untroubled year out of the hearing of the clanking iron, that all the time has been eating into the soul of some of their poor patients under the same roof, can never be expected to give a hearty concurrence to measures altogether different in their nature. We are too well aware of the impolicy of overstating a cause to peril a cause like that which we are advocating, by exaggerating the faults we denounce. But we have much reason for believing that lunatics are still kept enchained in certain asylums in England; and as to the minor evils, we have still stronger reasons for saying that we suspect these things to exist in many institutions in which, in the place of medical archives, the only carefully preserved books are certain manuscript compositions, inspired by hospitable luncheons and dinners, and of which the authors are enthusiastic but slenderly-informed ladies and gentlemen, who, accustomed to the more elegant condescensions of charity, are at a loss for terms in which to express their admiration of the little they have seen, and the much they have heard, as they hurried through the wards of the poor and the mad.

But so great progress has been made towards a humane and enlightened direction of lunatics, as to leave no doubt that all these lingering evils will be gradually swept away. Five and forty years ago, says Esquirol, lunatics were enchained throughout Europe. Eighty lunatics at the Bicêtre were unchained by Pinel in 1794, and the general treatment was henceforth improved; thongs and scourges were no longer delivered out to the keepers; and the result was, that many

lunatics, before deemed incurable, recovered, and that all the rest became quieter and more easily governed. France, he observes, with just pride, was the first nation to offer the spectacle of nearly three thousand lunatics kept in confinement (in and near Paris) without chains, without blows, without unkind treatment: and he quotes, with justifiable disgust, the cold-blooded opinions given by medical authorities in England at a later period, on these points of lunatic discipline. He alludes to numerous inventions, of more or less ingenuity, for confining the arms, legs, and heads of patients, on which we hope it is unnecessary to make any critical observations, the physician appearing likely in our day to supersede the carpenter and the blacksmith in this department of medicine.

A chapter in Pinel's admirable work is on the custom of flogging lunatics, as a means of cure: *Sur l'usage de frapper les Aliénés, comme un moyen de les guérir.* Every precept, before his time, was enforced by a rap on the knuckles, a blow on the head, or a lash on the back, given with a severe and virtuous gravity. There is astonishing convenience in this kind of despotism; and mankind always zealously cling to it. For all but its victims, its recommendations are overpowering. Eternal honour to Pinel! who, first of all in Europe, raised his voice against these atrocities, and pointed out the excitements they produced, and contrasted them with the calm that ensued on kind and compassionate treatment. Yet it moves our surprise that, after all his efforts, these evils should still have maintained their ground for so many years.

When M. Esquirol made his tour of inspection, lunatics were still commonly confined in hospitals for the reception of old and imbecile persons, with patients affected with the itch or with syphilis, with children, prostitutes, and criminals. This was the case in no fewer than thirty-three of the principal towns in France. In some other towns they were placed in the depôts of mendicity; and in others in the prisons, where, less favoured than the prisoners, they were not enabled to mitigate their condition by the trifling product of their labour, work being denied to them. Wherever placed, their sleeping-rooms, or rather their cells, were damp and unwholesome, and their food the refuse of the coarsest victuals given to criminals or paupers; whilst, in point of bed-comfort, the dogs of the establishment were better lodged than they. In almost every place of this kind the unfortunate beings were chained: it saved servants, it rendered better management superfluous—it even prevented the expense of strait-waistcoats. Sometimes a chain was attached, in large sleeping apartments, to every bed; and under their clanking the unhappy patients nightly sought repose. Against all the particulars of this shocking system, the medical men, much to their honour, made repeated protestations; but the authorities were always afraid of the insane, thought insanity incurable, and conceived that, if actual starvation was avoided, all was done which the miserable wretches required.

Whatever pleasure we should have in fancying such evils no longer to exist in any European country, we cannot forget that in Genoa, even at this time, about 250 patients are described by Dr. Lee as being confined to ill-ventilated wards, " which they are never allowed to leave until

they die or are dismissed the hospital." In the women's ward many are chained by the hands and feet; and the mortality of the institution is enormous.

The reform of such establishments has begun, and made considerable progress; but the great objects of philanthropy are seldom rapidly effected; the hostility of the unfeeling and of the prejudiced is never overcome by one assault; and unless the courage of the benevolent be accompanied by equal patience and vigilance, the work still remains long incomplete.

M. Esquirol has urged with great earnestness the institution of separate hospitals for the insane; a subject at present occupying the attention of the French government. In doing so, he has pointed out with much force the importance of having a few large establishments, instead of a small one for each department; among several reasons for which he represents the difficulty of finding a great number of physicians fitted by their acquirements and disposition to superintend them.

> "It requires," he says, "a mind of particular cast to cultivate this branch of the art of healing with advantage: the physician must have much time at his disposal, and make as it were an abnegation of self. It is not likely that a physician, already enjoying a great reputation and a large practice, will take charge of a small hospital, that will occupy all his time, expose him to dangers, and offer him but few chances of success. In fact, he who would be useful to lunatics should visit them several times in the day, and even during the night; he should not content himself with a visit made in the morning, as is the custom in common hospitals. But in a small hospital, what hopes of cure will sustain his courage? Charged with the care of an hospital containing from thirty to sixty lunatics, scarcely ten will offer any prospect of recovery; and of these ten, under favorable circumstances, he may cure five. And yet this small number of cures should not be considered discouraging, when it is remembered that in the departmental asylums the mass will consist of incurables, and the physician can only calculate on few favorable cases."

In this report, M. Esquirol gives a description of the plan for a lunatic asylum, which appeared to him, after ten years' consideration, the most convenient; and in conformity to which, we believe, several of these buildings have subsequently been erected. The general plan is that of a central building for the officers of the establishment, with wings for the male and female patients, so arranged as to admit of divisions and subdivisions of the patients; the separate parts forming quadrangles with a central square encircled by a gallery. The furious, the tranquil, the melancholic, the convalescent, are, by this plan, separated from all the other patients. This is very nearly the plan of the Bicêtre, and of the asylums of Rouen, Montpellier, le Mans, Nantes, and Marseilles.

Some arrangements, scarcely existing at the time, but which will now be acknowledged to be obviously necessary, are suggested in the report, such as that the compartments of the building destined for the furious should be of a more solid construction than the rest; that the floors of the rooms of patients who cannot be induced to cleanliness or decency in their habits should be of stone, and sloping towards or from the door; and that the convalescent department should be like a private house. In these and all other respects great diversity of plan obtains in the different English asylums. The strong doors are lined with iron in some, and in others merely consist of wood; whilst in some the doors

are only of ordinary strength. The convalescent department is seldom complete: it is indeed difficult to render it so, particularly in small establishments, where the number convalescent at any given time is very inconsiderable. Many very forcible reasons are given by M. Esquirol for placing all the patients on the ground-floor. The inconvenience and dangers of stairs, and the difficulty of having up-stairs rooms properly cleaned and attended to, are among the disadvantages of buildings of many stories; and the greater liberty of the patients, and greater facility of going out of doors when they please, or when free exercise in the open air is desirable for the alleviation of a paroxysm, as well as the readier attentions and inspection thus promoted, are some among the many advantages of the patients being thus placed. The fatal objection to this plan, implying so many separate buildings of one story, a kind of village in fact, is the great expense attending it. It is, however, as M. Esquirol well observes, deserving of consideration, whether economy is not best consulted by arrangements which most facilitate the cure of the inmates; a mode of reasoning too uncommon in relation to this subject.

The report concludes with a recommendation that to each asylum there should be an administrative council, consisting of the prefects of the departments associated in establishing it, of subscribers, the procureur-general, the principal gentry, &c.; the director and physician of the asylum having, as members of the council, only "a consultative voice." The director and the physician to be named by the minister, on the presentation of the council. A steward, a chaplain, an apothecary, and a superintendent to be appointed by the council, on the presentation of the committee of administration. This central committee to be formed by the minister of the interior; and with it the directors and physicians of all the asylums to correspond. The committee to make an annual general report.

The memoir entitled *Maisons d'Aliénés* is an enlarged and amended edition of the article bearing the same title in the *Dictionnaire des Sciences Médicales*. Of this memoir the introductory portion is entirely historical, and we shall pass it over, curious as it is, without comment. We are glad, however, to quote an opinion of much interest to superintendents of lunatics, coming from so great an authority as M. Esquirol, and which occurs incidentally when he is reciting the instructions given by Colombier for the reform of the Hôtel-Dieu, under Louis XVI., in 1785. It is, we think, a commonly received notion in England, and probably in other countries, that those who have the charge of the insane are prone, in consequence of that association, to become maniacal or imbecile. This opinion is expressed in Colombier's instructions, and the Bicètre and the Salpêtrière are referred to for examples. On the subject of this alarming belief M. Esquirol says, that in the course of forty years' experience, he has seen nothing which confirms it. We presume the truth to be, that the constant superintendence of lunatics is an occupation somewhat harassing and exciting; and that if the asylum is large, and its duties are conscientiously performed, and the superintendents are excitable, the nervous system may be somewhat severely impressed by continual anxiety, intermingled with the frequent and unavoidable agitations of a house in which so many persons are, by

the nature of their malady, exposed to so many accidents. Nearly all the superintendents and attendants of whom we have made enquiries relative to this point describe themselves as having been at first overwhelmed by the duties of their office; and, even after long habituation to them, as being now and then so far disturbed or subdued as to render a short absence, although only the absence of one day, indispensable to the recovery of their composure, or of their cheerfulness. There may be reason to fear that in very impressible constitutions, such effects, by frequent repetition, would become more serious. Much, however, depends upon the temperament; and those who undertake to cure unsound minds should possess some skill in managing their own.

In the brief, but distinct and interesting, notices given of the state and history of the principal lunatic asylums of France, we read almost a uniform record of past neglect and recent and happy reforms. On the progress of the latter M. Esquirol is entitled to look back with some self-complacency; his writings, his personal visits, his reports, his plans, his suggestions to the government, to committees, to architects, and to physicians, having materially contributed to many desirable ameliorations. The fuller details, the results of all his observations during his long and useful career, are promised to the public in a separate work, which he is about to publish, on lunatic establishments. It is evident that the uniformity of the amendments in the French institutions has been facilitated by the circumstance, that very few lunatics in France are confined in private asylums. For instance, in the department of the Seine, although there are twenty private asylums, the whole number of patients distributed among them does not exceed 400; whilst the three public asylums of Paris contain nearly 3000. In England and Wales, M. Esquirol was informed by Sir Andrew Halliday, in 1832, that the number of patients in the public asylums was 4077, and that there were not fewer than 2453 in private establishments. M. Esquirol laments that there are yet in France so few institutions entirely devoted to lunatics; and that there is nowhere any special and clinical hospital for the instruction of young physicians in their treatment. "Will our country," he says, "set the example?" We may echo his question in this country; where, as we have already had occasion to remark, (Number XIII. p. 34,) no direction of any public medical school has yet been found enlightened or courageous enough to add the opportunity for the clinical study of mental disorders to the other advantages of medical education. There are, we are well aware, some difficulties in the way of such arrangements; but we cannot think them insuperable; and the good to be obtained is very great, and very important to the public.

The want of buildings so separately constructed and arranged as to admit of a proper division of lunatics, according to the character and stage of their malady, is observed by M. Esquirol to be a very general defect, even of institutions entirely devoted to the insane. This fault is, he says, very remarkable in the most boasted establishments of England, and in many parts of Germany and of America. It doubtless arises from a too eager economy. There are very few houses in which the furious are rigorously isolated from the tranquil; the epileptic patients and those affected with accidental disease are not always separated from the rest; nor are the convalescent removed from those who are yet

under treatment for acute forms of mental disorder. In many houses, particularly in England, he remarks, the rate of payment nominally determines the patient's accommodations; but it can only really do so for those who are cleanly and quiet: and he does not approve of the buildings for the poorer patients being placed so as to show them the superior comforts of the richer. He thinks that the only basis of the classification should be the character and stage of the malady; but it surprises us that he should not take into consideration the effect produced on the minds of the patients by the presence or absence of comforts to which they have been accustomed. To circumstances of this kind few lunatics are quite insensible; and, after recovery, their reflections may be materially affected by them.

M. Esquirol disapproves of lunatic asylums being placed in towns, for obvious reasons; and he lays great stress on the necessity of having a sufficient number of courts, and of sufficient size, and of a cheerful aspect, for the exercise of the different classes of patients. He reprehends the practice of arranging the patients in lofty buildings, some in apartments half under ground, and some in rooms higher up, from which they cannot freely and readily descend. He prefers that the apartments should be on a level with the ground, not only for reasons which have already been mentioned, but because this plan gives them so much more freedom, without the annoying accompaniment of unlocking and unbolting doors whenever the patient is to walk out. His observations on the too common fault of constructing cells solely with a view to furious lunatics, who scarcely constitute ten out of a hundred cases, are such as are not always remembered, we think, at present; and he points out the propriety of noiseless locks, flat bolts, and windows which admit air and the cheerful light of day. It would be most ungrateful to forget the philanthropic and enlarged principles promulgated by Pinel at the close of the last century, or the sensible and humane observations on this matter, published by Mr. Samuel Tuke, in his Description of the Retreat near York, five-and-twenty years ago; works in which many of the most important principles of the treatment of lunatics are enforced in a manner so eloquent and so unpretending as to be worthy of all imitation. "Many errors," observes Mr. Tuke, "have arisen both in the construction and management of asylums, from an excessive attention to safety; and it has been made an excuse for much improper treatment, and for much vicious neglect on the part of the attendants." He mentions a visit he made to a house for insane persons, in which security was made a *primary* object; and where he found three of the keepers, in the middle of the day, earnestly employed in playing cards. We think, indeed, that a pretty correct estimate of the general management of a lunatic institution may be made by observing the number actually under corporeal restraint. It is here as in a regiment; where severe punishments are frequent, the commanding officer will generally be found inefficient.

In this particular there is apparently no asylum in England which presents so remarkable a model as that of Lincoln. Of all the works that have appeared on the subject of lunatic houses since the publication of Mr. Tuke's account of the Retreat, there is none which contains matter more deserving of attention than that recently published by Mr. Hill. His lecture is little more than a simple commentary on the

resolutions of the board of the management of the Lincoln asylum for twenty years past, during which period, under the superintendence of Dr. Charlesworth, and, latterly, with the vigilant co-operation of Mr. Hill himself, as house-surgeon, almost every kind of bodily restraint is stated to have gradually fallen into disuse as superfluous, or worse than superfluous, a mere substitute for want of watchful care. This great alteration of treatment, opposed to long practice and strong prejudice, and even to opinions yet very generally entertained, has only been effected, it is said, by the most careful adoption of all the parts of a system of constant superintendence, of well-preserved classification, and of humane and effective practical management. Very happily for the inmates of the Lincoln asylum, the managing board seems ever to have acted cordially with the medical officers; regulating even the building and arrangement with a view to curative effects rather than to mere short-sighted economy. Thus, besides other advantages, a classification has become practicable which answers the purposes of comfort, encouragement, safety, and cure. A well devised night-watch suffices to prevent the occurrence of suicides and of various irregularities, without the necessity of irksome and irritating night-restraints. The attendants are numerous, selected for good temper, and of sufficient bodily power; so that every patient is watched, and knows that he can be controlled; and yet every patient enjoys a great degree of liberty, and none are tortured by continual coercion. By means of sash-doors, the attendants themselves are liable to continual inspection, and little is left to their undisciplined temper or caprice. No corporeal restraint is inflicted without an order from the medical attendant; ill-usage of a patient is immediately punished; and a room is kept in which all the instruments of restraint are open to examination, so as at once to show their nature and the number in actual use at any time; whilst each application of them is regularly entered and reported. We know no more interesting document than the following, which is a summary of the tables appended to Mr. Hill's Lectures, showing the several instances of restraint, coercion, and seclusion, used in the Lincoln Lunatic Asylum, from March 16th, 1829, to March 22d, 1837, "*when the practice was wholly discontinued.*"

"SUMMARY OF THE PRECEDING TABLES.

Year.	Total Number of Patients in the House.	Total Number of Patients restrained.	Total Number of Instances of Restraint.	Total Number of Hours passed under Restraint.
1829*	72	39	1,727	20,424
1830	92	54	2,364	27,113¾
1831	70	40	1,004	10,830
1832	81	55	1,401	15,671½
1833	87	44	1,109	12,003½
1834	109	45	647	6,597
1835	108	28	323	2,874
1836	115	12	39	334
1837	130	2	3	28

"After deducting the number of patients introduced in the above Table more than once in the years 1829-30-31-32-33-34-35, and also the readmitted cases

* From March 16th.

within the same period, the actual number of patients restrained in the course of such seven years was 169.

"Of these 169 there remained in the house, at the end of such seven years, 43.	
Of these remaining 43, there were discharged from the books, during the years 1836-7, *not having been restrained at all* during any part of such two years	11
"..................... having been restrained only for about *seven hours* during any part of such two years	2
"..................... remained in the house December 31, 1837, *not having been restrained at all* during any part of such two years .	29
"..................... having been restrained *once only* (for about nine hours) during any part of such two years	1
	43"

We are perfectly aware that Mr. Hill's statements will be received in many lunatic asylums with surprise, and even with incredulity. The proportion of sane keepers to the patients is so very small in most of those institutions, and so much habitual assistance is calculated upon from imperfectly recovered minds, that the effects to be expected from a better system of watchfulness has scarcely yet suggested itself to the imaginations of the directors and superintendents. Perhaps, indeed, it is difficult for any one not familiar with the working of such a watchful system to believe it sufficient in *every* case, without the old physical restraints. If the Lincoln asylum can present a model of this kind, which all may visit and examine, the services of Dr. Charlesworth to the cause of humanity and in behalf of the insane, already considerable, will only be second to that of him who first released them from their chains. We know that many superintendents, not open in any degree to the charge of inhumanity, are of opinion that the occasional infliction of corporeal restraint, for a very short period (for a few hours for instance), is followed by such salutary effects as entirely to take away from it any objection on the ground of hurtfulness. Thus applied, they say, it abbreviates the paroxysms of many mischievous patients, and becomes a powerful persuasive to self-control. They suspect, also, that if such means are wholly laid aside, the keepers, however vigilantly watched, will take even more objectionable methods of subduing the patients, and produce a deceptive appearance of tranquillity by means of fear, excluding the patients not only from all violent manifestations of their malady, but almost from all manifestation of sensibility and of mind. The tranquillity of the Lincoln asylum is certainly one of its most striking peculiarities. There is scarcely a noisy patient to be found in it. The refractory ward is scarcely refractory; and the patients move about less, and talk less to visitors, than in any other asylum which is known to us. If patients, therefore, are as freely and indiscriminately admitted into that institution as into others, and if they are not *awed* into their submissive and almost torpid aspect, the effect of mere watching, and of the consciousness of being watched, is powerful indeed. The experiment of abolishing all restraint is too important a one to be rejected or received without free question and enquiry; and this appears to be encouraged, and even courted, in the Lincoln asylum. At all events, the evils of excessive restraint are always found to be numerous and shocking, and the object

of every physician ought to be to abolish it to the utmost extent consistent with the security and the amendment of the patients.

The fifty-first report of the County of Middlesex Lunatic Asylum, at Hanwell, shows that the managers of that large institution, containing 800 patients, have been the first to follow the example set by Lincoln. A table is appended to the report, containing a list of those in restraint each day for the four months of July, August, September, and October. The greatest number at one time, eighteen, was in July; it is seen to have been gradually reduced, until, in the middle of August, we find none in restraint. A few exceptions occur later in the list; but not one after the 20th of September. This has only been effected by making a sufficient increase of the number of attendants to ensure a very vigilant superintendence of the patients; and it is thought the general good effects of the change are already visible in the asylum.

In too many cases, we suspect, the mode of proceeding with lunatics in the early period of their affliction is the cause and not the consequence of the most alarming or the most offensive characters of the malady, which characters become exaggerated in its more advanced stages. Fits of violence are met by violent restraint, and an aggravation of the violence follows. Close bodily restraint induces uncleanliness; and the helpless creatures, disabled from any effort to maintain decent habits, are still more closely restricted, on account of the unhappy effects which the first restriction occasioned. Not a day passes, where this system prevails, in which some passionate keeper or some impatient nurse does not strap up some unhappy patient closely, and hastily, and fiercely; and the period of duration of such restraint, unless carefully and daily reported, (and where is this done?) is very uncertain, extending often to twenty-four hours, and galling the poor victim day and night.

Well worthy would it be of the managing committee of some great asylum to institute a normal school for keepers and nurses, where they might be rigidly accustomed to rely on the restraining power to be exercised by the proper application of a reasonable mind over minds of every shade of infirmity and disorder. Full, careful, patient, systematic experiment has not yet authorized any one, by its failure, to pronounce moral restraint insufficient for the control of any lunatic asylum in the world. Until such experiment is made and long persevered in, we fear that occasional bodily restraint must be resorted to. At the same time, we believe such restraint, if ever salutary and remedial, when most deliberately imposed, for the shortest period, and removed with the most considerate manifestation of a wish to give relief, which still we strongly doubt, will always be found to be inefficient and hurtful, in proportion to its long continuance; and especially hurtful if habitually resorted to for punishment rather than for protection. Of its real benefit at any time, for however short a period, and in any circumstances whatever, we are extremely sceptical. We say its benefit, not its convenience, which is often mistaken for benefit. Mr. Hill goes further: but those who object too hastily to his opinions should consider the qualifications with which he accompanies them: "*that in a properly constructed building, with a sufficient number of suitable attendants*, restraint is never necessary, never justifiable, and always injurious, in all cases of lunacy whatever." (p. 21.) If any view of restraint could palliate its disadvantages,

it would be that in which it is set in the Dundee Report; as a measure to which there is no alternative but terrifying and paralyzing the mind. But we do not believe that such is the alternative; and although the Dundee Report will be and is appealed to by all the admirers of the old system, from the strait waistcoat of the present day to the iron collar of Bethlem, and the chains of the Bicêtre, we are glad to find that there is actually no restraint at all imposed on the inmates of the Dundee asylum, "with the exception of instances of very violent excitement;" which instances, we learn from the same report, are as many as four or five, on an average, in 150 patients, a number which seems small, but is relatively considerable. If four or five require restraint at Dundee out of 150 patients, there should be always from twenty-five to thirty in restraint at Hanwell, which, in the present circumstances of that institution, would be looked upon as an enormous and even disgraceful number. Of the 800 patients there, the most bigoted admirer of sleeves and leg-locks would not be able to find a decent excuse for restraining more than three or four at any one time; and the actual fact is, that in that large institution, since the middle of August, there have never been two patients in restraint at any one time; and that out of 470 women there, not one has been in close restraint during the whole of that period. Nor do we learn that any unusual outrages or serious accidents have been the result of this emancipation. Some bedding, we understand, has been destroyed; some windows have been broken; a blow has now and then been given; and an attempt at escape has occasionally been made; but so far from these irregularities being on the increase, they are thought to be gradually diminishing, with the mortifying and tormenting restraints which it is more than half suspected was the cause of them. It is even thought that a blander character and manners begin to pervade the wards. Time will show how true this is, or how delusive; and those who incur the risk of making this great experiment will not, we venture to say, shrink from abiding the result.

Without endeavouring to proceed to the extent of the abolition of corporeal restraints, the character for humanity obtained in lunatic asylums appears to us to be little better than a kind of quackery. The quiet patients are exhibited in triumph; sagacious head-shakings are made over those sitting in the chairs of restraint, like so many beasts in an unclean stable; and the worst patients are not always shown. Besides which, it is the custom in a few asylums, a custom gravely sanctioned by the late Sir William Ellis, who was, we believe, the originator of it, to confine every unfortunate epileptic patient in bed at night, by a strap round one wrist, fastened to the side of the bed, to prevent the patient turning on his face in a fit. Thus ten patients, for the most part of a peaceable and inoffensive character, out of every hundred, are confined by straps for twelve hours out of twenty-four every day of their lives. We believe this practice to have been founded on a very limited acquaintance with the phenomena of epilepsy. But if we were to admit the fact of the turning of epileptics in a fit,—which we are far from admitting,—we should doubt the propriety of the general restraint of epileptics based upon it. "The severity of an asylum," well observes Dr. Charlesworth, "does not, as is supposed, consist in the outrage, blows, and active ill usage occasionally brought to light, and

which may be prevented by a superintendent having any claim to humanity or attention, and who has eyes to see bruises and a voice to ask their origin. Its torturing effect lies in the aching of limbs forcibly detained in one position, especially during the night, forbidden the ease and alleviation of change, with confined irritability for which nature has opened the vent of free motion, monotony, the feeling of oppression, surrounding miasma, contempt, and neglect:—all much more keenly felt than occasional violence, and sometimes prompting fatal acts of revenge or despondency." (*Lincoln Report*, p. 5.)

It is a most serious addition of the occasional misery of an epileptic, never to have a night of ease: for either the patient cannot turn comfortably about when confined by the strap, or the strap is useless. If the patient is liable to fall out of bed, the strap is objectionable, as the whole weight would be dragging at one arm. Lowering the beds of epileptics, as at Bethlem, would be better.

Those who speak in terms of eulogy of the moral advantage of even temporary restraints cannot divest themselves, one would think, of the apprehension that restraint once resorted to, and found convenient, will be not temporary, but continued for a hurtful period of time. To walk through a ward in which there is one noisy patient, and to order the patient to be instantly put in restraint, may pass for excellent discipline in the eyes of a hasty, frightened visitor; but if the visitor remained to see the order put in force, what would he behold? First, an ineffectual attempt of one or two keepers to effect the restraint; then a greater power brought to bear on the victim; and a scene of struggling, striking, kicking, biting, spitting, swearing, and screaming, which frights the whole ward from its propriety. Let him go again in an hour, and he will find the patient still noisy, shouting, and cursing all the powers that rule the asylum. In three or four hours it is still the same. At length, perhaps, the patient becomes silent. Happy it will be if even then restraint is removed and food is taken to him, or water to allay his thirst. But suppose no restraint is put on at all? Let the noisy patient's attention be diverted by being taken into the airing court; or, at the worst, let him be shut up in his room, the windows well secured, and the bedding removed. In that case we venture to say that in a much shorter time tranquillity will be restored. There will have been no struggle; and the punishment will leave no rankling sense of mortification; and the other patients will not be excited. Still, even in this case, the seclusion should not be prolonged. Many times, however, without any seclusion at all, a sensible keeper may so manage a refractory patient, that if the visitor were to return to the ward in a quarter of an hour, he would find the man whom he would have consigned to restraint quite tranquil, and civil, and cheerful.

In the acute stage of mania, one patient will doubtless require all the attention of one keeper, and perhaps of two, and for several days,or weeks, if the patient is not shut up. But in a well-regulated asylum this occasional attendance on troublesome patients should be a part of each keeper's duty, and taken by each keeper in turn for one or two hours. The attendants, if it is pretended that the patients are to be *cured*, should be sufficient to spare a keeper for an hour or two from any ward for such occasional duty. Then it will be seen that the acute

stage (no other means of control being omitted) is not of such long continuance; and that the patient's temper is unspoiled. The patient will gradually subside into tranquillity, and retain a grateful sense of what has been done; every trifling incident in their management being commonly remembered by them. At the same time, the other patients will not have been familiarized with the wretched spectacle of a poor creature dancing about in a strait waistcoat, like an intoxicated mummy; nor will their ears have been assailed with shrieks and curses from the maniac's cell. They will even, to some extent, appreciate the kindness and patience shown to the new comer; for they are keen observers, and have not forgotten their own sufferings.

In almost every asylum there are patients who will destroy their clothes, and some who cannot, it is represented, be persuaded to wear them. In the latter case we are glad to see M. Esquirol speak in disapprobation of forcibly confining the patient. One of the most unsatisfactory customs in some lunatic establishments is to exhibit these unfortunate creatures strapped down in chairs, and to comment, in moving terms, on their violence. The remedy, M. Esquirol says, is as bad as the evil. A dress of strong materials, with a strap round the waist, the dress being entirely fastened behind by a small lock (as done at Hanwell—see *Report*) can scarcely be removed by the patient. Boots and shoes may be easily fastened on by a similar contrivance, and in other cases it would be better to let them for a time be without their clothes, secluded of course from the gaze of visitors, than to add to their irritation, when possibly the wearing of clothes is a source of torment which they can only manifest by tearing them off. Chaining a poor frightened lunatic to his bed, also, when he perhaps thinks the bed full of snakes and scorpions, is ill-judged and cruel. If he persists in sleeping on the floor, having such notions, it is better to let him do so, and after a time he will be reconciled to his bed. It will doubtless be found that there are patients so ingeniously destructive as, with the help of hands and teeth, to tear up every article of clothing that can be devised. The absolute prevention of this can only be effected by putting the patient on straw, with body, and arms, and legs strictly and painfully confined. But are a few blankets, or a gown and cap or two, to be deemed of more value than the immunity of a poor creature from torture, of which the effect is only to subdue, and not to cure?

Of all the sights that can afflict a humane spectator in a lunatic asylum, none is so wretched as that of the patients who are confined in the wooden boxes, or coercion-chairs (a combination of a close-stool and the ancient watch-box), where they sit from morning to night—nay, we fear, from month to month. So revolting is this custom, that it is difficult to persuade one's self it can ever be indispensable. The experience of the Lincoln Asylum, and, more recently, that of Hanwell, with its 800 pauper lunatics, encourages a hope that there are few cases, or none, in which the uncleanly may not be brought to decent habits, and the epileptic guarded, and the furious controlled, by better methods. But to effect these ends, which no superintendent should rest satisfied until he has tried to effect, the keepers must be patient, and very watchful, and sufficiently numerous. We sincerely trust it may be found practicable to abolish from all asylums an invention which we are loath to believe a needful accessory. It is no part of our wish to reflect

in such a manner on any parts of the practice in asylums as to give offence to those whose days are devoted to the severe duties of such places; but it behoves them well to consider what might be done by a more liberal system, as regarded the keepers under them, and to appeal to the governing bodies for assistance towards improvements, which every man of humanity must wish for. There is no passage in the appendix to Mr. Hill's Lecture which we have read with more pleasure than the following: "Ordered, That the chairs used formerly for the double purpose of night-chairs and restraint (long fallen into disuse) be worked up." This working-up, and the formal destruction of iron hobbles and handcuffs, and even of strait-waistcoats, which we notice from time to time in the minutes, constitute triumphs in which we earnestly hope it may be found possible for all superintendents to share.

One of the items of recent expense, and which became a subject of comment and censure in a large asylum in the neighbourhood of London, was, "For six deal restraint chairs, £30." The asylum previously possessed thirty-five. At the present moment there is not one in use in the whole of the institution. Two hundred pounds worth of restraint chairs is thus thrown away. But the poor creatures who sate all day in those disgusting chairs may, it is said, be seen jumping about the wards like liberated children; not yet sure, when approached, that a blow is not coming, and yet shrinking with such expectation, and deprecating the expected cruelty in simple, but in touching words; but gradually acquiring confidence, and regaining the almost lost traces of humanity.

They who endeavour to abolish restraints must be warned that they will find much opposition on the part of keepers and nurses; that formidable bills for panes of glass will for a time be displayed before them; and blankets and counterpanes will be daily torn to shreds and scattered in the air. Periods of more difficulty and of temporary alarm and distrust must be expected, in which the superintendent will dread, most of all, the being driven back to use the old restraints; until experience and patience have devised and strengthened better means of controlling violence and preventing irregularity. To ensure the successful abolition of a system, the cruelty of which is, without all doubt or question, condemnatory of it, there must be substituted, for physical restraint, mental control, vigilant watching, care to repress violence, to allay irritation, and to heal quarrels. The attendants must be well versed in the readiest mode of overcoming violent patients; not struggling singly with them, a practice always threatening injury to at least one of the parties, but putting an end to opposition by combination; dexterously depriving arms and legs of inconvenient freedom of action; carefully removing missiles that may be rendered available to the patient's fury; and promptly and calmly bringing every fertility of device to meet every variety of difficulty. If patients are violent, they may be secluded for a short time; if they break windows, the shutters may be secured; if they tear bed-clothes, the bed-clothes may be covered with or sewed up in strong untearable materials; if they pull off or destroy their dress, strong dresses may be given, which will tire out the tearing propensity, secured round the waist with a belt of leather, having a small lock; if they strike, their hands may be enveloped by soft boxing gloves. Their

superfluous activity must be exhausted by exercise out of doors. In short, anything should be tried rather than confinement of limbs, the sure parent of uncleanliness never to be cured, and ferocity never to be tamed. And still, for years, the system may not work completely; for many of the subjects of it will be those who have been already vexed with every kind of restraint; have danced for days in strait-waistcoats; have been strapped and howling in bed for weeks together; or have sate in coercion chairs for months. No system can restore some of these victims. They must live out their lives in misery, whereof no small part has been of man's creation.

Much trouble must be taken, some sacrifices must be made, great anxiety must be endured, before the end can be achieved. Among other surrenders to be made by vanity, the glory of silent wards must be given up; for patients, who are happy, are not uniformly quiet. To fill a basement story with the inconvenient patients, to turn out the noisy into dismal courts, and to show an empty gallery; these are not parts of the system now advocated. Good conduct being uniformly enforced, the patients must wear the air of freedom as well as possess it. They will not sit wholly passive and silent, as so many beings with whom the world has no further concern; but will approach cheerfully or with vivacity, and talk to the visitor with confidence. The spruceness of the showhouse may be wanting; but the proportion of patients progressively advancing to health of mind, will be greater than it was under the ancient repressions.

As respects the actual attainment of these results, we cannot but think that the circumstances stated in the Hanwell Report afford a reasonable prospect of it. The progress already made is described in a few paragraphs, which, proceeding, after evident deliberation, from an asylum containing eight hundred pauper lunatics, almost wholly of the poorer classes, have certainly some claim to attention.

"The article of treatment in which the resident physician has thought it expedient to depart the most widely from the previous practice of the asylum has been that which relates to the personal *coercion* or forcible restraint of the refractory patients. Without any intention of derogating from the high character acquired by the asylum, it appeared to him that the advantage resulting from the degree of restraint permitted and customary in it, at the period of his appointment, was in no respect proportionable to the frequency of its application, that the objections to the restraint actually employed were very serious, and that it was in fact creative of many of the outrages and disorders, to repress which its application was commonly deemed indispensable, and, consequently, directly opposed to the chief design of all treatment, the cure of the disease. The example of the Lincoln asylum, in which no patient has been put in restraint for nearly three years, came also powerfully in aid of attempt to govern the asylum at Hanwell by mental restraint rather than by physical.

"Such an attempt could not be extended to all cases without some immediate inconveniences. Attendants accustomed to rely on the easy help of close restraints, were reluctant to abandon them; and unexercised in the resources without which their abolition produced inconveniences, which they were not likely nor able to compare with the remote evils produced by their continuance. Nor would the resident physician yet presume to say that strong restraint may never be required, but he begs to lay before the visiting magistrates a simple statement of the progress of an attempt to do without it. By a list of restraints

appended to this report, it will be seen that the daily number in restraint was in July so reduced that there were sometimes only four, and never more than fourteen, in restraint at one time; but that since the middle of August there has not been one patient in restraint on the female side of the house, and since the 21st of September, not one on either side.

"There have, however, been occasional and brief instances of restraint, unsanctioned in some cases by the physician, and which do not appear in this table; but it correctly represents the total absence of continued restraint during the whole period since August 9th. For patients who take off or destroy their clothes, strong dresses are provided, secured round the waist by a leathern belt, fastened by a small lock. For some who destroy the collar and cuffs of their dresses with their teeth, a leathern binding to those parts of the dress is found convenient. Varied contrivances are adopted, with variable results, for keeping clothing on those who would otherwise expose themselves to cold at night; and warm boots fastened round the ancles by a small lock, instead of a button or buckle, are sometimes a means of protecting the feet of those who will not lie down. As it is now and then necessary to confine the hands when a blister is applied, to prevent its removal; and as this, like all other temporary restraints applied with the justifiable plea of protection, is generally abused by being too much prolonged, or unnecessarily severe, a kind of cape, as a covering for a blister, capable of being secured over it, has been thought of, and will no doubt be found practicable. Those who are in the habit of striking suddenly, tearing the bed-clothes, &c., sometimes wear a dress of which the sleeves terminate in a stuffed glove, without divisions for the thumb and fingers. But no form of strait-waistcoat, no hand-straps, no leg-locks, nor any contrivance confining the trunk or limbs, or any of the muscles, is now in use. The coercion-chairs, about forty in number, have been altogether removed from the wards; no chair of this kind has been used for the purpose of restraints since the middle of August.

"It may be considered yet too early to pronounce a positive opinion on the general effects of these measures. In so large an asylum, filled with pauper lunatics, the means of mere mental control must always be limited, and the discontinuance of cruel restraints may only slowly be appreciated by the patients. But the resident physician is inclined to believe, after as careful observation at all hours as the space of a few months has permitted, and notwithstanding some peculiar difficulties, that the noise and disorder prevalent in some of the wards has already undergone diminution; that instances of frantic behaviour and ferocity are becoming less frequent; that the paroxysms of mania to which many of the patients are subject, are passed over with less outrage and difficulty, and that if cases are yet seen, which appear for a length of time to baffle all tranquillizing treatment, they chiefly, if not exclusively, occur in acute cases, the symptoms of which would be exasperated by severe coercion, or among those who, having been insane many years, have been repeatedly subjected to every variety of violent restraint.

"With respect to the discontinuance of the restraint-chairs, he may speak more confidently. Several patients formerly consigned to them, silent and stupid, and sinking into fatuity, may now be seen cheerfully moving about the wards or airing-courts; and there can be no question that they have been happily set free from a thraldom of which one constant and lamentable consequence was the acquisition of uncleanly habits."

The habitual comfort and cheerful alacrity of the attendants, who live in the atmosphere of excitement and anxiety, of which we have spoken, must by no means be overlooked. It claims even particular attention. Upon them it mainly depends, and ever must depend, whether or not *habits* of order, decency, and good behaviour grow up and strengthen. Unless *they* are well trained, the mental plan, however

enlarged, cannot supersede the physical plan: and the new discipline must be established, even for a few years, before its effects must be expected to induce conviction with those to whom every kind of restraint is dear. Whenever man obtains power over man, he relinquishes that power with an ill grace. He, said some ascetic philosopher, who has no intention to take away your life, is yet not sorry to have it in his hands. The rod, the lash, the chain, the constraining vest, the fretting hand and leg-strap, are only yielded one by one, with natural tears. But on these things another age will look back with surprise; rejoicing in the amended health, the improved appearance, the more decent demeanour of the victims of measures then exploded; and then, and then only, will it be known *how many* of the most wretched class of lunatics, commonly called "dirty patients," were rendered dirty by neglect, and by neglect alone.

The attendants, therefore, from whom so much is required, and upon whom, although of humble rank, the success of the noblest plans will turn, must receive all the consideration to which their harassing situation and their important duties entitle them. As many as can be spared with propriety from their duties, at one time, must be allowed frequent although short absences. Their remuneration must be such as to induce present content, and enable them to make provision for the years in which they will not have sufficient activity left to continue their peculiar labours. Arrangements should be made for their personal comfort. Their nights, except when they take their turn of night-duty, should be undisturbed. They should eat and drink in peace, if possible; at all events, they should dine in common rooms, one for the male and one for the female attendants; half of each dining at a time, after the toil of carving and distributing the dinners of their wards. Whilst their orderly obedience is to be rigidly exacted, they must be treated with kindness, openness, and confidence; made to understand the reason of what is exacted of them; not too sharply rebuked for occasional faults; and generously encouraged when they show a disposition to perform their arduous duties courageously and humanely.

We have said rather more on this subject than we designed to do on the present occasion; but it is time that it received universal attention; and we can assure those who sit apparently secure in the shade of old institutions, that we think they deceive themselves, if they suppose they can discourage the attempts of which we have spoken, or even creditably abstain from speedily making similar attempts themselves. Public attention is awakening, and public indignation may be aroused. Not to Lincoln alone, nor yet to Hanwell, is the great effort even now limited. Across the Atlantic the benign spirit of modern government has manifested itself. In Dr. Woodward's elaborate Report of the State Lunatic Hospital at Worcester, among many passages which we have read with admiration, none has gratified us more than that in which so experienced a physician states, that of 230 patients only *one* was, at the time of his writing, in personal restraint; and that five only were in strong rooms in consequence of violence; whilst, under a system of such leniency, "the furious and violent have become quiet and docile; the filthy and degraded have become cleanly and respectful; and the cir-

cumstances in which they are now situated, contrasted with the condition of suffering and wretchedness in which they formerly were, exhibit great improvement and decided benefit." (p. 58.)

So also, in the Bloomingdale asylum, another American institution, the fiscal concerns and moral management of which are superintended by six governors, Dr. Macdonald tells us that under their auspices the treatment of the insane is so mild and parental, that "it is long since there has been such a thing as a strait-jacket in the establishment;" and he adds, that "the utmost extent of freedom, consistent with safety, has been found the best substitute for these means of restraint."

The language of humanity is everywhere the same. All evidence, whether from *restrainers* or *non-restrainers*, bears on the same point, and wears the same colour.

The following case is from the American report:

"No. 3 is a case of homicidal insanity, the subject of which has been in confinement *thirty-four* years. Before he came to the hospital, he had for more than a quarter of a century been confined in a filthy dungeon, without the comforts of life, with neither bed nor covering to keep him warm, and infested with vermin to such a degree, that he could hardly sleep if the means of comfortable repose had been afforded him. He declares that for *seven* winters he did not feel the influence of fire, and that on one occasion a stout and healthy cock lighted upon a tree by the window of his cell and froze to death; this was the 'cold Friday and Saturday' which, in the recollection of all who felt its influence, was proverbially the coldest season of the cold. During these *three* days he declares he did not lie down or sleep, but kept continually walking to keep himself from freezing. He remained in this solitary and filthy cell, the object of the sport and abuse of every idle and mischievous person who took delight in the rage and violence which he could excite, till removed to the hospital. When he entered this institution he was furnished with a neat and cleanly room, a comfortable bed, and everything necessary for his happiness. He had not been shaved for many years, he had not eaten at table or in company, neither had he used a knife and fork during the whole period of this protracted confinement; he soon, however, relearned their use, and became, to a considerable extent, a civil, quiet man. Although the delusions of insanity remain the same, he is now comfortable and happy, he walks abroad at this time unrestrained, takes great care of the poultry, walks about the town and village in company with others, keeps his room in perfect order, makes his bed in the neatest manner, attends chapel every sabbath, and enjoys life as well as the nature of his delusion will permit." (p. 61.)

We might quote several cases of a like purport. The Lincoln report contains many more; but we can only make room for two or three of them:

"Nos. 547, 549, and 551.—These patients were admitted in January, 1836. They had all been confined in a workhouse for a number of years—say between fifteen and twenty. During this period of time they scarcely knew what it was to be at liberty: I have understood that they were chained both day and night to their bedsteads, and kept in a state so filthy that it was heart-sickening to go near them. They were usually restrained with the strait-waistcoat, with collars round their necks; the collars being fastened with chains or straps to the upper portion of their bedsteads, to prevent them (as I have since been informed) from biting their bedclothes: their feet were chained to the bedsteads with iron leg-locks, to which chains were attached. One of the poor creatures, who no doubt had her lower extremities at liberty, was so deformed from the continued

confinement, that she was unable to move about, her limbs having become contracted to such a degree that her feet were drawn up until the soles were even with the lower part of her back: when moved from one room to another, it was necessary for an attendant to carry her. These individuals *have never been personally restrained since their admission:* one of them I once placed under seclusion for a few hours only; but beyond that, no coercive means whatever have been employed. Two of these patients have been restored to habits of cleanliness:—one in particular now spends the greater part of her time in knitting, sewing, &c. Of course they have not been so orderly in their conduct as many of their companions; but is this to be wondered at, when for such a number of years they had been treated more like brutes than human beings? The crippled patient died a few months since of consumption.

"1838, April 12.—No. 662, æt. 20.—This patient has been readmitted this afternoon. She was brought in a strait-waistcoat, in a state of the greatest excitement. Five persons could scarcely bring her. She is single, and a member of the Baptist persuasion. This attack came on about a week since; the former about three years ago, from which she recovered, after remaining in this establishment about three or four months. She raves chiefly on religious topics, and is subject to sudden and violent fits of frenzy. During the former attack she attempted self-destruction by jumping into a stone-pit; she occasionally destroys her wearing apparel. Grief and religious excitement are assigned as the immediate exciting causes of the attack. 8 p.m.—She has been very active in her personal exertions, and is unable to control herself.

"April 13.—She has been under watch, and has been restless the whole of the night; she is still very active in her personal exertions, but more tractable than she was yesterday.

"April 14.—She has become quiet and orderly.

"April 15.—She continues calm and well-behaved.

"April 18.—The patient who was brought here on the 12th instant, confined with a strait-waistcoat, and guarded by several attendants, is already so far recovered as to have lost all disposition towards any inordinate action, and has been removed this morning to the moderate patients' gallery. It cannot be doubted, indeed she has herself stated, that *the irritation of personal restraint had occasioned the excitement she at first exhibited.* The strait-waistcoat was instantly taken off on her admission.

"April 20.—She was removed to the convalescent patients' apartment, and is now employed in needlework.

"From April 20 to June 11, inclusive.—Employed in household-work, needlework, &c. &c.

"June 11.—She has been discharged this day by the weekly board of governors. When discharged she applied for the situation of kitchen-maid, and was engaged." (p. 24.)

If Mr. Hill's cases lose any portion of their persuasive effect in consequence of his own zealous and successful efforts (a most unjust but not uncommon consequence), the testimony of those is not wanting who, without being committed to the abolition of restraint, are well known to have endeavoured to improve the management of lunatics. The excellent management of the Nottingham asylum, under its able and humane superintendent Mr. Powell, is so well known, that we cannot but regret to see the somewhat offensive term *Utopian* applied in the Nottingham Report to Mr. Hill's praiseworthy plans; plans which, if they were even to fail, it would be no dishonour to have tried. But it is nevertheless evident, that in the Nottingham asylum, except for protection, restraint is nearly, if not quite unknown. If a patient, to whom the effectual action of a blister is of great consequence, will not refrain

from pulling it off, the application of some restriction to the hands for a few hours can be considered no infraction of the non-restraint system. Other cases may be supposed in which the freedom of the hands may temporarily be encroached upon with convenience. That cited in the Nottingham Report is quite satisfactory. A patient had divided several rings of the trachea, and directed every effort to tearing open the wound. In such a case, we agree with Dr. Blake and Mr. Powell, that mechanical restraint was not only justifiable, but necessary; but we should call this *protection*, not restraint. If we were to call this restraint, we should call iron window frames, strong doors, and locks and keys restraint. It is the substitution of habitual corporeal restraint, severe, irritating, long-continued, in the place of practicable superintendence, against which Mr. Hill and all non-restrainers contend; and, as far as we are able to judge from the Nottingham Report, the condemnation of such restraint would be pronounced with no more earnest voice than that of the directors of that well-conducted institution.

We do not suppose, either, that there ever was a time in which the asylum at Hanwell was directed by those who thought restraint, even to the extent to which it might at any time be practised, anything better than a necessary evil. No stronger cases could be adduced against it than those cited in Sir William Ellis's Treatise.

> "I know an instance," he says, "where, from continued confinement day and night for years, the limbs had become contracted, the fingers twisted over each other, and the patient totally insensible to the calls of nature. Two stout, ignorant servants, neither of whom could read or write, had been the constant attendants. The maniacal violence and impatience of restraint with which the commencement of the disease was characterized, seemed to have banished from their minds every idea of treating the poor sufferer with decency or respect: and when the first violence of the attack had subsided, no solace was offered to the feelings of wounded pride; but a constant source of irritation remained, in the being obliged to submit to the domination of such associates. An airing was sometimes taken, though the miserable patient, tied hand and foot, was fastened in a blanket to the bottom of the chaise. No wonder that these circumstances should have produced their natural results, and that on an occasional visit from the friends sufficient violence should have been found as apparently to have made such severe confinement necessary. Even this case was not beyond the reach of amelioration. A removal into different society, kind, soothing, and respectful manners, the absence of all restraint, except during the actual continuance of the paroxysm, have rendered the patient cleanly, comparatively happy, and exempt from any exacerbation of the disease for six weeks together. Careful friction of the limbs has restored the use of the muscles, and the patient now enjoys a walk, or a ride, untrammelled. If such be the results where the disease has been of so long continuance, and the mental faculties [have been] apparently destroyed, no case ought to be considered sufficiently desperate to warrant the intrusting the patient at once to the society of the keeper or the nurse, or the neglecting any means which may possibly tend to cure the disease, or to diminish the sufferings."

We cordially concur in these sentiments, and have but to add our expression of concern and surprise, that the institution over which Sir William presided when they were written, cannot be quoted as one in which severe and unnecessary restraints have been unknown. It might

have been expected to stand free from the miseries of antiquated institutions.

Never be wearied, we would say in conclusion, to all superintendents of asylums, of devising means to avoid bodily coercion. Never be driven to restrain, by day or by night, one unhappy man or woman, from an apprehension that they *may* do mischief; for such a plan is unjust, cruel, and in the highest degree injudicious; unjust, because it punishes before the fault, and tortures on suspicion; cruel, because it consigns the injured being to dirty habits by rendering him helpless; and, above all, injudicious, because its direct tendency is to impede recovery. Let no one, omnipotent in the lunatic's cell, into which no eye penetrates but that of God, pass these considerations unregarded by; and, leaving his victim in bonds, retire to his own bed self-justified and deceived.

We must here say a few words on the subject of diet. In France, until 1819, lunatics were only allowed bread and water; but their diet is now that of paupers, consisting of soup, meat, and vegetables: wine is allowed in Paris and in the south; cider is given in Normandy, beer in the north. In Germany, on certain days, kept as it were sacred, (des jours solennels) the food is more abundant and delicate than common. It differs much in different establishments in our own country. At Lincoln animal food is allowed daily, but no beer. In some asylums meat is allowed four days in the week, and in some only three, and in very small quantity, seldom more, and sometimes less, than five ounces of meat when cooked. When this is the case, the lunatics have soup three days a week, and meat-pie, or more appropriately pie-crust, with less than two ounces of meat one day in seven. Such, until recently, was the diet-table of Hanwell; but the soup days are now reduced to two, and the quantity of meat is increased. In some asylums beer is freely allowed, in some very sparingly, and at Lincoln not at all. If allowed, it should be of good quality; and without much vigilance the patients will be defrauded of their just quantity, both of beer and bread, especially the imbecile and helpless, who may be half-starved with impunity; not understanding the wrong done them, or unable to represent it. Active patients, who are very useful helpers, are fond of beer; inactive attendants are fond of ease; and a compact is too often made between the parties at the expense of patients who are less alert.

But it is well observed by Mr. Farr, that "the adequacy of the dietary depends to a considerable extent on the amount of nutritive matter in the slops;" and the number of ounces of solid food allowed in a week constitutes the most important particular of comparison. We shall merely observe, that there is the strongest reason for believing that the mortality of those asylums is the greatest where the food is the poorest; and that the recoveries are the most numerous where the diet is the best. The extreme example of what an approach to starvation will do for lunatics is related by Pinel. In the fourth year (of the French revolution), the bread of the patients was reduced from a kilogramme to seven hectogrammes and a half, and by degrees to two: and the mortality which had been but 27 in the whole year, became 29 in two months. Cases of refusal to take food are less numerous than they are commonly supposed

to be; and the obstinacy is overcome, in some instances, by mere persuasion; in some by leaving the food with the patient, and saying no more about it. The stomach-pump does not appear to be generally resorted to; but when resorted to its first application is commonly productive of permanent success. All forcible methods of opening the mouth are very objectionable; with the exception of the simple one of closing the nostrils, which plan, aided by the use of a tin vessel with a perforated spout, guarded by a napkin, and introduced between the teeth when the patient separates them to breathe, appears to be the plan pursued when any device is requisite at Lincoln, and in some other asylums. The edges of the spout of these instruments ought, however, to be rounded, to prevent injury to the patient's lips or gums. With every practicable precaution, such devices are never quite devoid of evil. A strong suspicion is entertained by us, that refusal of food is often merely one of the results of the bad treatment to which the patient has been exposed. We believe it is not uncommon for patients to be brought into lunatic asylums, miserable in body and terrified in mind; marked with cords and chains, emaciated, bruised, and even wounded; and refusing, apparently dreading, to take food; and who after experiencing kind treatment for a few days, and once more feeling that they are not utterly deserted in this world, begin to eat and drink again.

The wretched manner of feeding lunatics and idiots, under the old system, when they received each his mess in a bowl or tin, and devoured it like wild beasts, is well contrasted by M. Esquirol with the comfortable manner in which they now take their meals in well-regulated asylums, where well-cooked food is regularly served to them, and they dine together. Their food, however, is not, he thinks, sufficiently varied. Its good quality and comfortable preparation are, doubtless, particularly conducive to the satisfaction and comfort of the patients, but it is by no means easy to induce them all to eat decently; this peculiarity of man, like that of combination of individual power, and of social union, being lost to the human animal when the mind is deranged. Lunatics are not intrusted with knives in every establishment; but this, like many other proofs of confidence, is possible, at least in many cases, where the attendants are sufficiently numerous. M. Esquirol mentions the plan adopted in some of the English asylums of giving them small knives, rounded at the end, the edge being only sharpened in the middle to about the length of three or four inches. Forks with very short prongs are also in common use. The superior safety of these instruments, as weapons of offence, is very doubtful. At Lincoln the food of every patient is cut into small portions in the kitchen. This should always be carefully done for the helpless. The just distribution of the food deserves a more exact attention than it commonly obtains; and weighing and measuring are alone to be depended upon. For patients who lie in bed, the most watchful care is necessary, to prevent their being left without proper food, and, with greater certainty, without their just share of beer.

M. Esquirol makes very few observations on the dresses worn in lunatic establishments. We cannot ourselves approve of the common and uniform garb so often imposed. Every tranquil and convalescent

lunatic should at least be dressed carefully, according to his station, and not put into the livery of a mendicant. Like every other source of mental irritation, capable of removal, this deserves consideration. In our variable climate, insane persons should be clothed in woollen, except in the hottest season. Negligence of dress arises in numerous instances from the negligence of the keepers. There are many patients whom no care can induce to keep any covering on their heads.

Repeated allusion is made by M. Esquirol to the importance of allowing space and exercise to the furious lunatic, whom strict seclusion has often exasperated. For this reason, among others, he prefers a large window opposite the door of each patient's room, which may be opened so as to permit his walking out of it without the necessity of opening the door. In case of necessity it is thus also rendered easier to surprise a dangerous lunatic, by feigning an entrance one way and effecting it the other. When a patient has a large window of this kind, he seldom breaks it; although, in other cases, the windows are often destroyed. An observation of this kind occurs in Mr. Tuke's Description of the Retreat, a work in which, we cannot but repeat, most of the enlightened and humane principles of treatment, now generally recognized, will be found pointed to; although many of them have been tardily acted upon in other asylums. We have always made enquiry respecting the occurrence of window-breaking, and it appears to us that the more simple and cheerful the window, the less hostility do the patients exhibit towards it. The confinement of lunatics in galleries or day-rooms, of which the windows are placed so high that they cannot look out, is most objectionable. Very light wire blinds, about eight inches from the window, in the inside, will protect the glass; and even these are only necessary in some wards.

M. Esquirol very properly considers nothing so minute in the management of lunatics as not to merit his attention; he makes several useful observations on the construction of the privies of an asylum, and is decidedly of opinion that they should be separated from the other buildings; the patients being enabled to reach them by corridors, which, with efficient superintendence, is found to be practicable.

In the arrangement of the beds or couches of the patients, the plan of placing them against the wall has inconveniences. M. Esquirol advises that for the violent they should be so detached from the walls as to be easily surrounded. The bed should either be fastened to the floor, or of such heavy materials as not to be moved by one person. Restless patients delight in dragging their beds to the door, or setting them up on end against the window; and the simple precaution of giving them beds too heavy to be lifted is often neglected where straps and strait-waistcoats are liberally applied to restrain these habits. For patients who cannot be kept clean, a bed with a double bottom is recommended: the lower one of wood, coated with lead, sloping towards the foot of the bed, and there perforated, having a reservoir of wood and lead beneath it; the upper bottom of the bed (*à claire-voie*), on which the straw and bed-clothes are placed, should be two inches from the lower. At Stafford, at Gloucester, and probably elsewhere, a strong ticking in a frame is placed some inches above the perforated bed of wood: each bed

has two of these, of which one is daily washed and aired. That plan, we should say, is the best which excludes the unpleasant smell so often found in such apartments. In the Gloucester asylum a strong petticoat is often found useful for the *male* patients, the complexity of whose lower garments is found incompatible with cleanliness. Perhaps the kilt and leggings adopted in the Nottingham asylum are preferable to this: but strict watching and attention are better than *any* contrivance.

We should be at a loss to point out any asylum in which the arrangements for the refractory, or rather for the noisiest patients, are yet so complete as to secure quieter patients from annoyance by day and by night: and there is, doubtless, considerable difficulty in effecting such arrangements, although they are especially desirable. The object is to provide for seclusion without subjecting the secluded to any unnecessary privation of light, air, warmth, space, and kind superintendence.

The neglect of proper means of warming lunatic asylums has been found productive of very bad effects, especially on those who are unable to leave their cells; and as the cells are often kept closed for warmth, the vitiated air has sometimes produced scorbutus and other serious maladies. These evils are at present, we hope, unknown in this country, where the apartments are generally all kept at a comfortable temperature. M. Esquirol condemns the arrangements, not unknown in some of our largest establishments, where may be seen assembled round a large iron cage, surrounding the fire-place, a crowd of lunatics, sitting on benches fastened to the floor, and to which some of the unhappy wretches are chained, to the great annoyance of the tranquil and the convalescent. We hope he is right in his conjecture, that these arrangements are now proscribed in England. An open fire, however, is a pleasant object to many poor creatures incapable of occupation; and in some asylums, the ventilation effected by the chimney is far from unimportant. At the best, an iron guard can only be looked upon as a mechanical substitute for other means of care; but not even the watchfulness of Lincoln is trusted to as a substitute for them. The warmth from open fires is less liable to become oppressive than that obtained by other methods.

In every asylum we are inclined to think the majority of the patients will be found to have a feeble pulse, and a small power of preserving the warmth of the extremities. They are prone to all the inconveniences of a low vitality; ulceration where pressure is made, mortification of parts remote from the centre of the circulation, and an inability to resist disease. These circumstances have a very important relation to the regulation of all parts of their diet and regimen, as well as to their medical treatment.

In a review, in our thirteenth number, of several works on insanity, we observed, that in some of the most recent of these an attempt was made to take more than due credit for the institution of labour among lunatics. At the Salpêtrière, says M. Esquirol, the word *work* resounds unceasingly in the ears of the female lunatics, and their ideas are found by this means to be led away from their distractions; their attention is roused; habits of order are established; and when the patient goes out of the institution, restored to reason, chiefly by this plan of labour, she

has also often become mistress of a small sum of money. He speaks of trades being followed by the male lunatics in many institutions; and particularly alludes to the asylums at Sarragossa and at Bareuth; nor does he omit to mention the Scotch farmer, whose method was quoted by Pinel. At the York Retreat the principle was long ago recognized, and to a certain extent acted upon. Much credit is due to the late Sir William Ellis for carrying it into fuller effect, although the employment of many patients in sedentary occupations is not favorable to health; and much pecuniary advantage is not to be expected from the labour of lunatics. To the lunatics themselves the labour is most salutary, being powerfully conducive to convalescence and recovery. The rich are to be pitied for the difficulty, in their case, of finding any suitable employment. If this can be done anywhere, it will probably be in the Crichton Institution; and the results cannot fail to be interesting. M. Esquirol advises gymnastic exercises; but what can be done for men who have been accustomed to hunt foxes four days in a week? We really believe that the best plan would be to consider them as grown children, and to establish schools for them in the asylum, on the model of infant schools, so as to give to their unexercised intellects some degree of power, and if possible to instil, even into the minds of those whose leisure hours have been filled up with pursuits little worthy of reasonable beings, some notion of moral responsibility. The sensual, self-indulgent habits of this class of patients, present a formidable obstacle in the way of their perfect recovery. Truly may we add, that no patients are more willingly abandoned, to any kind of treatment, by their nearest relatives and connexions.

To prevent the continuance of a delusion which can only lead to disappointment and mortification, we must repeat, that the work done in a lunatic asylum must not be calculated upon as very profitable in a mere pecuniary sense. Its true profit is of a remedial character. Six shoemakers, who are not sane, may not make more shoes in a week than two shoemakers of sound mind; and ten tailors, in the same predicament, may furnish fewer garments than three rational workmen; but then the whole sixteen are deriving benefit from the stitching, and welting, and hammering, and sewing, and cutting, and goosing, which is worth sixteen times the wages saved by their employment. If it is expected that the labour of the patients in any lunatic asylum will go far to meet the expenses,—an expectation which seems to have been formed by the French government, who have recently sent a deputation to England to enquire into this matter,—the expectation will most assuredly be frustrated. Nor can it be justifiable to apportion labour to lunatics with such an intention. Their malady, as Dr. Charlesworth has on many occasions justly pointed out, incapacitates them for severe and continued exertions. To exact constant day-labour from them may even counteract the remedial influence which is the chief value of employment. What may be done, however, in the way of employment, is shown by the fact that at Hanwell the number of patients, out of 800, who are usually employed, in some way or other every day, is upwards of 400.

Mr. Tulk, the chairman of the visiting magistrates, in a Report distinguished by such liberal and judicious views of asylum management,

as show how eminently qualified he is for the direction of so important an institution, has made the following remarks on this branch of the subject, which cannot have too extensive circulation :

"It has been asserted that many of the patients, by the value of their labour alone, could more than compensate for their various expenses. This assertion, calculated as it is to awaken the attention of the rate-payer, has been thrown out without enquiry, and is a complete delusion. It is not possible to make the labour of the patients in the asylum profitable, and to attempt such a thing would be inconsistent with humanity. Your committee must, in answer to speculations of this sort, repeat that a pauper lunatic asylum is not a workhouse; nor could you, without serious consequences, even if you choose to stifle every generous and kind feeling, attempt to get profitable labour out of an insane patient as you might out of a sane and healthy pauper. On this point your committee are satisfied, not only from the results of available labour in the Hanwell Asylum, but from the experience of similar well-conducted establishments. A few instances will be sufficient to show this. In the month of September, 1839, there were ten patients in the asylum employed in the making of shoes, and during that time eighty pounds of leather were delivered out to them. Their labour for that month produced 152 pairs of shoes. The cost of the leather, with cloth and linings, was 6*l*. 13s. 4d. The value of the shoes, at 1s. 8d. per pair, was 12*l*. 13s. 8d. If the cost of the materials be deducted from this, there is left, as the value of their labour, the sum of 6*l*. 0s. 4d., or about 28s. per week, which as nearly as possible pays the cost in wages and board of the shoemaker who superintends them. The same number of sane workmen, had they been employed for the same time, instead of 152 pairs of shoes, would have produced 960 pairs. Still more remarkable is the profit of coir-picking. Of coir-pickers there are, upon an average, 150, whose annual labour produces no more than about the sum of 5*l*. But the value of coir-picking to those patients who can do nothing else, and who, without this simplest of all occupations, must sit or wander about listless all day long, is not to be estimated in money, but must be tried by a far higher standard." (*Fifty-first Hanwell Report*, p. 7.)

The following passages from the Dundee Report afford valuable help towards forming a correct judgment of the real value of the work done in asylums :

"Since the present superintendent and matron undertook the management of the asylum, it has been much distinguished for the variety and extent of the exercises in which the patients have been engaged; and the happy effects of labour on the health and welfare of the patients are now so well known and acknowledged, that many enquiries have been made, and many visits have been paid, by professional and scientific men, to learn the means by which so satisfactory a result had been obtained. The debt was no doubt considerably increased by the erection of workshops and in providing manufacturing utensils; but there is a fair prospect that this debt will be ultimately liquidated and the pecuniary interests of the asylum promoted by the industry of its inmates. The following Table will give a distinct view of the condition of the patients in regard to their ordinary occupations :

NUMBER GENERALLY EMPLOYED, 1838-39.

	Males.	Females.	Total.
Weaving linen for sheeting, cotton bagging, &c.	13	6	19
Picking oakum	11	0	11
Tailoring and mat-making	2	0	2
Cutting firewood	1	0	1
Mangling clothes	1	0	1
Pumping water for the use of the establishment	3	0	3
Breaking metal for the turnpike road, and gardening, trenching, and laying out ground	21	0	21
Domestic purposes	1	0	1
Shoemaking and mending	2	0	2
Clerks	1	0	1
Dressmaking	0	2	2
Shoe-binding	0	1	1
Spinning	0	11	11
Winding for weavers	0	6	6
Knitting	0	2	2
Shirt-making	0	2	2
Quilt-making	0	1	1
Upholsterers' work	0	2	2
Stay-making	0	1	1
Flowering muslins	0	1	1
Fringe-making	0	1	1
Repairing bedding and clothes	0	3	3
Worsted works	0	1	1
Assisting in laundry	0	1	1
in scullery	0	1	1
in kitchen	0	1	1
in bed-rooms and wards	0	4	4
Marking clothes	0	1	1
Total	56	48	104

Ladies and Gentlemen not included in the above.

Work done by male lunatics.

542 Webs of bagging wove.
23 of sheeting, &c.
934 Cwt. oakum picked.
22 Pairs trousers made, in addition to many mended.
13 Waistcoats ditto.
2 Flannel jackets ditto.
14 Coats and jackets ditto.
2 Pairs drawers ditto.
56 Cubic yards metal broke.
55 Pairs leather shoes and boots made, in addition to many mended.

N.B. Garden works cannot be included here, nor cleaning of wards.

Work done by female lunatics.

27 Short gowns made.
32 Long ditto ditto.
60 Aprons ditto.
206 Caps ditto.
50 Petticoats ditto.
55 Shifts ditto.
36 Mattresses ditto.
28 Bolster cases ditto.
52 Pillow ditto ditto.
34 Pairs sheets ditto.
18 Ditto stays ditto.
42 Flannel waistcoats ditto.
38 Pairs flannel drawers ditto.
48 Ditto stockings knitted ditto.
81 Men's shirts ditto.
100 Webs sheeting wove.
400 Spindles hemp spun.
260 Handkerchiefs hemmed.
55 Pairs shoes bound.

And winding pirns for 665 webs.

The elegant and other articles prepared by the ladies cannot be inserted here.

"During the erection of the buildings, the patients are busily employed in throwing the foundations, making drains, and in removing earth and rubbish. By their labour several mounds have been erected within the airing grounds, paved on the top, which command a view of the surrounding country, and dispel the monotony and tediousness of a life much secluded from the world.

"The occupations of the higher classes are more varied, and their tastes are gratified in every reasonable wish and healthful exercise. Their amusements and recreations have been particularly specified in former reports: It may be proper, however, now to add, that, for some time, they have been indulged with more frequent excursions into the country; and for this purpose, open and close carriages are provided for them twice a week. There is one fact that cannot be passed over in silence, as it has excited feelings of surprise in the breasts of those who have most experience in the treatment of the insane: A lady of the highest class some time ago expressed a desire that some children were put under her care,—her wish was gratified. A few children from the neighbourhood attend her daily, and she superintends their education, not only with maternal tenderness but with prudence, temper, and judgment; and, amid the many privations to which she is subject, she is conscious that she is not living in vain, but is emplcying the means within her power of conferring some benefit on our race.

"The Directors have now full experience of the beneficial results that have flowed from the introduction of labour into the asylum, and they have the satisfaction to know that the practice adopted here is closely followed in a number of similar institutions. There is no doubt that these exercises have increased the number of cures, ameliorated the condition of the patient, and shortened the period of the residence in the house; for there are not a few who are now restored to the peaceful bosom of their own families, happy in the consciousness of conferring benefits on others, who without these means would have still been lingering in the airing grounds, sinking under the heavy burden of imaginary evil, or the pitiable victims of illusory hopes. Labour, exercise, and social intercourse, preserve the bodily health of the patients, prevent the mind from brooding over its own disease, and are a most useful associate to the other remedial measures that are usually adopted."

In the latest report of the Retreat at York, the same subject is spoken of with characteristic moderation and good sense:

"The subject of the employment of insane persons in manual labour, with a view to the promotion of bodily and mental sanity, has of late claimed much attention in several asylums, both in Britain and in Germany. The employment of the patients has always been deemed an object of great importance in the Retreat. It has been, from the commencement of the establishment, extensively introduced among the women; and a considerable number of men have also been, from time to time, engaged in the garden, and in other occupations. Particular care has been exercised to endeavour to lead the convalescent patients into some regular employment, adapted to their taste or former habits; and there can be no doubt that these engagements have been highly salutary; but the attempts to introduce a *general system* of employment amongst the men patients have heretofore proved unsuccessful. These experiments have usually been made more with reference to mechanical than agricultural work; but the committee, encouraged by the success of several other establishments, determined to make an effort to induce a number of the men, heretofore wholly unemployed, to engage in gardening and agricultural labour. For this purpose an attendant has been engaged, to take the more immediate charge of the patients employed in labour, and the experiment has now been going on for about three months. The period is too short to determine our permanent success; but we may state that the number of patients who have been induced to work, and the quantity of work done, have both of them exceeded

our expectation. Nineteen men patients have been more or less employed during the three months in agricultural work ; the average number so engaged has been about fourteen. They have dug out a grass field of about two acres, and it is now promising a very abundant crop of potatoes and turnips, chiefly the former. In our next report, we purpose entering into a more particular detail of the experiment; we will therefore only add, that the employment has evidently conduced to the comfort of those who have been engaged in it.

"It may be asked: Is it safe to intrust the insane with the requisite instruments for the labour we have described? The answer to this enquiry from eight asylums in Great Britain and Germany, in which the labour system has been introduced, and from which we have obtained particular information, is, that no accident whatever has occurred in connexion with labour ; and they are equally unanimous in stating, that the danger of injury to patients or their attendants, is much greater under the *mere safety system*, than under that which puts spades, rakes, and hoes, into the hands of a large portion of the men patients. Of course discretion is used from day to day in the selection of patients for employment, and care is taken to place them in the charge of suitable attendants." (p. 5).

The Reports of many other institutions now contain allusions to the occupation of lunatics, (those of Aberdeen, Montrose, and the Worcester State Hospital, among others;) but few or none allude to any species of amusement afforded to them, even for the patients of the richer class. It is indeed one of the many disadvantages of persons in the poorer stations, that they have never known any variety of innocent amusements in their lives; and that the very word amusement, when applied to them, awakens associations of unruly behaviour, low gratifications, and sensual irregularity. The strange attempt made long ago at Charenton, and which M. Esquirol relates and stigmatises, to have plays performed by and to lunatics, was both delusive and unsuccessful. We are fearful that lunatic dances will not be found less objectionable, except on rare occasions, and for a short time, and with careful restrictions. Music affords many lunatics a high degree of pleasure. Many who are considered refractory or dangerous, will listen with evident satisfaction to a musical performance. Tears will flow from the eyes of those who show little other sign of sensibility. Yet very few lunatics, among the common people at least, can sing harmoniously; the voice, like the mind, seems "jangled out of tune, and harsh." Playing with a ball, and riding on large and well constructed rocking horses, are the chief amusements of the female patients at Hanwell; and it is found there, as it must be in every pauper asylum, that nothing is more difficult than to furnish a variety of recreation. Various light forms of needlework, feeding poultry, nursing cats and kittens, attending to singing birds, are a kind of substitutes for other amusements among the female patients; but the men are without these resources. Cricket might be practicable; and a bowling-green is found at Nottingham to be a great source of pleasure. We must not presume to mention skittles, which have never enjoyed much magisterial favour; nor cards, which the gambling of the higher ranks has deprived of their inherent harmlessness. Draughts, backgammon, and chess, furnish some with amusement for a few hours in the day. We are now speaking with reference to a class of lunatics, few of whom read with much satisfaction or even facility. For those of higher rank, the resources of amusement

are sufficiently numerous. Dr. Macdonald mentions, as adopted in the Bloomingdale asylum, "gymnastics, nine-pins, quoits, the care of domestic animals, draughts, chess, backgammon, dominoes, bagatelle, battledore, the graces, painting, ornamental and plain needlework."

In apportioning the duties of the officers, the principle is not so invariable and simple as M. Esquirol appears to consider it. For instance, he imagines the obvious division of the economic and medical departments to be universally distinct. They assuredly ought to be so, and we presume they are so in nearly every country except our own; where the physician of such establishment is sometimes chiefly valued for his excellence as steward, farm-bailiff, and overseer of works; the office of physician in ordinary being intrusted for the most part to the matron. Of such a state of things, M. Esquirol appears to have no conception.

"In every lunatic house," says he, "the higher functions are divided into two very distinct orders. To the director, steward, or superintending agent, belongs the general administration of the *matériel* of the establishment, the responsibility, maintenance, and execution of the regulations relative to the admission and discharge of patients, and the superintendence of the conduct of the different individuals employed in the establishment (employés). The chiefs of such establishments should have frequent communication with the physician in chief, and have an understanding with him in relation to all the changes and ameliorations that may be required for the interests of the patients confided to their high superintendence. To the physician should be reserved the supreme direction of all that immediately concerns the patients and the medical service."

By this arrangement, the physician would be enabled to attend to his proper duty, *the care and cure of his patients;* which he cannot be if he is steward, and butler, and builder, and farmer, and brewer, and baker, and secretary, and treasurer, and market-gardener. The comprehensive genius of English physicians is alone equal to those multifarious duties. In an institution mentioned by Dr. Crowther, the director was in the habit of visiting the wards once a week, and sometimes only once a fortnight. We may here observe, that Dr. Crowther has very ably drawn the character of a useful physician to such an establishment; and he justly observes, that the number of cures depends upon his unremitting daily labour.

We attach so much weight to every opinion of M. Esquirol, that in addition to the remarks already quoted from him in this and a former number, on the requisite qualifications of a physician to a lunatic house, we cannot but cite his opinion on the same important point in the article on lunatic houses.

"The physician should be, in some sort, the principle of life of a lunatic hospital. By him everything should be put in movement: he directs all the actions, for he is called to be the regulator of all the thoughts. To him, as to a centre of action, should be referred all that interests the inhabitants of the asylum; not only what relates to medicaments, but also what relates to hygiène. The agency of the administration which governs the *matériel* of the establishment, and its superintendence over all the *employés*, ought to be hidden; the director should never appeal against a decision emanating from the physician, and should never interfere between him and the lunatics, or between him and the keepers (*serviteurs*). The physician should be invested with an authority from which nobody should be exempt. . . . . The physician at his visit dictates his prescriptions to a medical and pharmaceutical pupil; the superin-

tendent of the men, and the female superintendent of the women, assist the physician, each in their department; every servant is near the patients, to give an account of them, and to answer questions. The physician ascertains the condition of each lunatic when admitted, and directs his place in the asylum; he orders his removal from one quarter to another; the medical police of the house belongs entirely to him; he prescribes the use of the strait-waistcoat, of restraint or coercion, of baths, of douches; he indicates the kind of amusement, or of occupation suitable to each patient, accords recompenses, &c. &c. With him rests the permission to see the patients; he grants certificates of cure and of discharge, and gives admission to visitors to the interior of the hospital."

It will be observed by every reader, that M. Esquirol considers the treatment of the disease,—of the insanity, as the *first* object in every lunatic asylum. But we must here repeat, what we have before and more than once said, that when we consider the deficient knowledge of many lunatic house-keepers, their total want of acquaintance with medicine, the fulness of their ignorance respecting the human mind, and their mere trading habits, the inevitable result is, a conviction that the restoration of the patient to his senses is the *last* and not the first thing thought of. When an appointment of this kind happens to be vacant, fifty candidates start up for it, with various pretensions, the rarest being that of having even seen a lunatic before. Whole books of testimonials are collected; asked for without a blush, and granted with shameless disregard of the qualifications required: it being too evident that any man who has followed any branch of medicine, or surgery, or pharmacy, is thought fit to undertake the management of the insane.

The sentiments which we have endeavoured to diffuse, supported by the high authority we are enabled to adduce in favour of them, will, we trust, be soon too universally entertained to allow this system, so disgraceful to medical science, and so outrageous to humanity, to exist in any quarter of the kingdom.

The difficulty of procuring proper keepers has been felt by every one who has had the care of lunatics. This office, whilst it offers no compensating advantages, demands a somewhat unusual combination of moral and physical qualities; firmness, forbearance, courage, patience, good sense, humanity, and the utmost promptitude of action, together with sobriety and all the cardinal virtues, are some of the requisites of a keeper who can be intrusted with the details of an enlightened director. Such persons are not easily met with; and servants of lower qualifications are hired at lower rates; and even these are not always sufficiently numerous to pay strict attention to every patient, or to support one another's authority. Their small number in many asylums is evidently compensated for by the habitual restraint of the patients, and by debarring them from proper exercise, and a degree of liberty compatible with efficient watching. In the old Bedlam, says Esquirol, there were five keepers for 120 male lunatics, and two for 120 females. We have found them vary at present, in different asylums, from one to 25 patients to one to 7 patients. In France the keepers are one to 10 patients. M. Esquirol speaks highly of employing convalescents as keepers: they are, generally, he says, compassionate to the infirmities which they have themselves suffered; they aid the physician more efficiently; and their example is encouraging to the other lunatics. The keepers, he adds, should observe a passive and absolute obedience to the orders which they receive

in the presence of the lunatics; they should not give an account of such patients as are present; nor should they remain too long in one division of the hospital. To a certain number of them there should be a superintendent of superior education, capable of conversing with the patients, hearing their complaints, consoling them, and encouraging them. In some asylums we have found old military men among the best keepers; keeping up their own authority, and yet obedient to superior orders. With a few such men, having well-selected young men from the country under them, the staff of keepers might generally, we think, be made very effective. Much of the difficulty of procuring efficient persons, male and female, will be overcome where they are liberally paid, and their comfort is in every particular attended to; circumstances indispensable to their cheerful performance of duties demanding sure sacrifices, and even attended with occasional danger. It should ever be remembered, that whatever the skill or diligence of the physician and matron, the keepers and nurses will be paramount in the wards, over the violent and the tranquil, over the feeble and the sickly, over the peevish and the melancholy, for at least sixteen hours out of the twenty-four.

When speaking of the treatment of lunatics in a former number, we had occasion to mention M. Esquirol's humane objections to the rotatory chair, the bath of surprise, and other ingenious inventions for subduing the refractory. He is evidently of the opinion which we have already expressed, that an asylum in which such measures are resorted to, or in which many of the patients are closely shut up, or chained, or confined in the strait-waistcoat, is deficient in moral government and mental discipline. Short seclusions (*la réclusion momentanée*), the camisole (a waistcoat with long sleeves), applied for a short period (*pendant quelques instans*), confinement in an arm-chair constructed for the purpose (*fauteuil de force*), the douche bath long continued, cold affusions, and certain privations, are, he observes, more than sufficient as means of repression, in the hands of a physician who knows how and when to employ them; at favorable times and with moderation: and none of these means should be ordered except by the physician, nor practised except in his presence or that of his head superintendents. M. Leuret cures many patients of their hallucinations by the douche, which he applies until they recant; but we have great doubts of the propriety, or even of the general success of this practice.

M. Pasquier, of Lyons, formerly physician to the hospital of Antiquaille, near that city, into which establishment insane patients, and those affected with cutaneous and venereal affections are admitted, has devoted his attention to the question of the best of the various plans for an asylum devoted to lunatics alone. The hospital of Antiquaille was originally a convent; but its romantic situation, on the declivity of a mountain, however suitable to its first destination, is inconvenient for an hospital for lunatics, offering no facilities for airing-grounds or sufficient divisions, or means of seclusion. In 1821, when M. Pasquier became physician to the hospital, there were one hundred and eighty-two lunatics there, and no kind of classification had been attempted: all that was practicable was the separation of the curable, who constituted five-eighths, from the rest. The violent patients were necessarily shut up until they became calm: the convalescents enjoyed no separation from the rest. The

reform of the institution began at this period. Chains and other means of violent repression were abolished. The results of the improved treatment were very favorable; but we can only refer to the second portion of M. Pasquier's pamphlet, in which he lays down a plan for an asylum for 500 lunatics. He considers a building of an octagonal form, with wings radiating from it, as the most convenient; and he objects to the plan of a building with several isolated divisions, as creating difficulties in the way of superintendence, and productive of great expense. We find no writer taking the enlarged view of this point which M. Esquirol does, who conceives that the plan which leads the most directly to the *cure* of the patients may possibly prove to be in the end the cheapest.

The first division which M. Pasquier would recommend would be for the sexes; the second for epileptics; and the others according to the duration and intensity of the symptoms. Consequently there would be a division for the curable, one for the incurable, and one for the convalescents. A division for the younger patients, and a division for those above the class of paupers, he also mentions as desirable. He would further subdivide the epileptic lunatics into the furious and the tranquil. But the difficulties of classification are always greater in practice than on paper; and it would be, we apprehend, not only shocking but hurtful to put all the violent epileptics together. Their distribution among patients, able and not unwilling to aid them in their fits, would seem the kindest way of providing for all epileptics; but even this has some obvious disadvantages. M. Pasquier would subdivide the incurable, so as to separate the paralytic and infirm, those who are dirty in their habits, those who are turbulent, and those who are calm and orderly; and for the latter he would have various workshops. The convalescents would be secluded from all the rest, and placed in the neighbourhood of the physician, apothecary, chaplain, &c., and near the gardens, so as to afford them diversity of amusement. A farm, baths, &c., would form a part of the establishment. The government of the institution, approved of by him, would apparently be nearly that recommended by M. Esquirol. Some excellent remarks are made on the hygienic management of the hospital; but they would not be new to any of our readers. The most important point in the pamphlet, is that which relates to the classification of lunatics, which ought, we are convinced, to comprehend even more subdivisions than are mentioned by M. Pasquier, and many more than exist in any of our public lunatic institutions. As regards the convalescents, especially, we conceive that very careful subdivision should be attended to; and indeed we think the whole subject of convalescent treatment so important, that it might very properly employ the ablest practical pen to treat it properly.

We would suggest the propriety of so arranging the reception-room and wards, that all the new patients, or certainly all the recent cases, and all the tranquil, on their arrival at the asylum, should be exposed as little as possible to sights and sounds capable of aggravating their distress, and making the change insupportable to them. A cheerful room of reception, and direct access to a quiet ward, under the care of a keeper or nurse of good appearance and tried kindness of heart, would make a lunatic house a real *asylum* for many a poor wretch who

is now plunged, without remorse, into the most revolting scenes that a refractory ward can furnish.

After these remarks on the latest foreign publications in our hands on this subject, it seems but just to mention that a considerable portion of Sir William Ellis's Treatise on Insanity is devoted to a consideration of the construction of asylums, and the mode of their management. The following we presume to be, in fact, a description of what Hanwell is, or was intended to be:

"The residence of the superintendent and matron, with the various business offices, should be placed in the middle of the centre; and behind these should be the kitchens, sculleries, washhouse, bakehouse, brewhouse, &c. &c., so as to admit of easy access from the centre The wards for the males should occupy one side, those for the females the other side of the building. If the whole of the ground-floor is elevated, which it ought to be, in order that it may be perfectly dry at all seasons, a passage may very easily be made in the basement, from the kitchen to the extreme corners of the central part of the building, along which the provisions, &c. &c., may be conveyed from the various domestic offices, and from these corners to the different wards of both the male and female patients. The gardens, farm-yard, and all other buildings connected with the out-door labour, should be placed at the back of the various offices, from which there should be easy access to them. The airing courts for the wards in the centre of the building, will be on each side of the domestic offices; and, of course, completely separated from each other; those for the side-wings ought to be placed on the east and west sides. If it can be conveniently managed, the entrance to the building should be on the north side, as it is much more cheerful to have the galleries in which the patients walk to front the south; and it is never well for them to be so placed as to be able to see all the persons coming and going to the asylum."

All the observations appended to this description bear upon Hanwell. The healthiness of its site is enforced by the observation, that although few patients are received at Hanwell "until organic disease of the brain has taken place, to such an extent that they are incurable when they are admitted, yet the air is so salubrious, that the deaths, in proportion to the average number of patients in the house, are fewer," &c. &c.

Sir W. Ellis recommends that the building should be of brick or stone, and fire-proof; the roof of iron, and containing cisterns for hot and cold water. He advises a building three stories high. We have seen the many strong reasons advanced against this by M. Esquirol: but, says Sir W. Ellis, "an important saving may be effected by it." We are decidedly of opinion that where this is the general plan of the building, there ought at least to be separate buildings of one story, for the refractory and some other classes of patients; for the sick and for the convalescent; and these ought to communicate with a distinct court or space for exercise. What is proper must not always be sacrificed for what is convenient.

A keeper is recommended as necessary for every twenty or twenty-five patients; and each ward, it is advised, should contain that number only; and for each fifty there should be a dining-room. This number of keepers is decidedly too small for perfect attention to patients in the state called moderate; and quite insufficient for the refractory, for whom the keepers ought to be as one to six or seven. The larger the wards are, the more difficult becomes the proper subdivision and

classification of the patients. Large wards, with a keeper's room at one extremity, are, in many respects, inconvenient.

A great variety of minute details are given by Sir. W. Ellis respecting the plan of the house, which are of much interest and importance, but must be read in full to be clearly understood. The regulation of the asylum, as described by him, is more connected with the subject of our present observations. We observe, however, that the establishment of a "walk for spinning string" is strongly recommended, as conducive to the comfort of the patients; and where the galleries, or adjoining wards, cannot be converted to this purpose, a covered way, erected expressly for it, is thought important. For every hundred patients there should be, Sir W. Ellis says, about sixty-six sleeping apartments; and the "sleeping apartments for single patients should not be less than eight feet six inches long, and six feet nine inches wide, and twelve feet high." The remarks on the drains, water-closets, &c., the proper regulation of which is so important wherever a number of individuals are collected together, and so especially important where a love of mischief is superadded to negligence, are very judicious. It is advised that none of the doors be panelled; lunatics breaking through such doors with great facility.

The ventilation and warming of a large lunatic asylum is always a subject of anxious consideration; particularly in the present day, when writers attack a stove with as much severity as if the inventor of a means of keeping mankind comfortable were little better than a criminal; and the troubles of hot air rival the proverbial troubles of hot water. The perfect ventilation of a lunatic asylum is especially desirable. Modern science scarcely appears to have mastered the difficulty of giving a constant supply of pure and dry air, of a moderate temperature, and without exposure to draughts, to buildings of large size, crowded with human beings. Although the difficulty of warming a large building with hot water is very great, Sir William Ellis is inclined to think that this might be still most advantageously done by means of a complete apparatus for each ward. "Pipes heated by steam, and passing under the floor of the galleries, after many experiments, appear the readiest and best mode of heating any very extended building, by one or two apparatuses." "Upon a trial of the two plans at the asylum at Hanwell, it was found that the pipes heated by steam attained the temperature of two hundred degrees of Fahrenheit in an hour and a half; and eight hours elapsed before the same length of pipes, heated by hot water, reached the temperature of one hundred and thirty degrees." If the joints of the pipes are made of iron cement, they are said not to give way, and no wet or discomfort is made in the apartments. Dr. Woodward, of the Worcester State Lunatic Hospital, ascribes the healthy state of that institution in a great measure to the arrangements for warmth and ventilation, both of which objects are effected by hot-air furnaces in the basement. He is of opinion that every other mode of warming is objectionable; and that no other assists ventilation. The object, he observes, is to keep up a constant and regular influx of warm, pure air, in such abundance as to change the whole atmosphere of the apartments frequently. He recommends that the furnaces should be placed in the basement, that the air-chambers should be capacious, and the passages

large; and thus a considerable volume of air may be forced into the apartments, heated not many degrees above the temperature at which they are to be kept; and thus the whole air may be frequently changed, and the foul air forced out at the ventilating passages. In all cases the external air, and not the air of a cellar, should be used. The proportion of the outlets for bad air must have a relation to the inlets for the good air. All these are points of importance, and, we suspect, not yet fully understood by practical men.

Whilst care is taken to introduce warmth into large buildings, the admission of fresh air is often almost forgotten. A supply of warm air in winter is desirable, whatever mode of warming the rooms and galleries be adopted; and in all buildings intended for the reception of a great number of human beings, the necessity of an abundant supply of pure air should strictly be kept in mind before the general sickliness of the establishment points to the oversight.

There is, perhaps, no institution in which lunatics have been more systematically employed in various kinds of labour than at Hanwell; and where this is done, Sir William Ellis thinks that it is not necessary that the airing grounds should be very large or numerous. We deem this very questionable; and certainly the cheerful character of the airing grounds is seldom sufficiently attended to; their aspect, when all view is excluded by high walls, being at least calculated to aggravate rather than to cure melancholy. If it could be made consistent with vigilant inspection, the airing grounds should be so disposed that the patients should not always see in one view the whole extent of their walks, and the actual narrowness of the boundary. M. Esquirol was particularly struck with the deficient space allotted in English lunatic asylums for the exercise of the patients in the open air. Certainly, the monotony of the airing courts is often grievous enough; gravel below; and high walls around; and the sky above;—not a tree, not a leaf, not a blade of grass to be seen. We should be glad to find the example of the York Retreat followed, by animals of various kinds being kept in the courts; particularly such as would become familiar with the patients, and consequently a source of innocent pleasure, tending even to awaken the social and benevolent feelings. (*Tuke's Description of the Retreat, p.* 96.) The disposition of the moderately extensive grounds at Lincoln deserves to be mentioned with approbation: the mere site of the Lincoln asylum is matchless in this country.

Some of the inmates at Hanwell have learnt to occupy themselves in trades with which they were previously unacquainted: six, for instance, have become shoemakers. There are, also, or were, shops for joiners, painters, glaziers, brush-makers, and coopers. Many of the female patients are employed in fancy and plain-work; and we believe every keeper, nurse, and servant contrives to find a helper, and consequently has much official toil done by deputy. The brew-house, the bake-house, the laundry, the kitchens, large and small, thus become so many convalescent wards.

The Hanwell asylum is placed under the management of fifteen of the Middlesex magistrates, who meet there once a month, and also visit it at uncertain intervals, sometimes inspecting the whole and sometimes a part of it. The effect of these visits is represented as being very

useful; and we cannot doubt that they are very advantageous. The Reports just published, although they contain allusions to past untoward circumstances, clearly manifest a uniformity of views and sentiments in the governing and medical bodies. Without this, the distraction of the officers would rival that of the patients. It is of the utmost importance to the maintenance of the effective discipline of an asylum, and of that unity of plan which should pervade every department of it, that the magistrates, or governing committee, should not consider themselves as by their office exempt from conformity to the plan, or suppose that they can, without injury to the institution, scatter throughout its wards expressions at variance with the sentiments of the officer to whom, as a temporary chief at least, they have deputed authority; and who, if his plan is defective, may be admonished; if pernicious, dismissed; but must not, amidst his anxious toil and thought, be teazed and thwarted at every turn. Few days will pass without his being troubled by unexpected accidents, and depressed by unavoidable disappointments: if he is also stung by perpetual irritations, he will either seek security in indolence and indifference, or run much risk of becoming qualified for a place in one of his own wards. Even Pinel laments, in bitter terms, the incessant opposition to which he was subjected, and the harassing manner in which his plans were long obstructed by those who had probably formed no conception of his enlarged and disinterested views. Dr. Crowther, in a work noticed in our Thirteenth Number, expresses himself strongly concerning the difficulties incidental to magisterial management, a point on which we took the liberty to differ from him. We repeat, after long experience of committees of private gentlemen, that we should infinitely prefer them, as a governing body, to any exclusive collection of professional men; expecting from them more varied experience, less jealousy, and fewer conflicting prejudices on practical subjects, as well as a smaller disposition to vexatious interference in petty details. Awkward members are to be found on all committees, we do not doubt; but a preponderance of magistrates disposed to narrow views, or afflicted with a suspicion that all whom they employ are in perpetual combination to deceive them, is surely not to be feared in this country. If, unhappily, there should be a disposition to cavil over every act of the physician or superintendent, and to insult some officer or other every month, the asylum would soon exhibit the results of such disturbing forces.

It is evident that Sir William Ellis, who was steward, and treasurer, and farmer, and much besides, was so overladen with duties, that some part of them, and especially the medical and moral, was of necessity delegated to others. Thus, immediately after breakfast, the house-surgeon, who is the physician's assistant, is described as going round the wards and seeing all the patients: he then "makes a report of any new case of sickness to the physician, whom he subsequently accompanies in his rounds: he also makes up all the medicines, and keeps the medical case-book. In the afternoon this officer again regularly goes round the wards," &c. In short, the physician in chief sees the new cases; but the daily and almost hourly inspections, the constant superintendence, the study of the different cases, and the records of them, seem to have devolved upon the assistant. It can scarcely be otherwise; for the

physician, under such a mechanical system, must be receiving or paying money, buying stock for the farm, and mindful of the caprices of the market. Nay, the assistant medical officer must become an assistant to the matron also. It is written in Sir W. Ellis's book that "this officer (the house-surgeon) and the clerk, in each week, inspect the stock of linen, bedding, clothes, &c., in each of the male wards, and, comparing it with the inventory, report any deficiency to the matron."

We are well aware that the moral influence exerted over at least half of the patients and all the nurses by the matron is of very great importance. Supposing the physician to possess every requisite for his situation, he must be nearly powerless over a great part of the establishment without the intelligent and philanthropic co-operation of the matron, on whose goodness of heart and firmness of character it will depend whether for that half of the establishment his wishes are fulfilled or frustrated. Female ingenuity and womanly tenderness can alone devise and apply many means of encouraging and soothing the female patients; reanimating them by confidential employments, and influencing the feminine heart by arguments and suggestions which man's less delicate sensibility can scarcely apprehend; so as to promote convalescence by modes of which he could scarcely ever be perfectly master.

The duties, then, of a matron, without encroaching on those of the physician, are, we acknowledge, scarcely less important. Sir William Ellis, with an implied compliment to the conjugal state of very doubtful desert, conceives that these two functionaries should if possible be married. This of course effects the unity of interest so steadfastly advocated by Sir William; but it may also open a door for confederacy of mismanagement. Yet the matron only can fully carry into effect, as regards the female patients, all the designs of the physician. If gifted with benevolence and understanding, her influence becomes beneficially exerted over at least one half of the asylum; and it will often happen that the zeal and uncalculating philanthropy of a woman will accomplish in a few weeks what man, more cautious, would scarcely achieve in as many months. But we should not wish to see the matron the real physician of the house, and to behold the physician in chief up to his knees in the straw-yard, or plagued to death with the accounts of the butcher, and the baker, and the pig-jobber. That the precise line of duty of all the officers of this establishment required some revision at the time when Sir William Ellis's book was published, is pretty evident from such passages as the following; in which, at the same time, some good and convenient arrangements are pointed out:

"The execution of the different orders made by the committee is intrusted to the resident medical superintendent, a physician, and the matron, who are man and wife. When the peculiar circumstances of these establishments are taken into consideration, it seems a most desirable arrangement that the direction of them should be in the hands of married persons; it gives a home feeling to the parties, and prevents the little petty quarrelling and jealousies which are found continually to exist where single persons preside, and each has a separate interest to attend to. These officers have the entire management, under the control of the committee, of the details of the institution, and give the orders for such things as they have received instructions for from the committee, and for any works of necessity that may arise. The medical and moral treatment of all the patients is under the immediate direction of the resident

physician and matron; the resident physician also acts as the treasurer to the institution. The resident physician and matron are assisted by the house-surgeon and his wife; the former of whom, immediately after the patients have breakfasted, goes round the wards on both sides of the house, and carefully examines into the state and general health and comfort of the patients, and makes a report of any new case of sickness to the physician, whom he subsequently accompanies in his rounds: he also makes up the medicines, and keeps the medical case-book. In the afternoon this officer again regularly goes round the wards; in fact, his duty consists in the exercising a constant watchfulness over the servants, particularly over the male keepers, and in the becoming intimately acquainted with the character and circumstances of each individual patient, so as to contrive, with the physician and matron, that not an opportunity may be lost of taking advantage of any favorable turn in the disease. This duty is unceasing; it embraces occasional visits, at uncertain times, to the different male wards, before the servants rise in the morning, to see that the keepers do not permit the patients to get up before they themselves are dressed and ready to attend them, and similar visits after the patients are put to bed at night; to take care that the patients' clothes are taken out of their bedrooms; and that the epileptic patients are so secured as to be unable to turn upon the face, without which precaution they are liable to die from suffocation, in case of a fit coming on. It of course also embraces an attendance, in conjunction with the physician, on any special cases of sickness, as often as may be needed. This officer and the clerk, in each week, inspect the stock of linen, bedding, clothes, &c., in each of the male wards; and, comparing it with the inventory, report any deficiency to the matron." (p. 290.)

The patients' library, for which they are principally indebted to the kindness of Mr. Gurney, is in charge of the female storekeeper. The books consist of biographical works, voyages, travels, and anecdotes; which, *with tracts*, are distributed every Saturday for the use of the ensuing week. The Penny and the Saturday Magazines are also taken. With the help of these, the patients are occupied and kept tolerably quiet on Sunday : a day on which, from the absence, it is said, of their customary occupations, but, more truly, from the absence of their keepers, they are generally more troublesome than usual.

In the old times of lunatic asylums, the keepers used to lock up or chain down their patients on Saturday night, that they might keep holiday from Saturday to Monday. The poor maniac was scrubbed with less ceremony than a horse on the Sabbath-day morning, by an under helper, and fed with less courtesy than a dog; he lay in his straw, on that hallowed day, dirty and distressed and disregarded; whilst his keeper was junketing at Lamb's Conduit-house, the Three Hats, or the Apollo Gardens. The grosser parts of this sketch are now effaced; but, if we mistake not, Sunday is yet the day in all the week most marked by neglect of lunatic-patients. Until very recently one half of the nurses and keepers at Hanwell were absent every Sunday, and some of them from Saturday to Monday.

In making the preceding observations on the constitution of Hanwell, as laid down by Sir W. Ellis, nothing is farther from our intention than to arraign his general good sense or his humanity. Of the latter his work affords many proofs, one of which is alluded to in our former Number, when speaking of the desirableness of making some provision for lunatics when cured; and we have much pleasure in referring to another, in his observations on the bazaar in the Hanwell institution; the various kinds of work called for by which have most beneficially

employed several patients who could not be made industrious by any other means. The faults on which we have commented are incidental to the great ignorance of the public, relative to the prospect of cure afforded in mental disorders by careful medical and moral superintendence, and a consequent depreciation of the value of services, for which the regulations usually in force make little provision.

That the duties of a superintendent are far higher than those apparently chiefly contemplated by Sir William Ellis, is well known to many who have visited various institutions of our own country; and, still more, of the continent; whilst the elaborate system pursued in some of the latter, (we may particularly instance those of Siegburg and Winnenden,) are known to require such a devotedness of the time and capacity of the physician, as would be quite impossible under the plan introduced by Sir William Ellis into so large an asylum as that of Hanwell. At present, we believe, few vestiges of that plan remain. There is now a steward, upon whom devolve many of the duties formerly attached to the office of the physician; the superintendence of the buildings, the farm, the gardens, the live and dead stock, and the provisions: and the physician is consequently enabled to attend to his proper duties. That part, however, of the old system which was not open to objection has been carefully retained, and, with the exception of some kinds of labour which were found not to pay the expenses incurred by them, the work at Hanwell has undergone no suspension. It is, however, considered more as a remedial than a profitable application of the patients; and the proper office of the keepers and nurses, which had almost merged into that of mere workpeople, has been carefully restored. The first object of the governors of the asylum is the care and cure of the patients; and to this, labour and everything else is made subordinate. With this limitation, the principle of employing the patients in various kinds of work cannot be too highly spoken of. Sir William and Lady Ellis appear to have acted upon it with an energy and an effect seldom equalled. From twelve to forty patients were constantly occupied on the farm, and as many or more in gardening. Half a dozen of the female patients assisted to milk the cows; and eight were employed, under the direction of one sane person, in brewing and baking for the whole establishment of 660 individuals. From sixteen to twenty patients assisted the laundry-maid to wash. One of the two keepers of each ward was in their time a mechanic; and he daily selected, or had intrusted to him, a portion of the patients, whose employment he superintended: the other keeper attended, after a fashion, to the wards, and employed the patients in picking coir, twine-spinning, and in-door employment. The arrangements in the women's wards were of the same kind.

Our observations have exceeded our proposed limits; but it is difficult to be brief on a subject in which the minutest particular becomes of great practical influence and consideration. The claims of the insane on their happier fellow-creatures are many and sacred. In the ruined bodies and minds of many of them we do but behold the last results of all the moral and political evils which cling to ancient social structures, however various in their plan; adorning them, perhaps, in the eye of

the unreflecting spectator, and hiding the rents of a decaying architecture; but ever heaping fresh sources of destruction around the foundations, and assisting time to crumble the whole edifice into dust. The varied forms of misery, of privation and neglect, of abandonment physical and moral, which, in various combinations, make up the great unceasing contention for existence revealed to the physician in all his intercourse with all classes below the richest, concentrate their baleful rays upon the madhouse. Poverty there has done its worst; and man is reduced to a state from which, too often, there is no relief but death. From what causes proceeding it is not our task to investigate, but certain it is that if we go from house to house, except in a small section of society, we do but find disclosed the infinite forms of embarrassment and anxious pain. Madness is the climax. Always let it be impressed upon the minds of those who take the charge of lunatics, that they are called upon to exert their faculties, and to regulate their own actions, for the benefit of the most distressed of mankind, whose wretchedness, deep as it is, may yet be aggravated by unkindness; and that, except them and one "who turneth Him unto the prayer of the poor destitute, and despiseth not their desire," a lunatic has no friend. Relatives cast them off; society banishes, and all fear them. Their very amendment is looked on with suspicion; and for them the eye of affection beams no more. To the guardians of the asylums, whither they are driven for protection or cure, is given the high and singular ministry of securing the comfort and happiness of poor and helpless creatures, forcibly repelled from the more vigorous herd. They may still avert suffering from them: they may surround them with many blessings which they have a capability to enjoy, even in their bereaved intellectual state.

Those who are unacquainted with the history of families over which the plague of insanity has fallen, know but a part of the miseries incidental to human beings. If they could behold the accumulated trials of wives, daughters, and mothers, under such circumstances; the immediate privations, the alarm and agitation, the sacrifices long endured for those who repay such devotion with frantic abuse, with an ingratitude the result of disease, but which scarcely the less wounds and grieves the hearts of those who still love the doomed and falling creature, whose sense and whose character are alike undergoing ruin; they would be convinced that there is no sorrow like their sorrow. Sudden accidents fall upon the working-man, too, in which a fall, a blow, a wound, immediately injures the brain, and incapacitates the honest labourer, yet in the prime of life, from all future profitable work: and who can see and talk to this victim of calamity, in the quiet moments and intervals of his malady, when his anxious thoughts turn with honest faith to his home, to his wife, to his children, without commiserating that ruined humble household; not forgotten, but no longer supported and defended by the unfortunate husband and father, who must linger out his life in an asylum.

It may be thought that madness, like death, knocks alike at the palace gate and at the labourer's hovel; but ever more heavily and darkly does the misery which it flings over devoted households fall on the poor. In the families of the rich, individual eccentricities of temper and manner, and wild and wasteful extravagance, are borne with comparative ease. They are sources of vexation more than of suffer-

ing. Abundant comforts and sources of diversion are ever at hand; and however much the erratic relative may be a plague to his connexions for a time, complying doctors are not long wanting, who, called upon by gay impatient wives, or proud and very much shocked relations near of kin, consign the delirious brother or phrenetic husband to an asylum, far away from every acquaintance of his prosperous estate. When he dies, his wealth is divided with a dignified satisfaction; elegant mourning gives assured testimony of a respectable degree of grief, and devout thanksgivings are breathed over superb prayer-books for the comfortable release. Among the poor, the progress of such a case, and its long dragging consequences, are more harassing. Eccentric conduct brings various kinds of punishment on the incipient lunatic himself; blows, and impositions, and imprisonments, and contumely. By degrees his whole family, his hapless children, his affectionate brothers and sisters, or a poor devoted wife, are reduced by successive sacrifices to the lowest condition of poverty. The cottage is disfurnished. Food becomes scanty. With slow-moving charitable rescue, the parish-officers relieve them of their heaviest burden. Yet when, at length, the poor creature, who brought all this suffering upon them, dies in the pauper lunatic asylum, these faithful relatives may be seen, in faded funeral garments, and at an expenditure for the day of a sum, small indeed, but to them considerable, their day's earnings, and sometimes by the omission of their day's food, following to the unregarded grave the remains of the unhappy wretch whose release is truly a mercy; shedding honest tears, for which, as in more exalted stations, there is not the consolation of property inherited, or the consciousness of a graceful sorrow.

In every point of view, then, it is difficult to conceive a task more important, humanly speaking, than that undertaken by the superintendents of lunatic asylums; to whom are committed for repair the spoiled minds of society, with the hope that they can restore the delicate balance, the loss of which has disabled the man for all his duties in society, and made him useless or dangerous; and has cast out the woman from the circle of affections and of decency, pitiable, and distracted, and degraded. Their accomplishments, derived from nature and cultivation, should be proportionate to the undertaking. Their character, whatever it is, will diffuse itself over the household, and exercise a secret influence on every keeper and nurse, and on every patient in whom any trace of sensibility remains. The responsibility hence resulting is enormous. Good sense and good temper, so essential in every social office, are doubly essential here. To which should be added, the most exact order, enlarged views, so surely based as to be unswayed by opposition or difficulty; composure of temper in the midst of agitation, and not to be disturbed by the most violent and undeserved abuse, or by the not unpardonable foibles of agents unprepared by education and habits to fulfil at all times all their intentions. Their duties are not only important but incessant. It is not only that every moment may bring an accident, and every advancing step may be the herald of agitation; but the minds of which they have the charge are never stationary, but advancing or retrograding every hour. To examine the new patients, to watch their progress, to detect the first glimmering light of convalescence, and watch it into day; to remedy the body when its disordered functions manifestly prevent reco-

very, and to rouse the mind when it lies under a fancied load, and retains a power and wants the will to be exerted : these are a part of their duties, and they comprehend particulars too numerous for any written detail; too constant, too anxious, too serious for any human witness to estimate. They are also often the medium of reunion between the patients and their friends and families; and even when the convalescent is discharged, can scarcely fail to look after their steps for a time with kind solicitude. Nothing can bear the officers safely and well through all these exertions but a sense of duty. Fame will scarcely reward them ; and their department of exertion implies an abandonment of most of the worldly advantages which stimulate ambition beyond the boundaries of the asylum.

But, even without these reflections, the superintendents of a lunatic asylum find their hearts appealed to on every side. To them an hundred helpless hands are held out; and many a faltering, palsied tongue addresses its petitions. Under their management hope revives, even in the cell, and on the bed of straw, and smiles relight the faces of those before forlorn and dead to every joy. By their care the frantic outrage of the maniac is abated, and the unspeakable wretchedness of the melancholic is diminished; by their timely and soothing words the awful dreams of the visionary, who "sees horrid night the child of hell," are oftentimes charmed away. Every act of their benevolence produces its palpable good. Every word, every look of kindness, finds its way to some pained heart, and does its blessed office. The great end, too, of all their exertions,—the restoration of mental power, is infinitely noble. The physician feels that to restore health of body is an elevated art, the value of which those best can appreciate who have ever wanted the blessing. The art of the mental physician is to restore alacrity of attention, readiness of memory, warmth of imagination, accuracy of judgment, and the power to will and to do; the loss of all which is the most grievous part of sickness.

If, then, the abnegation of self in those who take the charge of lunatics is expected to be almost complete, it is that they may be intrusted with the administering of aid to minds more imperfect than their own, and wholly secluded from the cheerful ways of reasonable life. In a world full of common duties, they are separated and devoted to some which may be said, without exaggeration, to be among the highest which a sentient and intellectual being can be privileged to fulfil.

To perform these duties efficiently, they must literally live with lunatics. Constant association with the wild minds that diversify the wards of an asylum can alone give a mastery over them, in every changeful mood. There is nothing to despise in such an occupation. To create the mind has been pronounced a work worthy of the Divinity, and to describe it the highest reach of philosophy: it is no mean task, therefore, to disencumber it of its physical oppressions, to recal its wanderings, to dispel its phantoms, and restore so high a work to unembarrassed exercise.—Perhaps a still more important task yet remains to teach mankind the *causes* of these most fearful visitations ; that they may also learn the means of avoiding afflictions difficult to cure, and of which the tendency is to accumulate in every successive generation.

ART. VII.

1. *On the Origin of the Moral Qualities and Intellectual Faculties of Man, and the Conditions of their Manifestation.* By FRANÇOIS JOSEPH GALL, M.D. Translated from the French, by WINSLOW LEWIS, Jun., M.D.—*Boston, U.S.*, 1835. Six vols. 12mo.

2. *Phrenology, or the Doctrine of the Mental Phenomena.* By J. G. SPURZHEIM, M.D. *Fourth American Edition.—Boston, U.S.*, 1835. Two vols. 8vo.

3. *Traité de Phrénologie Humaine et Comparée; accompagné d'un magnifique Atlas, in folio, de* 120 *Planches, contenant plus de* 600 *sujets d'Anatomie Humaine et Comparée.* Par J. VIMONT, M.D., &c. —*Paris et Londres*, 1835. Two vols. 4to.

4. *Cours de Phrénologie.* Par F. J. V. BROUSSAIS, Membre de l'Institut, &c.—*Paris et Londres*, 1836. 8vo.

Lectures on Phrenology. By F. J. V. BROUSSAIS, M.D., &c.—*Paris*, 1836.

5. *A System of Phrenology.* By GEORGE COMBE. Fourth Edition.—*Edinburgh*, 1837. Two vols. 8vo.

6. *Elements of Phrenology.* By GEORGE COMBE. Fourth Edition.—*Edinburgh*, 1837. 12mo.

7. *The Constitution of Man considered in Relation to External Objects.* By GEORGE COMBE.—*Edinburgh*, 1837 and 1839. 12mo, Seventh Edition; and Royal 8vo, Seventh Impression.

8. *On the Functions of the Cerebellum.* By Drs. GALL, VIMONT, and BROUSSAIS. Translated from the French by GEORGE COMBE. *Also Answers to the Objections urged against Phrenology by Drs. Roget, Rudolphi, Prichard, and Tiedemann.* By GEORGE COMBE and ANDREW COMBE, M.D.—*Edinburgh*, 1838. 8vo.

9. *The Phrenological Journal.* Nos. I.—LXI.—*Edinburgh and London*, 1823-39. 8vo.

10. *Selections from the Phrenological Journal; comprising Forty Articles in the First Five Volumes, chiefly by George Combe, James Simpson, and Dr. Andrew Combe.* Edited by ROBERT COX. —*Edinburgh*, 1836. 12mo.

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12. *An Introduction to Phrenology.* By ROBERT MACNISH, LL.D. *Second Edition.—Glasgow*, 1837. 12mo.

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22. *On the Physiology of the Brain as the Organ of the Mind.* (*Transactions of the Provincial Medical Association.* Vol. vii.) By CHARLES COWAN, M.D.—*Worcester*, 1839. 8vo.

23. *Medical Notes and Reflections.* By HENRY HOLLAND, M.D. (Chap. xxx. on Phrenology.)—*London*, 1839. 8vo.

THE time seems to us to have now arrived when a careful and conscientious examination of the truth and merits of phrenology has become imperative on every intelligent member of the profession, and when its claims to attention can no longer be safely neglected, even by those who are more concerned about their personal reputation than about the advancement of science and the improvement of mankind. If phrenology be true, its importance to medicine and to philosophy can scarcely be overrated, and no one can be more usefully employed than in advocating its cause; whereas, if it be false, and the observations on which it professes to rest be really incorrect, a great service would be rendered to medicine by at once demonstrating their hollowness, and directing the able and zealous exertions of its misled followers into a

safer and more profitable channel. Acting on this conviction, we have ourselves lately bestowed much attention on the subject; and we feel that no apology can be required for now laying the results before our readers.

In contemplating the past history of phrenology, the difference of tone and manner in which it is now spoken of cannot fail to be remarked. Five and twenty years ago, when the late Dr. Gordon made his unprovoked and ungenerous attack in the Edinburgh Review on "the man of skulls," whom he imagined to have been slain in the same Review twelve years before by the abler hand of the late Dr. Thomas Brown,—the public, then profoundly ignorant of the merits of the question, went so heartily along with him in the torrent of invective, abuse, and ridicule, in which he so inconsiderately indulged, that for years after, the subject was never alluded to without a smile of contempt or a laugh of derision, and the gentlest fate which was assigned for it was that of speedy and eternal oblivion.

How different the state of things is now, few even of its most inveterate opponents require to be told. For years phrenology has ceased to be the subject of drawing-room gossip, or the favorite topic of the ridicule of the shallow. In mixed society it is as little heard of as any other branch of physiological or scientific enquiry, which the rules of good breeding naturally warn us to reserve for a more fitting occasion; and from this circumstance many imagine that it has wholly disappeared. But when we examine a little more closely what is passing around us, the signs of its vitality and growth are found so numerous and palpable as to shadow forth rather a long, and vigorous, and useful existence, than the speedy extinction with which it has been threatened. In proof of this, we would refer, among other things, to the numerous works which have lately appeared, not in this country only, but in America and on the continent, and the titles of some of which are prefixed to the present article, not for review, for that were impossible, but as indications of what is going on. We would refer, also, to the variety of quarters in which phrenology is already received, and more or less acted upon, as established truth. We confess, indeed, that, although far from inattentive to its later progress, we were not prepared for the numerous evidences of its extended diffusion which forced themselves upon our notice, without enquiry, in a late tour through part of England, Scotland, and the north of France, Paris included. In asylums, schools, and factories, we found it recognized and acted upon, where ten years before not a trace of its existence was to be heard of. Not only, however, are works on phrenology rapidly multiplying in number, but they are improving in character; and in accuracy of observation, sobriety of inference, and vigour of thinking, a few of them may bear a comparison with any physiological or philosophical works which have lately appeared. That these qualities have not been without their natural effect in exciting a widely diffused interest in the public mind, is evident from the extraordinary and steady sale which several of the phrenological works, the best, we believe, of their class, have met with, in the face of the active and influential hostility of the leading journals of the day, led on by Lord Jeffrey himself, in the Edinburgh Review, and also by the Quarterly. If this demand had lasted only for

a year or two, it might have been plausibly enough ascribed to fashion and a love of novelty; but when it has extended, as in the instance of Mr. Combe's books, over a period of twenty years, it is difficult to account for it, except on the supposition of their possessing a real and abiding interest, derived either from the inherent nature of the subject, or from the manner in which it is treated. Not to mention the wide diffusion of the works of the founder of phrenology, and his colleague, Spurzheim, we have now before us the *sixty-first* quarterly number of the Phrenological Journal, which has been carried on for upwards of sixteen years, and, as we are told by the editor, is yearly increasing in circulation. We have also before us an advertisement of the last edition of Combe's "Constitution of Man considered in Relation to external Objects," in which it is mentioned that that work, being an application of phrenology to human improvement, continues in constant demand, after a sale of *forty-five thousand copies* in Great Britain and Ireland alone, besides large editions in America, and translations into French and German. The "System of Phrenology" of the same author, which contains the best exposition of the doctrine, its evidences and applications, although selling at a guinea, and therefore not likely to be bought without due consideration, has already gone through four editions, and, as we have learned, still continues in increasing demand, to the extent of 600 copies a year. In like manner, the "Introduction to Phrenology," by the late Dr. Macnish, have sold, as appears from the advertisement, to the very large extent of 5000 or 6000 copies within three years, notwithstanding the increasing number of competitors in the market. We might mention many other evidences, of a similar nature, to prove the progress which phrenology is making in public opinion; but for these we must refer the reader to the curious volume of Mr. Hewett Watson, on "The Statistics of Phrenology," in which an account is given of the various works published, and societies existing, in this country, and in which the reader will find much useful information, of an authentic kind, relating to the past history and present state of phrenology.

As further evidence, of a very unequivocal kind, we may refer to the numerous courses of lectures given on the subject within the last five years in most of our larger towns, and to the intelligent audiences by which they were attended. Even the frequent display of phrenological busts in the windows of shops is a sign not without meaning to reflecting minds. But perhaps more than all, the rapid diffusion of phrenological ideas under the cover of ordinary language, and without any reference to their true source, is a proof not only that the new philosophy is making progress, but that it is found to be of direct utility in questions of nervous disorder, insanity, education, morals, and crime. We are acquainted with medical and educational works which have gained no small repute, from the copious but unacknowledged use they have made of the doctrines of phrenology, and the reputation of which depends chiefly on their borrowed views. We have sometimes, indeed, been tempted to smile at the ready acceptance which strictly phrenological ideas have met with when thus stolen and offered at second-hand, only a little altered in dress to prevent their paternity being traced. But much as we rejoice in the diffusion of useful truth, we cannot refrain from condemning this plan of acquiring a temporary popularity at the expense of science; and

we are glad that the risk of detection will soon become so great as to deter most men from such unscrupulous conduct. It may seem at first view a light matter thus to put forth a truth in disguise; but in reality, its forced separation from the principle which alone renders its application safe and advantageous, deprives it of much of its practical value; and it is for this reason, as well as for its dishonesty, that we object to the practice.

If our space permitted, we might further refer to the account given in the last number of the Phrenological Journal of Mr. Combe's progress in the United States, and to the works of Vimont, Broussais, Ferrarese, and other continental authors, to show that, abroad as well as at home, phrenology is exciting the serious attention of men of science. But we must content ourselves with the simple statement that such is the fact; and that, among the more recent of the French medical works, the principles of phrenology are either expressly or tacitly assumed, as if no doubt had ever been entertained regarding them. Many hesitate, and justly, about the details, but we do not go too far in affirming that a conviction of the truth of the leading principles of the new physiology of the brain is fast diffusing itself over the continent.

With these facts before us, we need scarcely add that our past silence has not arisen either from participating in the contempt with which phrenology was formerly treated, or from having been unobservant of its more recent progress. From the first we saw that, whether true or false, the subject was one of great extent and serious import; and we delayed forming or expressing any opinion till we should have sufficient time and opportunity to verify its principles and scrutinize its details. Having now done so, sufficiently to qualify ourselves for giving an opinion, we should shrink from our duty, both to our readers and to science, were we to hesitate longer in avowing our conviction that phrenology embodies many facts and views of great general interest, and direct practical utility to the physician, the philosopher, and the philanthropist; and that as such, it has established a claim to a more careful, serious, and impartial examination on the part of the profession than it has ever yet received. We do not by this mean to affirm that all the facts and doctrines taught by the phrenologists are accurate and true; so far from it, we have satisfied ourselves that many have been admitted without a sufficiently scrupulous examination; and that not seldom, the conclusions deduced from them have been pushed beyond the limits of strictly logical inference. We are consequently not inclined to adopt either of them without due verification. But it would be the height of injustice were we on that account to reject the whole as unfounded, and to maintain that they cannot possibly be true, merely because they are in contradiction to our own preconceived opinions; and yet, to the most unphilosophical and illogical mode of proceeding we have condemned, may be traced almost all the opposition which Gall's discovery has met with.

If the functions of the brain had been already ascertained by some method of enquiry of a more satisfactory nature than that resorted to by Dr. Gall, we might have argued, with some fairness, that if his observations were inconsistent with those already obtained, they could not possibly be true. But when it is notorious that all other methods of investigation *have failed* to unfold the mystery of the cerebral functions,

it is as obvious as the noonday sun, that no information which we may possess can enable us to decide *a priori*, and *without any examination of the evidence*, that his mode of enquiry is fallacious and its results untrue. To entitle the judgment of any one to the least weight, either for or against the reality of the discovery, it must be based upon a careful examination of the facts and evidence. If a man propounds as a new discovery that the function of the liver is to secrete milk, we are logically entitled to disregard his assertion, because we are already in possession of demonstrative evidence that the function of the liver is to secrete bile. But it is very different with the case of the brain. When Dr. Gall affirms, that by a new mode of enquiry, easy of practice, he has ascertained that the anterior lobes of the brain serve for the manifestation of intellect, the posterior lobes for that of the animal passions, and the coronal region for that of the moral feelings, we have no right whatever, either in sense or in philosophy, to say, "No! this is a mistake." So long as we do not possess a shadow of information at variance with his assertion, it would be to assume in profound ignorance the privilege of Omniscience to say, that such a thing "*cannot be*." With regard to the brain, we are in precisely the same situation as we would be with regard to the spleen, if some physiologist were to discover that its use was to secrete a particular kind of digestive fluid, and were to describe how he made the discovery, and how it might be verified. If the greatest philosopher that ever lived were thereupon to deny, *without examination of the evidence*, that the spleen served for any such purpose, who would attach any weight to his objection, or who would care one straw for the adverse opinion of any man who had not thought it worth his while *to test the fact*, before deciding upon its truth? In like manner, when Gall professes to have found out the functions of the brain, and explains how he made the discovery and how it may be verified, it would be equally childish and futile to satisfy ourselves with the simple denial without direct examination of the fact, that the different organs above specified serve the purposes pointed out by him. Either we must meet the question of fact by a personal and extensive appeal to nature, or we ought to avow that we are not prepared to speak definitely as to the truth of the doctrine.

We are aware that many talk of phrenology as a mere theory, invented by the fertile imagination of an enthusiast, and under this impression think they treat it with all due respect, when they give it half an hour's consideration before they express an opinion of its merits. We confess that we ourselves once belonged to this rather numerous class of persons, and that we extracted much amusement from the pages of Gall and Spurzheim, by a playful travestie of some of the curious anecdotes by which they occasionally illustrate their positions; and which, considered apart from the context, have often a somewhat ludicrous aspect. But when at length we came into contact with Spurzheim himself, and remarked, instead of the wild enthusiasm of a visionary, the truthful earnestness, the calm and forcible appeals to fact and reason, and the occasionally almost solemn feeling of the importance of his mission, with which he advocated his cause, we felt that the subject was of too grave a nature to be either hastily admitted or slightingly rejected, and resolved to try his positions by the strict test

of observation before finally deciding upon their truth. The result was, as we have already said, not the blind adoption of the whole phrenological doctrines, but a growing and conscientious conviction of the soundness of the great principles on which they are based, and of the practical value of many of their details. But although we see strong grounds for believing that an imperishable foundation has been laid, the edifice itself is still far from being complete, and many years and much labour will be required to bring it to that perfection of which even its present outline shows it to be susceptible, and which, in their short-sightedness, some of its admirers imagine it already to have attained.

Gall's discovery, if such it shall turn out to be, of the functions of the brain, was no premeditated invention, but, like that of the principle of gravitation by Sir Isaac Newton, the result of accident. When he first observed at school that the boys who gained places from him by the facility with which they learnt and remembered words and recitations, while they were much inferior to himself in general talent, were all remarkable for a peculiar prominence of the eye, like that known by the name of bull's eye, he merely *remarked a fact ;* and when he was removed to another school, and subsequently to college, his attention was arrested by the fact that there also the talent of learning easily by heart was accompanied by the prominent bull's eye. At that time he knew nothing of the cause of the prominence, nothing of the position, structure, or functions of the brain, and nothing of the philosophy of mind. He attempted no explanation, and had consequently no theory to support. He satisfied himself with observing that the fact was so.

For a long time Gall remained at this point; but, as he advanced in years and reflection, it at last occurred to him *that if one marked quality of mind was thus indicated by a peculiarity of conformation, the same might be the case with others.* This was the prelude to all his subsequent examinations. He began to remark with care the different forms of head and differences of disposition and talent by which his companions were respectively distinguished. To facilitate his researches and ensure greater accuracy of observation, he now took casts in plaster of every remarkable head or forehead which presented itself; and by comparing the peculiarities of each with what he knew of the mental qualities of their originals, he gradually became possessed of a very interesting series of observations throwing additional light upon the facts with which he started. Occasionally, when he thought he had succeeded in tracing a connexion between some marked feature of mind and peculiar form of head, an instance would present itself of the same mental peculiarity with a different form of head, and dash to the ground the conclusion which seemed approaching to certainty. Not discouraged by these results, he neither hesitated to give up the opinion which was thus disproved by facts, nor found his faith in the uniformity of nature at all shaken. He submitted to the correction, but continued his observations, and rarely failed by perseverance to discover the cause of his error and to add to the stock of positive truths. The ultimate result of his labours was the gradual development of the physiological and psychological doctrines now known under the name of phrenology.

Phrenology then may be considered in two distinct lights: first, as an exposition of the functions of the component parts of the brain; and

secondly, as a theory of the philosophy of mind. Considered in the former light, the evidences of its truth must be sought for in oft-repeated observation of the concomitance and connexion of certain functions with certain portions of the brain; whereas, considered purely as a system of mental philosophy, its truth may be judged of, like that of other theories of mind, by the facility and consistency with which it explains the phenomena and admits of practical applications to the purposes of life. The former kind of evidence, viz. that of *direct observation*, is by far the most conclusive, and, as coming within the strict province of physiology, is that to which medical men ought chiefly or first to direct their attention. But the evidence arising from complete adaptation to the phenomena is also entitled to great weight, and may indeed suffice for those who study it chiefly as a branch of philosophy. The best way of all, however, is to investigate the subject from both points of view, and embrace both kinds of evidence; but on the present occasion we must confine ourselves almost exclusively to its consideration as a branch of physiology.

Taken in its widest sense, phrenology professes to be *a theory of the philosophy of mind, founded on the observation and discovery of the functions of the brain, in so far as that organ is concerned in the mental operations.* Its fundamental principles are the following:

First. That the brain is the organ of the mind, and is concerned in every mental operation, whether of emotion or of intellect.

Second. That the brain does not act as a unit, but consists of a plurality of organs, each serving for the manifestation of an individual faculty of the mind.

Third. That the energy of function or power of manifestation is proportioned, *cæteris paribus*, to the size of the organ; or, in other words, that a large organ will, *all other conditions being equal*, enjoy a power of action proportioned to its size, and consequently manifest the corresponding faculty with greater energy than if it were small.

And lastly. That by observing carefully a sufficient number of cases in which the same part of the brain predominates in size over all the other parts, and ascertaining what particular quality of mind is exclusively in excess in the same individuals, we obtain a direct clue to the discovery of the functions of all the organs of the brain, and require only that the observations shall be so carefully made and so extensively repeated as to obviate every chance of error before adopting the inferences as established. Let us now see how far these principles are in accordance with nature and with previously existing knowledge.

That the brain is the material organ, without the intervention of which the mind cannot operate during life, is so all but universally admitted, that we shall adduce no facts to prove it. It is true that some over-scrupulous men, like Lord Jeffrey and Dr. Abercrombie, still doubt whether the mind acts through the medium of material organs, except in its communications with the external world; but as the proposition is regarded by an overwhelming majority of physiologists as demonstrated, we shall, on the present occasion, assume it to be true.

Nearly the same assumption might be made with safety as to the brain consisting of a plurality of parts, each performing a distinct function. But the truth of this principle is put beyond a doubt by a mass of evi-

dence which we cannot stop to detail, and is further confirmed by the successive additions which the brain receives as animals rise in the scale of intelligence, and by the successive development of its different parts, as the human being advances from the fœtal to the mature state, and from a state of unconsciousness to one of sensation, emotion, thought, and action. During this transition, the different parts of the brain are developed, not simultaneously, as a unit would be, but successively and irregularly. In one individual, eminent for talent, the anterior lobe is early and largely developed, while in another, whose intellect is purely idiotic, it remains small and contracted. In like manner, partial insanity, and injuries of the brain attended with a partial affection of the mental powers, equally afford a presumption of a plurality of cerebral organs. If necessary, it would be easy to multiply such indications and proofs; but as the advocates of the unity of the brain are few and far between, and their views are entirely without influence on the thinking part of mankind, we consider it needless to occupy more time and space in proving what is so rarely and feebly denied.

The *third* principle, and that which it is of most consequence to explain and demonstrate, is the proposition that *organic size is, cæteris paribus, a measure of functional power.* The first two principles are common to phrenology and to physiology in general; but the third, in its broad and specific form, is peculiar to and lies at the very foundation of phrenology, and will therefore require a more detailed and careful examination. If it be false, phrenology must crumble to dust like the dry leaves of autumn driven along by the winter's blast. If it be true, those who oppose phrenology on the assumption of its falsity must themselves fall, and like decaying leaves around the living parent stem, even serve to nourish and support that which they attempt to destroy. To the examination of this point we shall therefore, without scruple, devote considerable space.

The form in which the above principle is generally expressed by phrenologists is, that *size of brain is, cæteris paribus, a measure of mental power.* Inattention to the simple meaning of this proposition has been the chief cause of the opposition it has encountered from scientific as well as unreflecting men. Notwithstanding all that has been done by phrenologists to enforce attention to the important condition of "other circumstances being equal," almost all the opponents, from the Edinburgh Reviewer down to Dr. Holland—the latest who has published on the subject—continue to utterly disregard it, and speak of the proposition as maintaining that *size alone* is the measure of functional power; or, as Dr. Holland chooses to state it, that "*the gross condition of quantity represents the intensity of quality.*" Having set up this phantom of their own imaginations, like a pyramid on its apex, many of the anti-phrenologists proceed with heavy blows and an approving conscience to knock the support from under it; and when it topples over in obedience to their efforts, they turn round in triumph, and claim the merit of having upset *phrenology.* We have seen this feat performed again and again in the presence of phrenologists. On such occasions their simple answer was, "You have upset a phantom of your own creation, but you have left the phrenological pyramid, resting on its basis, untouched and undamaged;" and such is in reality the case.

As it is in general far more easy to make merry with fiction than with truth, it required no great effort of wit in Lord Jeffrey to divert his readers, by referring to grandmamma Wolf, in the fairy tale, as a high physiological authority on the side of the phrenologists, when she tells little Red Riding Hood that she has large ears to hear her the better, large eyes to see her the clearer, and a large mouth to gobble her up with the greater facility. But his mirth did not alter the substantial fact established by the researches of comparative anatomists, that where great nervous sensibility is required, whether for hearing or sight, a proportionally large nerve is an invariable accompaniment, whatever the shape or appearance of the organ on which it is ramified. Neither did it alter the fact that the venerable lady's large external ear was really capable of receiving a larger number of atmospherical pulses, and her large eye a greater number of the rays of light, than a smaller ear or eye would have been. His joke nevertheless was a good joke. It possessed the rare merit of diverting, at the same moment, not only himself and those whom he misled, but also those against whom it was directed. The only difference was, that he laughed at what he supposed the absurdity of his opponents, while they were merry at the absurdity of the egregious blunder into which he had fallen, and from perceiving that, in point of both fact and argument, the venerable grandmamma had the great reviewer entirely at her mercy.

If the phrenologists are to be judged by their own statements and acts, and not by those falsely ascribed to them, we should say that, so far from having adopted the proposition which Dr. Holland refutes, they even deserve credit for adding to the evidence formerly existing, that "gross quantity" or size alone is NOT a measure of the functional power of an organ. We have taken some trouble to enquire, and have never met with one phrenologist who did not utterly scout the notion of organic size being THE ONLY condition of functional energy; and who was not prepared with *proofs* by the dozen of the absurdity of such a proposition. Dr. Holland says, "this *relation of mere bulk of substance to the perfection or intensity of a faculty is, primâ facie, very improbable.*" To be sure it is; but what surprises us is, that a man of Dr. Holland's good sense should have had any doubts about the matter, when he might have satisfied himself of the fact by half an hour's observation; or, if he preferred the authority of others, by consulting any good phrenological treatise in his library. Yet, strangely enough, while he stickles about the insufficiency of the evidence in support of phrenology, he does not hesitate to admit opinions unfavorable to it upon no evidence at all; and in this particular instance really argues against one of its plainest and most easily demonstrable principles, merely because he has not taken the trouble to understand its meaning.

For demonstrative evidence of *organic size being*, cæteris paribus, *a measure of functional power* (a very different proposition from "mere bulk" bearing a constant relation to "intensity of quality"), we would refer the reader, first, to personal observation in the field of nature; and secondly, to the concurring testimony of every anatomist and physiologist who treats of the relation between structure and function. We are not aware of a single work of any reputation in which the above principle is not tacitly adopted as nearly self-evident. It pervades every corner of

comparative anatomy, and is constantly, though not ostensibly, resorted to as a guide to the discovery of function. If, in an unknown animal, the optic nerve is found to be large relatively to the other nerves of the senses, we never hesitate to infer that the power of vision will be greater in proportion than where the nerve is relatively small. In the same way, we never discover a large olfactory nerve and extended nasal apparatus, without inferring that the animal must be endowed with a powerful sense of smell. And when it is affirmed by phrenologists, that the brain forms no exception in this respect to the rest of the organization, they merely state a principle in words which is admitted universally in practice. Indeed, all the modes of discovery hitherto employed, Camper's facial angle among the rest, tacitly assume this very principle as their basis; while it has been left to Gall and his followers to direct attention to it, and demonstrate its importance, as a specific truth. In proof of this statement, it would be easy to multiply quotations from any accredited work on comparative anatomy; but one from an indisputable authority may suffice: "It appears," says Cuvier, "that *there are always certain relations between the faculties of animals and the* PROPORTIONS *of the different parts of the brain.* Thus, their intelligence appears to be always great in proportion to the development of the hemispheres and their several commissures. It appears even that *certain parts of the brain attain*, in all classes of animals, A DEVELOPMENT PROPORTIONED *to the peculiar properties* of these animals; and one may hope that, in following up these researches, we may at length acquire some notions respecting the particular uses of each part of the brain." On another occasion, when speaking of the cerebral lobes being the place "where all the sensations take a distinct form, and leave durable impressions," Cuvier adds, "l'anatomie comparée en offre une autre confirmation dans *la proportion constante du volume de ces lobes avec le degré d'intelligence des animaux*;" thus admitting the influence of *size* of the cerebral organs upon the power of manifesting the mental faculties as distinctly as Dr. Gall himself could assert it.

But, it may be asked, if the principle of size being, *cæteris paribus*, a measure of power, has been thus virtually and universally admitted by men of science, whence arise the objections advanced against it by such men as Dr. Holland, when it is *specially* brought forward by the phrenologists? The only answer that can be given is, that the full value of the principle as a means of successfully prosecuting enquiry, was unknown till demonstrated by Dr. Gall, and that consequently it had never been a subject of serious consideration among men of science as a distinct and specific proposition. Even now, however, its truth is so palpable that it is never objected to, except when confounded with the very different and erroneous proposition that size *alone* is a measure of power; and, in point of fact, Dr. Gall has been the first to explain the apparent anomalies which other physiologists met with in their researches, by drawing attention to the necessary limitation of *cæteris paribus*. And when this is kept fairly in view, it becomes nearly as impossible to deny it, as to deny that a whole is greater than a part. Both phrenologists and antiphrenologists are agreed, for example, that a large forehead *generally* indicates superior intelligence; but the faith of the former in the influence of organic size, as affecting intensity of function, is not in

the least shaken by the fact that there are *some* large foreheads unaccompanied by any intellectual superiority. Nobody, indeed, knows this fact so well as the phrenologist, because he has not only observed it, but alone has examined the cause of the difference, and found that *the other conditions of the brain are not the same*, and, consequently, that so long as cause and effect continue related as such, the results in mental power cannot possibly coincide. The large and healthy expanse of brow which distinguishes the bust of Bacon may be equalled, *in mere size*, by the unhealthy expanse of forehead in the cretin or idiot; but will any one venture to infer from this that the size of Bacon's *healthy* brain added nothing to its functional power? A single example of this kind is sufficient to demonstrate that size alone is not a measure of intensity, but it leaves absolutely untouched the phrenological proposition that size is an important condition of functional power. Great energy of mind cannot coexist with a small size of brain, because no other healthy conditions can supply the want of size. But a large brain may coexist with feebleness of mind, because from original malformation, defective constitution, or disease, its power of action may be also defective. Large muscles, in the same way, may coexist with little bodily strength in a very lymphatic or relaxed constitution, and in certain states of health; and yet it is never doubted that, *all other conditions being equal*, large muscles are more powerful than small ones. For more than this the phrenologists do not contend.

Had Dr. Holland attended to the foregoing most obvious distinction, as laid down in all the works on phrenology which we have ever seen, he would scarcely have ventured to misrepresent Gall's discovery as resting "on the presumption of the gross condition of quantity representing the intensity of quality;" and, when speaking of the small brains of idiots, and the large brains of eminent men, as affording the best proofs of the influence of size, he would have had no difficulty in explaining the *apparent* exceptions to which he alludes, and reconciling them to the general rule. Rightly interpreted, *there can be no exceptions to a law of nature;* and when we meet with cases which seem to contradict the principle of organic size being a chief condition of functional power, we can come only to one of two conclusions. Either the principle *must be fallacious* and size be wholly uninfluential *in all cases*, or it *must be real and operating in all.* In particular cases its power may be controlled or its action modified, by causes which have escaped observation; but there is no contradiction in the laws of nature, and we may rest assured that if the principle under discussion has a real operation in any case, it will exercise an influence in all, whether or not we can detect the causes by which its perceptible results are modified.

We almost feel that an apology is due to our readers for insisting so much on so obvious a truth; but the very fact that science has been retarded by its neglect and misconstruction, compels us to enforce it even at the risk of tediousness. Sometimes in conversation, after we imagined that the question was placed clearly before the mind's eye, we have been met with the triumphant assertion that our proposition was annihilated by the simple comparison of the small brain of the intelligent poodle with the large brain of the stupid ox. But are all the other conditions the same in such a case except size? No doubt the brain of an ox is a

brain as well as that of a poodle; but is there no difference in their structure, no difference in the proportions of their anterior lobes, and no difference in the number and complexity of their convolutions sufficient to exercise an influence on their functions in addition to mere size? Looking to the philosophical principle of *cæteris paribus*, it is clear that the proper way to arrive at the truth is to compare the brain of a clever with that of a stupid poodle, and of one ox with another, as nearly as possible of the same age, state of health, and constitution. If this be done, and the intelligent poodle be found to have the smaller anterior lobe, then by all means denounce the principle of size as untrue and at variance with fact. But if the reverse be the case, do not attempt to set the truth aside, by comparing two things so essentially different as to make absolute agreement impossible. If this precaution be kept in view, we venture to affirm that the more the proposition is scrutinized, the more firmly will it be found to rest on the unassailable foundation of truth.

Admitting the brain to be the organ of the mind; admitting also that the brain is not a unit but a congeries of organs, each having its appropriate and peculiar function; and lastly, admitting that the energy of every function is proportioned, *cæteris paribus*, to the size of its individual organ; it follows necessarily, as is remarked by Cuvier, that the size of any cerebral organ affords a direct clue to the discovery of its function. Let us suppose, for example, that the use of the optic nerve was unknown, but that it was invariably found to be far more largely developed than any of the other nerves of sense, in animals with powerful vision; such as the eagle, and much less so in animals which see very imperfectly, such as the mole; and that no instances were to be found in the same species, in which, *all other circumstances being equal*, powerful vision coexisted with the smaller nerve, or a larger nerve with feebler vision; would we not be justified in at length inferring that the use of the nerve was to serve for vision? In like manner, if a particular portion of the brain is invariably found to be large, in relation to the other parts of the same brain, in individuals remarkable for timidity and wariness, and relatively small in persons remarkable for rashness and the absence of fear, and no instance can be adduced in which, *cæteris paribus*, the proportion between the feeling and the organ is reversed, are we not entitled, after sufficiently extensive observation, to hold that the use of that part of the brain is to serve for the manifestation of the sentiment of cautiousness? And if this mode of investigation is applicable to one part of the brain and to one faculty of the mind, it is obviously applicable to all. The only indispensable condition of evidence of this description is that the coincidence shall be real and uniform, and not imaginary or accidental; and here is precisely the grand point of difference between the phrenologists and their opponents, and in regard to which the former have never been fairly met. But as this point is of fundamental importance in determining the truth of phrenology, it will be necessary to devote a little space to its consideration.

The phrenologists affirm that by observing concomitance of function with size of organ in an infinite variety of instances, as above explained, they have succeeded in tracing a connexion between certain faculties of the mind and certain portions of the brain. Whether there are suf-

ficient grounds for maintaining the existence of such a connexion is evidently a *question of fact*, against which *à priori* argument can be of no avail. The only way to meet the phrenologists successfully is to adduce facts at variance with their conclusions; and even Dr. Holland admits that the conclusiveness of this appeal cannot be denied, for he allows that if the facts tally with the statements of the phrenologists in a large proportion of cases, so as to make reasonable allowance for error or ambiguity, *the improbability must be laid aside, and the whole admitted as a new and wonderful truth.* "Here, then, by common admission, is a direct question of evidence, the amount and strictness of which are solely to be considered."

Dr. Prichard, and other writers on the same side, take a similar view of the subject; but the phrenologists complain, and not without reason, that the very men who are foremost in admitting the question to be one of fact alone, are the first to "turn their backs upon themselves," and attempt to solve it by argument and probabilities, which, considered *as evidences*, are worth nothing. Instead of meeting the followers of Gall by well-observed and hostile facts, Dr. Holland merely says, "Here I think it will be found that the phrenologists are yet wanting in what is needful to establish their system, notwithstanding all the observation and ingenuity which have been bestowed on its proof;" and in answer to their facts, he contents himself with assigning sundry reasons for quietly setting them aside.

"Look," he says, "at what they have in aid of their determinations, where the question concerns the relation between a certain outward form of cranium and some faculty or quality of mind, alleged to be in correspondence with it. First, the equal chance of affirmative or negative, as to each particular quality predicated. Secondly, the plea of a balance of some indications by others and opposing ones. Thirdly, the want of exact definition of many of these qualities or faculties making it difficult to arrest for error where there are so many ways of retreat. And fourthly, the incidental discovery of character by other and more ordinary methods. I well know that the candid disciples of the system will not consciously avail themselves of all these methods. Nevertheless, each one of them has, more or less, been made use of; and looking to the chances and facilities thus obtained, it may be affirmed that the number of true predictions in phrenology is less miraculous than it would be, were this number not to exist." (p. 509.)

We admit at once that all this is very plausible, and that, as a reason for exercising caution in observing and in drawing inferences, it is very useful; but *does it in any degree meet the question of fact, and prove that the alleged coincidences are unreal?* We cannot see that it does, and we are of opinion that one well-authenticated fact, opposed to those of the phrenologists, would outweigh a volume of reasoning in a matter of this kind. Dr. Holland states that phrenologists appeal to coincidences between mental power and cerebral development, but he regards the coincidences as "not sufficiently numerous," and adds that during his intercourse with Gall and Spurzheim, he had several opportunities of noticing the failure of their judgments upon these particular faculties, as well as in other cases where the doctrine ought to have indicated rightly the relation between faculty and organ. But Dr. Holland does not adduce any details of these failures from which his readers might judge for themselves, whether they were real, and if so,

whether they resulted from the outward indications being erroneous, or from a mere personal blunder in estimating them, such as may happen and does happen daily in the case of a chemist or mathematician, whose science nevertheless remains unaffected by the blunder.

We also have heard of erroneous inferences being made by phrenologists, and have taken some trouble to investigate their nature. In some, we should say in most, instances, the error has proceeded from the rash judgment of incompetent persons. In others, we have known a well-qualified phrenologist commit a mistake, either from giving an opinion hurriedly, or from speaking more decidedly than the real difficulties of the case warranted. There are instances, for example, in which a number of organs are so equally developed, and in which the corresponding mental powers are so nearly equal in energy, that it is impossible to assign a marked predominance to any of them. It is in cases of this kind that the influence of education and external circumstances is greatest, and that the quality which is most assiduously cultivated will assume prominence in the character. Take two men, for example, in whom the selfish and the devotional feelings are originally almost equally strong, and breed the one to the church, and confine him to the society of the kind and benevolent, while you place the other in a counting-room, amidst all the excitement of money-getting—the one will assuredly become, not pious and disinterested in the highest degree, but certainly *more* pious and disinterested than the other; while the phrenologist, who affirmed that they were naturally or originally on an equality in this respect, and that the two faculties were nearly equally balanced in both, would most likely be regarded by their respective acquaintances as greatly in error. Again, we have known a phrenologist hastily pronounce an organ to be moderate, which was really large, and thus give rise to an apparent contradiction. But although this may happen now and then, *it does not alter the reality ;* it leaves the organ of the same size as before, and if a more careful comparison shows it to be really large, the induction remains valid, although the manipulator committed a mistake. This, however, is carefully kept in the back ground by the opponents of phrenology, who often confound an erroneous estimate of a fact with hostility of the fact itself, and thence infer that phrenology must be in fault, when there has been merely an error on the part of the individual, for which the science ought never to be made answerable. If the observations made by the phrenologists *are* incorrect, surely there can be no great difficulty in obtaining authentic facts to prove their inaccuracy. And yet, while all thinking men on both sides agree that the question can be authoritatively settled only by a reference to fact, it is somewhat remarkable that the phrenologists alone have taken pains to observe nature and to form collections of facts, which they have further laid open to public inspection and verification in their museums; while their antagonists have neither published nor collected any opposing facts, but have contented themselves with the vague assertion that such exist, and with arguing that therefore those of the phrenologists must be untrue.

Here, we think, lies the great error of those who contend against the truth of Gall's discovery. All of them—even Dr. Roget, Dr. Prichard, and Dr. Holland—state, *in a general way*, that their experience is against

the alleged concomitance of mental faculty and cerebral organ. But instead of themselves specifying facts and giving details entitled to confidence, they complain that the observations recorded by the phrenologists are not "sufficiently numerous" or accurately made to prove their positions, and argue that hence these must be disbelieved. This mode of proceeding, when expressed in plain language, appears palpably absurd. The phrenologists state principles, and adduce "some" facts patent to everybody, which tend, *pro tanto*, to prove them. Their opponents, however, say, "No; do not believe one of them, for *we* know facts which do not tally with them, but which *we shall keep to ourselves*, and which you must believe merely on our assurance." The phrenologists have been accused of claiming a large measure of belief on the part of their followers; but their claim is backed, not only by hundreds of published cases, but by museums full of specimens, copies of the more remarkable of which are to be found in almost every large town in Britain. Whereas the anti-phrenologists make a sweeping claim on the public to disregard all these evidences, and to believe them worthless on their own mere affirmation, unsupported by facts of any description! Is it to be wondered at that opposition so directed has been wholly ineffectual in arresting the progress of phrenology or disproving its truth? We think not; and we suspect that if phrenology is to be put down at all, it must be by an opposition more in harmony with the Baconian rules of philosophizing than any hitherto attempted.

Dr. Holland, Dr. Prichard, and Dr. Roget, all have the sagacity to perceive that, however plausibly the matter may be argued on either side, the truth of phrenology must in the end be decided by an appeal to facts alone; and such being the case, we think our remaining space will be much more profitably occupied with a few remarks on the best mode of testing the phrenological facts by observation, than with comments upon any other parts of the general argument.

If it were necessary, this would be the place to show that there are no insuperable difficulties in the way, to prevent the size and configuration of the brain from being pretty accurately estimated during life, by observing the outward form of the head. In the early days of phrenology, the want of parallelism between the tables of the skull, and the existence of the frontal sinus, used to be rather favorite objections. But they are now nearly abandoned by anatomists. Some parts of the skull are always thicker than others, but the greatest difference in the thickness of the parts, which have reference to phrenology, scarcely ever exceeds one or two lines, whereas, in cases of extreme development of brain, the difference of external size often exceeds an inch; so that, even after allowing for *the utmost possible* divergence between the tables, enough will still remain to indicate the development of brain below.

The existence of the frontal sinus generally makes it difficult in mature age, and especially in males, to ascertain the size of two or three of the smaller organs situated, according to the phrenologists, behind it; but we cannot see that it is of the least weight as an objection to the truth of phrenology in the main. The sinus rarely appears at all before puberty, and consequently cannot interfere with the accuracy of observations made before that age. It is also rarely much developed in females, and therefore an ample field for observation is open to which no objection of this kind can apply. But in this, as in other cases, the scope

for controversy would be greatly narrowed, and truth be far more easily attained, if both parties were more careful to fix their attention principally upon the real objects of discussion, and not to lose sight of essentials in their keen pursuit of mere accessories, which serve only to perplex and mislead.

Admitting, in its fullest force, everything that can be said about want of parallelism between the tables of the skull, and about the existence of a frontal sinus of variable magnitude, all that we can honestly conclude is, not that the unsoundness of the phrenological principles has been established, but that a certain amount of difficulty stands in the way of their universal application. Thus, in some cases of chronic disease, the thickness of the skull increases to the extraordinary extent of an inch or upwards, and in other instances it diminishes to little more than the thickness of paper. In old age, also, the skull is sometimes of very irregular thickness, from the inner table following the surface of the diminishing brain faster than the outer. But during health and in mature age, such aberrations are never to be met with. When they do occur, however, it becomes evidently impossible to determine, *with certainty*, from the mere examination of the outward form of the head, the size and form of the contained brain; and therefore, Dr. Gall expressly *rejects, as inconclusive*, all observations made during *old age and disease*, because they necessarily involve an element of doubt. Many of such cases afford valuable *illustrations*, but can never be received as *proofs*. These must be derived exclusively from the period of life during which the essential correspondence between the external indication and the form of the brain can be relied upon.

In investigating the claims of phrenology, in short, it ought never to be forgotten, by either friend or foe, that the first and grand object ought to be *to ascertain its truth;* and that till this be done, it is needless to confuse the question by discussions referring solely to the difficulties of applying it to individual cases. The greater the facilities afforded for the verification of evidence, the sooner and more easily will phrenologists succeed in obviating all the difficulties of mere application; and if the balance of evidence shall turn out hostile, the matter will be at an end at once, and further discussion on any part of the question will become altogether superfluous and unnecessary.

How, then, are the alleged facts of the phrenologists to be most easily verified or disproved? As neither argument nor ridicule can set them aside, our only remaining, and by far the shortest, way is at once carefully to examine nature, and see whether our observations harmonize with or contradict those of the phrenologists. If they agree, let us give up prejudice and adopt them as true; and if they differ, let us at once reject them, and all the inferences deduced from them, as incorrect and untenable.

In surveying mankind, with a view to observe whether the alleged concomitance between certain qualities of mind and configurations of brain holds good, it will be apparent to every thinking enquirer, that a large proportion of society consists of what are called common-place characters, who are not distinguished by any striking mental feature of either a good or a bad kind, and who display an average amount of kindness, piety, conscientiousness, affection, pride, vanity, caution, selfishness, and temper, and also about an average amount of acuteness of

perception and reasoning power; but who exhibit neither genius nor originality, and never seek to leave the beaten path of everyday usefulness in which Providence has placed them. On minuter examination, each individual of this large class is found to be distinguished by *shades* of the general character, and to possess a little more of one quality and a little less of another than his neighbour, but still to display nothing that marks him as very distinct from the general herd. If, as the phrenologists affirm, the development of the brain corresponds with the features of the character, it will follow that the mass of mankind, in any one locality, will present brains differing little from each other, and equally allied to a common type as we have seen their characters to be; but that, on minute examination, shades of difference will be perceptible in their heads, corresponding to the differences really existing in their minds. But it will also necessarily follow, that the difficulty of observing and appreciating these minuter shades of cerebral differences must, to an inexperienced person, be equally great, as it would be for a stranger to discover, at a first interview, the slighter shades of character by which each is distinguished from his neighbour.

Influenced by the difficulties of accurate observation amidst a general uniformity of this description, the phrenologists wisely advise beginners not to trouble themselves at first by looking for *proofs* among individuals known only for average mental endowments, and in whom, consequently, all parts of the brain may be nearly equally developed. After they have acquired experience in observation, they may obtain additional light by this means; but *in testing the truth* of the phrenological concomitance, it is far more satisfactory to begin with well marked cases, in which one or several of the mental faculties are very strong or very deficient, and in which, consequently, if phrenology be true, we may expect to find the corresponding parts of the brain equally remarkable for size or deficiency, and therefore easy of observation. For the same reason, they advise that the larger organs of the propensities or moral sentiments be selected for verification, in preference to the smaller and more difficult organs of intellect, and that the attention be fixed, at first, exclusively on strongly marked cases, in which no doubt can exist either as to the energy of the mental faculty or the magnitude of the organ. We would even go farther and counsel those not much accustomed to precise observation, to commence with cases in which a particular region of the brain or group of organs preponderates over the others, and in which the character is broadly marked by the energy of the corresponding faculties; just as in studying the geography of a new country, we should first make ourselves familiar with its leading features, and more general divisions into districts and counties, before seeking to determine minutely the positions of its towns or the precise courses of its rivers. When the eye is thus trained to the correct observation of the larger features, it will experience much less difficulty in taking accurate cognizance of details.

According to the phrenologists, the brain, considered as the organ of mind, may be divided into three great regions, the first comprising the anterior lobe, and serving for the operation of the intellectual faculties; the second comprising the coronal region, and more immediately connected with the moral sentiments; and the third comprising the posterior lobes and base, and serving for the manifestation of the propensities common to man with the lower animals. In a person of a well-consti-

tuted mind, these three regions, and the corresponding groups of faculties, are in due proportion to each other; but wherever the character is marked by the predominance of the lower passions and by feebleness of intellect and moral emotion, as in most criminals, the posterior and basilar regions will be found in excess, and the coronal and anterior portions narrow and defective, or the "forehead villanous low." Where, on the contrary, as in Melancthon, the moral sentiments and intellect form the prominent features of the mind, and the passions are weak, the anterior and coronal regions will rise high and arched over a comparatively small base and posterior region.

Here, then, is a good field for a beginner. To ascertain how far physiologists in general, as well as phrenologists, are right in considering the anterior lobe of the brain to be more immediately connected with the intellectual faculties, it will be easy to compare the expanse of forehead in congenital idiots with that of men of ordinary intelligence, and still more of men of great and general talent. In most cases of this kind, the idiocy arises from defective development of the brain, and especially of its anterior portion; and it requires one only to visit a few asylums or workhouses to observe the stinted dimensions of the foreheads of idiots, as contrasted with the lofty brow of a Bacon or a Shakspeare. The creative genius—the highest attribute of intellect—of Michael Angelo scarcely formed a more striking contrast to the mental inanity of the idiot mentioned in the Phrenological Journal, vol. ix., p. 126, than do their respective foreheads represented from nature in the annexed woodcuts.

Michael Angelo.

An Idiot, aged 20.

If we pursue this enquiry throughout the whole family of man, we invariably find the forehead most developed among the races most remarkable for general intelligence and the reverse. Lowest in the scale of organization are perhaps the aborigines of some parts of New Holland; and from them we have an almost regular gradation through the Carib, the Esquimaux, the North American Indian, the New Zealander, the Negro, the Sandwich Islander, and the Hindoo, up to the European, who has decidedly the largest forehead and highest intelligence of them all. It is true that among idiots we occasionally find an example of a very large and prominent forehead and head, as among the cretins of Switzerland; but these are generally cases of hydrocephalus, or of other forms of cerebral disease in which disorganization has taken place, and in which the mental faculties have become impaired as the disease advanced. We have seen smallpox induce idiocy in this manner in a scrofulous subject; and it is not an uncommon termination of long-continued mania. These facts, however, constitute no exception to the axiom, that a brain below a given size is incapable of manifesting the mental faculties in a healthy and efficient manner.

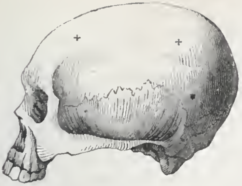
If, to induce us to test the fact by direct observation, we required farther presumptive proof of the connexion of the anterior parts of the brain with the intellectual powers, we would refer to the general experience of mankind, and to the many attempts made to measure the one by the other. Camper's celebrated facial angle, which affords results generally accurate but presents easily explicable exceptions, is founded on the principle of the anterior lobes being not only the seat of intelligence, but proportioned in development to the extent of the intelligence; and it fails only from overlooking disturbing causes, which phrenology at once points out, and enables us to avoid.

To ascertain the connexion of the animal propensities with the posterior and basilar portions of the brain, we have only to observe the heads of men who are notorious for the fierceness of their passions, for selfishness, cunning, and utter want of principle; and those of men whose delight is in doing good, quietly and unostentatiously, and whose passions are never roused, except in defence of suffering humanity. If, for example, the heads of a Sykes and a Fagin do not form a contrast, in the preponderance of the basilar regions, with those of men like the Brothers Cheeryble of Boz, as remarkable as the contrast between their characters, we need scarcely go further, as it would prove, unexceptionably, the non-existence of the alleged concomitance. But if the contrast is, in reality, as striking as it is said to be, then let us note it well, and continue our observations on characters of a different kind, till evidence shall accumulate sufficient to warrant an opinion on the general truth of the principles on which the phrenological mode of investigation is founded.

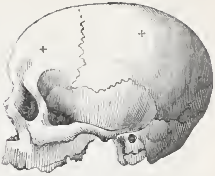
Having ourselves bestowed much pains on the verification of the phrenological evidence, and learnt, by experience, the best way of surmounting its attendant difficulties, we would earnestly recommend those of our readers, who are really desirous of satisfying themselves of the

truth of Gall's discovery, to begin by visiting any of the phrenological museums, such as the splendid collection of Deville, in the Strand, and placing, side by side, thirty or forty heads of abandoned criminals, and as many of persons of superior intelligence and morality, and contrasting the general features of one class with those of the other. In this way, differences will become palpable, which, viewed singly, might be overlooked; and if, with shades of difference in other respects, the whole of the criminals' heads shall be found to present a large base of the brain, and a comparatively low and narrow forehead and coronal surface, while those of individuals noted for superior virtue and intelligence show the proportions reversed, it will become very difficult to deny the probability of some fixed relation subsisting between the organization and the mental qualities. We have tried this test, on a great variety of occasions, in France, in Italy, and in Germany, as well as in this country and in Ireland, and we feel bound to admit, that the general coincidence was very striking. Among the criminal heads we found two or three, on different occasions, which presented a larger forehead and coronal development than the rest, and which brought them nearer the type which is considered to indicate average morality and intelligence; but, on further enquiry, we found that these apparent exceptions belonged to criminals superior to their class, by the very traits of character which their heads indicated; and that they had come under the law of the land, not from the energy of low and brutal passions, but from employing their intellects in schemes of embezzlement or forgery. We are not aware, however, of even one instance of a really ferocious and degraded character being unaccompanied by a decided preponderance in the basilar and posterior convolutions of the brain. Nor have we been able to discover a single example of a person presenting such a development, being noted in the world for refined morality or elevation of mind.

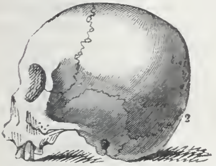
Having repeated this experiment a sufficient number of times, with different sets of heads, it will be instructive next to compare the skulls of savages with those of any of the European nations, or the least civilized with the more civilized—the New Hollander, for instance, with the Hindoo, or the Carib with the South Sea Islander. The Phrenological Society of Edinburgh possesses, what Tiedemann, after visiting it, admits to be the largest existing collection of national skulls, and many of the societies scattered throughout the kingdom possess either skulls or casts of skulls from various parts of the world. Deville's museum also contains many, which are accessible to every one. In several of these museums we have tried the same plan of contrasting different races with each other, and, speaking generally, the coincidence of development of brain, with the known character of the respective races, appeared such as could hardly fail to strike every intelligent and conscientious observer, as affording the strongest presumptive proof of Gall's discovery. We introduce the annexed diagrams as strikingly illustrating the foregoing observations and our subject generally.



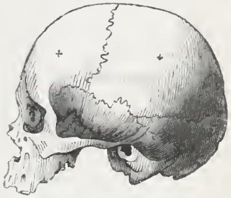
Negro



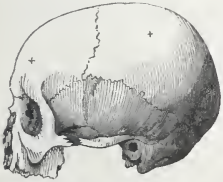
Hindoo.



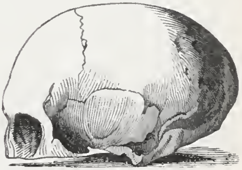
Peruvian



European



Sandwich Islander.



The Poet Burns.

We have seen an exhibition of national skulls arouse attention and excite an interest, which ended in ultimate conviction, in minds prejudiced to the last degree against phrenology; and it may be thought worthy of notice, that the anatomist Dumoutier, who is the Deville of Paris, is at this moment on a voyage round the world, in one of the discovery ships sent out by the French government about a year ago; and that the principal object of his mission is to collect skulls, and take casts or drawings of the skulls and heads of the natives, wherever the ships may touch, for the purpose of serving as phrenological illustrations. We have no doubt that he will return with a rich and valuable collection. In this respect, the conduct of the French government differs widely from that adopted by our own about ten years ago, when the collection of skulls, made for phrenological purposes, by Mr. Collie, Surgeon of H.M.S. Blossom, during a similar voyage of discovery, was taken possession of on his return, and rendered of no use either to science or to himself. Captain Beechey would not even accept the offer of a short report on their phrenological indications, which was volunteered by Mr. George Combe, and which would have added to the

interest, at least, of Captain Beechey's narrative, without possibly doing it any injury.

Having so far prepared himself for making accurate observations, the next step for the phrenological enquirer will be, to examine the general outlines of the heads of those persons whose dispositions are most marked and best known to him; still confining himself, however, to the regions rather than to individual organs. Let him for a time disregard all medium cases, and seek only for extremes. It is from the latter that PROOFS are to be most satisfactorily obtained; for, as yet, the numerous difficulties inseparable from imperfectly defined cases, would only perplex and confound him. The medical man possesses many advantages in pursuing this enquiry. He not only sees human character and human weaknesses in the confidential intercourse of private life; but in hospitals, in gaols, and in schools, he may select the most conclusive cases as evidence, and multiply proofs to his heart's content, before pinning his faith to any man's creed. But in all his proceedings, let him be cautious and steady; neither hasty in adopting evidence, nor precipitate in rejecting it. Some things appear at first sight to be conclusive, for or against a doctrine, while they are so only from being imperfectly known. But wherever, on due examination, facts seem to demonstrate a truth, let nothing turn him away from its adoption; and, on the other hand, let nothing tempt him to retain an opinion which facts appear conclusively to falsify.

It will be found impossible, we think, for any candid person to pursue the above mode of enquiry, for any considerable period, without becoming impressed with the conviction, avowed by other eminent observers as well as by Gall, that the degree of intelligence is, *cæteris paribus*, proportioned to the development of the anterior lobes of the brain, not in man only, but also in the lower animals. In Vimont's magnificent work on Comparative Phrenology, proofs of this fact superabound; and it is a matter of common observation, that dogs, horses, monkeys, and other animals remarkable for intelligence, have large and rounded foreheads. We are aware, indeed, of supposed exceptions to the rule in persons who present an apparently large and broad forehead, and yet are by no means superior in talent. But in all such cases, where the original constitution or temperament is not very low, and disease has not impaired the cerebral functions, the anterior lobe will be found to be really very moderately developed; and the fallacy to arise from judging of its size by height and breadth alone, *without taking depth into account.* A deep anterior lobe is one which extends far forward over the orbitar plate of the frontal bone, and projects over the eye and cheek-bones. A shallow anterior lobe, on the contrary, is short, and scarcely advances far enough to protect the eye. The distance forward, from the lower extremity of the coronal suture, is a good indication of the length of the anterior lobe, and will be found to vary not a little, even where the mere fronts look equally large. This will be easily understood, by supposing an observer to be placed directly opposite the ends of two logs of wood, each a foot square, but the one twenty feet long, and the other only ten. It is clear, that were he to judge merely from the end view, he would declare both logs to be equal, although, in reality, the one was double the size of the other. It is the same with the anterior lobe; in order to avoid mistakes, its depth or length must be reckoned, as well as its height and breadth. We have heard this called a "loop-hole" for the phrenologists; but,

call it by what name you please, the question which concerns us is, simply, whether it is *a fact?* We confess that our experience obliges us to admit the reality of the distinction here pointed out, although at one time we overlooked it; and it is nowhere more palpably seen than in the large-looking but really shallow foreheads of the Peruvian skulls, compared with the apparently smaller but much deeper foreheads of the Greeks, French, or British.

A similar precaution is required in estimating the development of the coronal region of the brain. Many of the criminal heads present a rather broad upper surface; but it extends almost like a flat plain, and rises little above the level of the points of ossification in the parietal and frontal bones, instead of forming the high and arched appearance which we remark in the heads of Sully, Melancthon, and others, noted for the energy of their moral feelings. But the best way to ascertain the real size of the coronal region, is to compare a number of heads of persons remarkable for moral endowments, with those of depraved criminals, or of persons known to be deficient in the higher feelings of our nature. If this plan be followed, the difficulties will, to a great extent, disappear. But in this, as in other comparisons of a similar kind, it ought to be kept in mind, that it is not the absolute size of a portion of the brain, in one individual, that is to be compared with its absolute size in a different individual. The true point of comparison is the predominance of a given portion over the other portions *in the same head*, with a similar preponderance over the other portions in a different head. The comparison is, therefore, not a single but a double one; and it is not absolute size that is to be compared, but the relation between an existing preponderance in each of two heads, considered with reference *each to its own standard.* For example, there is a wide difference betwixt affirming that A's nose is larger than B's, and affirming that A's nose is larger relatively to the rest of his features than B's relatively to his. The latter proposition may be perfectly correct, and yet B's nose be the larger of the two in absolute size. It ought, therefore, to be distinctly understood, that in all comparisons between different heads, this double standard or comparison is implied—because, if this be overlooked, much confusion may arise.

Having thus made ourselves familiar with the larger divisions of the brain, we next proceed, in our verification of the phrenological evidence, to test the functions ascribed to the individual organs or portions of the brain; and here, also, every precaution must be used to avoid error, and we should be careful to begin with those organs, which, from their size or situation, are most easily observed.

Some of the leading propensities are in great activity in childhood and youth, and, when possessed in a high degree, present very favorable opportunities to the enquirer. In early life, manners are not yet broken into that conventional standard to which most people endeavour to approximate on becoming active members of society, and consequently the natural qualities of the individual stand forth in a more recognisable form than at a maturer age. Hence the facility with which we may then test such propensities as self-esteem, the love of praise, cautiousness, affection, secretiveness, and destructiveness. The sly timidity and shyness of one child contrasts strongly with the bold and confident openness of another. In one, a fiery temper rages without control; while another is remarkable

for patient submissiveness. Contrasts such as these cannot be mistaken, and if the organization shall not be found in harmony with each, phrenology must inevitably perish. Facts alone are what it has to stand upon.

It would be out of place, even were it possible, to enter here into a detailed exposition of the mode of observing every individual organ, or of the evidence on which its function is held to be ascertained. For that we must refer to the works named at the head of our article, and particularly to the "Functions of the Brain" of Gall, the "System" of Combe, the "Human and Comparative Phrenology" and plates of Vimont. All that we can do here is to point out such things as we found most useful in making our own observations, and to add that in verifying the individual organs, we derived the greatest assistance from placing side by side (but always with reference to the principle already explained) heads and skulls in which the organ in question was possessed in opposite degrees of development. Thus, in examining destructiveness, we placed a row of murderers and ferocious savages alongside of a row of virtuous characters and Hindoos; and in studying the organ of tune or melody, we contrasted a row of musicians with an equal number of persons indifferent to music. In this way, the larger features come out prominently, and leave no doubt as to the conclusions deducible from them. It is in this way that the collections of skulls and casts of dead and living characters, formed by Deville and many of the phrenological societies, become of great practical value; and we would advise those who, like Dr. Holland, reject the evidence altogether, on the plea that the facts are not numerous enough, to study for three months those which *already* exist in such a collection as Deville's, before they again express an opinion on the subject. We are far from thinking that, after doing so, they will agree in every inference drawn from them by Deville himself, or by other phrenologists; for the latter, like other fallible men, often enough take a step beyond the point of solid support, and in consequence sink into the mud of error. But we should be greatly surprised to meet with any man of average honesty, intelligence, and industry, who did not rise from such an enquiry with a higher respect for the genius and labours of Gall, and with more than a suspicion that the new physiology of the brain is true, in its great principles at least, and requires only to be assiduously cultivated to lead ultimately to a rich harvest of important results. To those who really seek truth, we would say, Do not be too much influenced, either by the successes or the failures of the phrenologists, but *go to nature and observe for yourselves.* Individuals may make "lucky hits" or occasional "mistakes;" but if the main facts are true, they will remain to speak for themselves, in a voice which cannot be misunderstood by any one desirous of understanding them; and will be found to substantiate the opinion of Cuvier—that, as "*certain parts of the brain attain, in all classes of animals, a development proportioned to the peculiar properties of these animals*, one may hope, *by following up these researches, at length to acquire some notion of the particular uses of each part of the brain.*"

Before leaving this part of the subject, we must repeat, that in judging of the development of an individual organ, as a direct test of its function, *its size ought first to be compared with that of the other organs in the same*

head, and not with any abstract or ideal standard. A faculty is strong or weak in proportion to the other faculties of *the same* mind, and the general character takes its hue from *its own* predominant qualities. Hence the obvious necessity of measuring mental power and cerebral development with reference to the individual himself, when seeking for *proofs* of the concomitance of the one with the other. It is only by keeping in mind this standard, that we can compare the size of an organ in one head with its size in another.

Long as we have already dwelt on the subject, there are numerous points of much importance, directly connected with it, which we have been obliged to pass over in silence, and others which we have touched upon very cursorily. But as our object is not to teach phrenology, but to draw attention to it as eminently deserving of serious enquiry on the part of the profession, our omissions are of less consequence. At the same time, we wish we could have spared room to state more fully what phrenology is, and to show a few of the numerous applications which may be made of it if it shall prove to be true. In the prevention, discrimination, and treatment of insanity and of nervous diseases, it already affords great assistance to the physician; and when it shall be freed from some of its accompanying errors, and brought to a maturer state, there will hardly be a possibility of overrating its practical value in education, in legislation, in the prevention of crime, and the treatment of criminals, as well as in medicine. If true, it furnishes the elements of the physiology of the brain and of the philosophy of mind; and no ghost is required to tell us how useful both of these branches of knowledge must be in improving mankind, and adding to human happiness. Although we are not so thoroughly satisfied as to consider ourselves phrenologists, in the full sense of the term, we have paid enough of attention to it, to warrant our forming a high estimate of its value, if it shall ultimately prove to be true. That it is rapidly advancing in professional estimation, is evident from many signs, and, perhaps, from none more clearly than the extent to which our best-conducted lunatic asylums are already under phrenological guidance. Every day, indeed, is adding to the number; and the direct evidence, proceeding from many quarters, that phrenology is found of daily and hourly use in the treatment of the insane, certainly affords a strong presumption that, in its great outlines at least, it must be both true and valuable.

We have said nothing about the objections against phrenology, founded on its alleged tendency to materialism, fatalism, irreligion, &c. &c.; because discussions about consequences are utterly superfluous till the truth be ascertained. IF PHRENOLOGY IS A TRUTH, *it is impossible that its use can lead to anything bad.* If it is true, God is its author, and something more than assertion is needed, to prove that HE has connected any one truth with consequences necessarily hurtful to his creatures. IF IT IS FALSE, its consequences may and *must be* bad; but then the way to get rid of them is to *prove it false*, in which case, the consequences will fall along with it into one common grave, and give trouble to no one. We may add, however, that to our minds it seems to leave materialism and fatalism precisely where it found them, and to plant religion on the imperishable basis of adaptation to the constitution which God has given to the mind of man.

Art. VIII.

The Transactions of the Provincial Medical and Surgical Association. Vol. VII.—*London and Worcester,* 1839. 8vo, pp. 574.

This volume, like its predecessors, opens with a report of the proceedings at the previous meeting of the Association; but as we have already had occasion to notice these, we shall proceed at once to give some account of its literary contents. The first article is Dr. Malden's retrospective address. Although it is evident that the professional avocations of that gentleman have prevented him from engaging in the laborious and extensive research requisite to produce a full abstract of the improvements and other novelties—the latter, as usual, materially outnumber the former—of the previous year, especially of those originating in foreign countries, yet his elegant oration will be read with no common interest. The brief enquiry into our recent national contributions to medical science is judiciously and candidly conducted, and exhibits Dr. Malden, in the light of an experienced practitioner, closely scrutinizing the views of others, by the aid of his own lengthened clinical acquaintance with disease. As a literary composition, Dr. Malden's essay possesses merit of a very high order, and may be triumphantly appealed to in refutation of those who affirm that the character of the studies and pursuits of medical men unfits them to become graceful and elegant writers.

I. The Transactions are, as usual, remarkable for the importance of their topographical papers. One of the corresponding members of the society, Professor Otto, of Copenhagen, has enriched the present volume with an elaborate account of the medical peculiarities of that capital. Here we have an essay exuberant with matter, all of which is admirably arranged, while each particular topic is treated of with the degree of fullness and precision due to its relative importance. Dr. Otto commences by passing in review the situation of Copenhagen; its buildings; the modes of ventilation and warming practised in private houses; the climate of the town and surrounding districts; the physical and mental constitution of its inhabitants; their public and private amusements; their habitual food and manner of living; and, finally, the means employed by the authorities for the preservation of the general health. The examination of these topics constitutes the natural exordium of such a paper as the present, as it embraces all those agencies which past experience has taught us to regard, as capable of influencing the character of disease, and the efficiency of remedies in a particular region. We might extract, to a large amount, from this portion of the essay, with the certainty of engaging the reader's attention, but must content ourselves with recommending it to his perusal. The regulations adopted by the government for limiting the sale of poisons to proper persons, for preventing the consumption of spoiled food, and the use of noxious or poisonous colouring matters in the confection of sweatmeats, &c., must, if actually put into practice, and not, as is too often the case elsewhere, merely nominally prescribed, exercise a very material influence on the prevention of the evils they are meant to check. That the enlightenment of Danish medical policy is not, however, without its limits, clearly ap-

pears from the fact, that of the five members of the Royal Board of Quarantine, not one belongs to the medical profession. The evils likely to arise from this arrangement are in a great measure averted, according to Dr. Otto, by correspondence between the Board and the Royal College of Health; we doubt the success of the remedy, and may, with fairness, remind the Professor of the old adage, "prevention is better than cure." The notions prevalent in Denmark, on the subject of contagion, are, it would seem, somewhat antiquated, and requiring modification, quite as much as the organization of the Board of Quarantine. That "Copenhagen is much exposed to the importation of the *yellow fever*" is surely an assertion at which, although it is qualified by certain provisoes, we may, at the present day, be permitted to marvel.

Dr. Otto considers the diseases of Copenhagen under the three heads of *epidemic*, *endemic*, and *contingent*. There is nothing particularly worthy of notice related respecting diseases of the first class, with the exception of variola. After briefly alluding to various outbreaks of this disease, which have occurred at Copenhagen since 1528, the writer proceeds to describe the observations made during an epidemic of the disease, which has lasted from May, 1832, up to the present time. Of 1045 patients admitted into the hospital during this period, 44 died: 147 of these individuals, who had not been vaccinated, furnished 34 of the deaths; while 10 only of the remaining 898 persons, all of whom had been vaccinated, perished. The general inferences, drawn from the observation of the epidemic, are expressed by Dr. Otto, in a number of propositions, of which the following strike us as most important: "The prophylactic power of the vaccine gradually decreases, and after the course of some years, ceases entirely, with the greatest part of the vaccinated. The character of the cicatrices, after vaccination, is in no proportion to the greater or less modification of the smallpox." We shall shortly have an opportunity of examining the validity of these propositions. We shall now content ourselves by stating that the first is anything but proved.

Belladonna is no longer, we are informed, regarded as a prophylactic against scarlatina. If Dr. Otto's estimate of the intellectual character of his countrymen—namely, that they possess more judgment than imagination—were strictly correct, they would not have been so long in arriving at their present conclusion; they would have seen through the hollowness of the evidence on which the alleged preventive powers of the drug rested, and quietly added the tale to the host of fallacies originating with dreamy speculators.

The endemic diseases of this insular city are, ague, catarrh, rheumatism, gout, hemorrhoids, scrofula, tubercular consumption and hydrocephalus, autumnal cholera, psora, and a syphiloid affection resembling the Scotch *sibbens;* stone, dysentery, and scurvy, are represented as of rare occurrence.

Catarrh, manifesting itself by cough and coryza, is stated to lay the frequent foundation of phthisis and other intractable diseases. We are persuaded Dr. Otto is too well acquainted with the rigorous demonstration required, in more southern latitudes than his own, of such important practical facts as this, to feel irritated at our unwillingness to credit the reality of the assumed correlation of bronchitis and tubercle.

So long as Dr. Otto withholds the proofs of his assertion, we shall continue to believe, that the laws discovered to regulate the relations of those diseases, in other climates, hold good in the capital of Denmark. Two tables are given, from which it appears that tubercle produces one eighth of the total mortality and one thirtieth of the total sickness of Copenhagen.

Sixty deaths occurred from hydrocephalus in 1835, and sixty-two in 1836, showing the extreme frequency of the disease. Our author is disposed, with many of his colleagues, to consider this affection of scrofulous nature, from its especially attacking children of that constitution. He appears to be unaware of the much stronger reason afforded by the pathological anatomy of the disease, for regarding it in the majority of instances as of tuberculous nature. The reader need not be informed that we refer to the presence of tubercles in the pia mater of hydrocephalic patients, a condition forming the anatomical character of the disease described by our countryman, Dr. Hennis Green, under the title of *granular meningitis.*

We pass with our author to the category of contingent affections. Genuine sthenic inflammations appear, on the whole, to be somewhat rare in Copenhagen, scarcely one out of twenty inflammatory disorders being of that type. Those met with are stated to be "rheumatic, nervous, or gastric complications, and varying according to the different seasons and the state of the weather." Of what these conditions are complications we are not informed. Venesection is consequently not carried to any great extent; in cases of pneumonia, for example, one or two bleedings of twelve ounces each are usually found to be sufficient; tartar emetic has of late been used with great advantage after venesection. Calomel and opium are often prescribed, particularly in inflammations of the lungs and liver; "though not so much and so indiscriminately as in England:" for this last the Danes have good reason to thank their stars.

The treatment of typhoid fever pursued by Professor Bang is described at some length, and emphatically lauded: "such is its happy result, that of three thousand patients, only three hundred and seventy died." In other words, one of every eight cases terminated fatally. Now of one hundred typhoid patients treated by M. Louis, simply with emollient ptisans, seltzer water, and linseed enemata—a mode of treatment scarcely differing from pure expectancy—nine only died, that is, one in eleven. And yet, as we can ourselves testify, his cases were frequently of the severest kind, accompanied with intestinal hemorrhage to a considerable amount, the formation of large eschars, &c. The natural course and mortality of disease should at least be ascertained before we pronounce panegyrics on our treatment.

We have so recently laid before our readers a full analysis of Dr. Guldberg's remarkable work on Delirium Tremens,* of which the materials were collected at Copenhagen, that scarcely any novel matter referring thereto can be gleaned from the present essay. The relative frequency of the disease is, however, calculated through a longer term of years than in Dr. Guldberg's volume. "From the year 1826 to 1836

* See British and Foreign Medical Review. Vol. VI., p. 320.

inclusive, of 23,765 patients admitted into Frederick's Hospital, 990 (1 in 24) had delirium tremens,—a number which must astonish the reader, the more when he reflects that this has occurred in one hospital only, and an equal and perhaps still greater number must have been treated in the general hospital, as well as in the military and naval hospital and in private practice." Dr. Otto's patriotism no doubt prevented him from inserting delirium tremens in his list of *endemic* diseases: it is plain, however, that Shakspeare was right (he did not often err) in setting down " your Dane " as a great drinker.

The majority of Danish practitioners still continue to treat syphilis with mercurial preparations, especially with calomel; when symptoms of ptyalism appear the medicine is discontinued, but resumed when they have been removed. Twelve years ago, however, Dr. Otto, on his return from a visit to this island, turned his attention to the non-mercurial treatment of the disease; several of his brethren have since made trial of the plan, and the following is the statement given of their joint experience of it: " The results which were published proved the value of the method even beyond expectation; so that I now never use mercury against syphilis, effecting a complete cure of all its forms within a much shorter time than was formerly done by mercury, and without seeing secondary symptoms arise afterwards. Professor Wendt, Dr. Müller, and Dr. Svitzer have been equally successful with this mode of treatment; though they think that there are forms of syphilis which require mercury, and consequently now and then employ it. I frankly confess that I have not seen such cases."

The subjoined figures will give the reader some idea of the movement of the population of the kingdom of Denmark during one year, 1835:

Population.	Marriages.	Births.	Deaths.
1,234,000.	10,088.	41,052.	30,170.

Proportion of girls to boys as 1000 : 1053.
.... of do. to do. born dead as 1000 : 1531.
.... of illegitimate to legitimate births as 1 : 8.
.... of deaths to whole population as 1 : 41.*
Number of deaths by suicide 192.

The proportion of illegitimate births would appear to be greater than in any other country; from 1831 to 1835 there were 197,300 births, and of these 18,688, or 9 per cent., were illegitimate.

III. *On the Medical Topography of Swansea.* By Mr. J. W. Gutch.

IV. *On the Medical Topography of Exeter.* By Dr. T. Shapter.—We postpone, for the present, our intended notice of these papers: the latter is indeed still uncompleted.

V. *On a Peculiar Affection of the Uvula.* By Mr. E. Thompson, of Whitehaven.—Mr. Thompson is of opinion, that in cases of simple inflammation of the throat terminating suddenly by death, the fatal event is caused by asphyxia, from enlargement of the uvula. " The affection

* In England it is 1 in 49. See Farr's letter to the Registrar-General.

consists in the elongation of the mucous coat of the uvula, by the gradual effusion of lymph, to such an extent as to endanger life, by its being inspired into the glottis, and putting an immediate stop to respiration." We by no means question the fact of such elongation occurring, but no *proof* is given by the author that death has ever been induced in the manner alleged. Mr. Thompson does not increase the confidence of the critical reader in his accuracy, by indulging in such loose and questionable statements, as that "elongation of the uvula occasionally destroys life in typhus fever, by producing almost instant suffocation."

The secretaries seem to have forgotten their critical functions, in allowing this paper to pass muster in its present state. The meaning of such sentences as the following—there are not a few such—may not be doubtful, but assuredly the language is not passable. "In the first case a somewhat hasty *examination had nearly lost a valuable life;* for had the paroxysm occurred in the night, or had *he* not been capable of great exertion, death would have certainly taken place."

VI. *A Report on Midwifery in Private Practice.* By Mr. C. B. Rose, of Swaffham.— Mr. Rose contributes a brief but really interesting statistical record of his obstetric practice during a series of years. He very correctly observes that, as reports of this kind have hitherto been chiefly furnished from the documents of public institutions, our knowledge on the subject of the statistics of parturition is in truth only applicable to the lower orders of the community. We trust the example he has so creditably shown them, will be followed by such members of the association as are engaged extensively in midwifery practice: in this way a mass of facts, from which most useful inferences respecting the value of female life might be drawn, would ere long be produced. Mr. Rose's cases, and the peculiarities attending them, are grouped in the following table:

600 Cases.		Sex.		Twins.	Preternatural Presentations.			Face.	Funis.	Hemorrhage.		Convulsions.	Stillborn.
Nat.	Pret.	Boys.	Girls.		Foot or knees.	Breech	Arm or elbow.			Unav.	Accid.		
582	18	316	294	10	7	9	2	1	2	2	18	2, and 2 with prem. sym.	34

This practitioner met with one case in which the placenta and fœtal head were expelled together, the former having been pushed into the vagina by the latter; the hemorrhage was not alarming, and both mother and child did well.

The paper contains some not injudicious remarks on the exhibition of the ergot of rye in labour, which may be profitably consulted.

VII. *Cases of Hernia Cerebri, with Remarks.* By Mr. G. Mallett, of Bolton.—Mr. Mallett resides in a mining district, and has *consequently* met with several cases of severe fracture of the skull, and briefly relates

the chief circumstances of five of these, four of them attended with encephalocele. We shall abridge the most remarkable of the latter. A circular portion of bone belonging to the frontal and parietal bones, and about one inch and a half in diameter, was driven by a falling piece of coal at least three quarters of an inch into the brain of a boy aged twelve years. When seen after the accident, the patient was sensible, the right side paralysed, vision of the right eye nearly gone, pupil dilated; there was neither fever nor delirium. After the lapse of a week a portion of brain protruded, gradually increasing in size until it measured six inches in its antero-posterior, three and a half in its transverse, and two inches in its vertical diameter. The mass was then removed with the knife, the patient being scarcely conscious of the passage of the instrument through its substance: there was a piece of bone found in its interior. In eleven weeks after the operation, the patient was able to walk a distance of upwards of two miles. Subsequently he recovered his original share of intelligence, and with the exception of persistent paralysis of the right arm, appeared in every respect as before the accident.

Mr. Mallett is of opinion that, inasmuch as in the four instances in which cerebral hernia occurred, a foreign body was discovered in the tissue of the brain; and in the case of fracture, uncomplicated by such protrusion, no fragment of bone or other extraneous matter was therein detected, encephalocele is, in the majority of cases, the direct result of foreign irritation.

In all the cases of protrusion, moderate pressure, and various topical applications, were first had recourse to, but in two were found totally ineffectual. Here the operation was performed. If, after ablation of the protruding substance, there should be a tendency to its reproduction, Mr. Mallett advises the employment of moderate pressure, and the daily use of a nitric acid lotion, containing forty drops of the acid to two ounces of water.

VIII. *A Case of Intussusceptio, with Separation of Five Inches of Intestine, followed by complete recovery.* By Mr. John Fox, of Cerne Abbas.—An interesting case, in which inflation of the bowels was had recourse to, and, though so late as the sixth day, with manifest advantage.

IX. *A Case of Molluscum.* By Mr. C. Fowler, of Cheltenham.—Correct descriptions of this singular affection are so rare, that we willingly transfer the following history (slightly abridged) to our pages:

"This singular affection first made its appearance about twenty or thirty years since, [the patient is a male, aged sixty-eight,] commencing in small accuminated elevations upon the waist and wrists, and gradually increasing to their present magnitude. The large and pendulous tumour hanging from the crest of the left ilium, and which it was customary with him to place in his breeches pocket for support, whilst at work, was the first that made its appearance; this, and another large one between the arm and thorax of the same side, were the only ones which, from occasional subjection to pressure, gave him any degree of uneasiness or pain. These tumours were thickly disseminated over the whole cutaneous surfaces, from the crown of the head to the sole of

the foot, and varied in size, from the smallest visible elevation of the skin to one on the left waist, which weighed about three quarters of a pound; some of them are attached by broad bases, and others by stems of various degrees of length and magnitude. The tumours upon the back and waist were the most thickly set of any, and looked like clusters of reddish grapes; those upon the abdomen and lips were longer and more irregular in form. The skin covering them was perfectly sound, but possessing more of a rose tint than the surrounding integument; in those, however that had been subjected to pressure upon their summits, the colour had changed to a dirty bluish gray; the skin, likewise, was very much attenuated, and easily raised from and rolled over the subjacent tumours; forcible compression with the fingers seemed to produce very little uneasiness in them, and the sensation to the touch, when handled, very nearly resembled that of fatty tumours, only somewhat harder in consistence, and many of them, particularly the larger ones, were distinctly lobulated. Two of moderate size, situated upon the abdomen, were removed, and on making a section of them, no very great difference was observable in them from the appearance of fatty tumours generally; there was the same semi-transparent yellowish tinge pervading the whole mass, as in the latter, but the consistence was denser, the cut surface smoother and firmer, and the substance more uniform throughout. The skin was loosely connected with them, and a fine cellular membrane surrounded the whole, and dipped into the interstices of those that were lobulated. The excision of these tumours was accompanied with the loss of but a few drops of blood." (p. 365.)

The case is illustrated by an excellent engraving.

X. *On the Physiology of the Brain as the Organ of the Mind.* By Dr. Cowan, of Reading.—We have noticed, in our last article, at considerable length, the subject of this interesting and important paper. Dr. Cowan is a philosophical, in other words, a reasonable phrenologist.

XI. *Remarks on the Use of the Fucus Esculentus as a Bougie.* By Dr. T. A. Aitkin, of Poole.—Dr. Aitkin has employed the stem of this plant, as a bougie, in several cases of stricture of the rectum, with uniform satisfaction to himself and his patients.

XII. *Observations on the Physiology and Function of the Liver.* By Dr. M'Cabe, of Cheltenham.—The drift of this communication is not very obvious; at any rate, it contains nothing worthy of notice, and its place might have been well supplied by matter of a more practical character.

XIII. XIV.—*Reports of the Town Infirmary and of the Asylum for Children at Birmingham.* By Mr. F. Ryland, of Birmingham.—These are very interesting papers, but furnish no materials for analysis. They deserve the attention of the medical topographer and statistician.

IX.

Rendiconto Clinico per gli Anni Accademici, 1835-36, *e* 1836-37. Di CARLO GIACINTO SACHERO, Professore di Clinica Medica nella regia Università di Torino.—*Torino*, 1838. 8vo, pp. 356.

Clinical Report for the Academic Years 1835-6, *and* 1836-7. By C. G. SACHERO, Professor of Clinical Medicine in the Royal University of Turin.

FOURTEEN beds, our author tells us, are allotted in the general hospital, for the purpose of clinical instruction; "a small number, it may seem at first sight, to those who like to wander over many objects, without fixing their view on one; and who, miscalculating their own powers, believe that they have lynx's eyes." But the professor satisfies himself with an aphorism from the learned Stoll, "multa non multum videre volunt, qui maximum ægrorum numerum videndi prurigine tentantur."

The diseases of those received into the clinical wards are usually severe. The first four months of the academic year are devoted to the males' wards, the last four to the females'; and, in the course of these eight months, from 130 to 160 cases are presented for observation. A certain number of pupils are attached to each bed, whose office it is to attend the patients by turns, and, on the admission of each, to obtain information concerning his or her history, symptoms, and condition, from which they have to make accurate reports to the professor, and to form their own diagnosis. The professor himself next examines the patient, and confirms or annuls the pupil's opinion. The latter has then to keep a daily record, which constantly lies at the bedside, to be consulted by the other pupils, and which, at the conclusion of the case, is reduced into a proper form, with the minutes of the public dissection in those which terminate fatally.

The volume before us is composed of cases thus written, and of the professor's remarks upon them, and upon pathology in general; and though none of them are of remarkable interest, the work affords a fair idea of the present condition of both practical and scholastic medicine in this part of Italy. The cases are arranged according to an appended nosological table, in which the chief divisions are founded on locality. We have first, "Diseases of the vascular system," including periodical and inflammatory fevers. In most of these cases, "some rapid bleedings, gentle purgatives, soothing drinks or diaphoretics, abstinence and rest, were sufficient to overcome the irritation, and to prevent their passing into *angioitis*." Here we come at once to the prominent feature of the work. This angioitis is a disease, to the name of which most Italian physicians are much devoted, and they class under it many different affections, in which the symptoms during life are by no means positively indicative of any inflammation of the vessels, and in which, after death, the only evidence of such a disease is redness of the interior of the aorta and its branches. The following case, for example, is given as one of *angioitis acuta:*

"A servant girl, aged thirteen, of sanguine temperament, rather fond of wine (*ua po' amica del vino*), and often exposed to atmospheric changes, became ill

on the last day of May, and came into the hospital on the 1st of June. On her admission she was bled to ten ounces, and in the evening she presented the following symptoms: frequent palpitation of the heart, very acute pulsating pain of the head, increased by motion, face flushed, eyes glistening, skin very hot, tongue red at its edges, and mucous in the middle, respiration anxious, pulse *cardiaco-cephalic*, vibrating, metallic, pretty frequent. Diagnosis: *Angioitis.* Another bleeding was ordered in the course of the evening, and cooling drinks. On the three following days the symptoms varied but little, and she was bled five times more, and took digitalis. In the three next days the violence of the fever abated, but the pain of the head continued, and required two leeches (!) to be applied to the mastoid region. An exasperation of the symptoms occurred on the evening of the 10th, for which she was bled again on the following morning. On the sixteenth day of her disease she had sweat; the symptoms then gradually diminished, and she left the hospital on the twenty-ninth." (p. 63-4.)

There is nothing in this history to indicate that the patient had more than a tolerably severe attack of inflammatory fever, unless, indeed, the ability of a girl of thirteen to bear eight considerable abstractions of blood in ten days, be supposed to prove some more serious disease. Yet this seems a good example of that which is commonly called angioitis, and for which (its importance being much exaggerated by long arguments) it is thought necessary to employ the most severe treatment.

But angioitis exists in diseases the very antipodes of inflammatory fevers, as in the next case, which is one of *angioitis lenta*, or *arteriasis:*

"A countrywoman, aged twenty-two, born of cachectic parents, and with a pallid countenance, had for more than a year suffered from suppressed menstruation, after having bathed. She then began to have dyspnæa, on going up hill, palpitation of the heart, weight in the head and loins, loss of appetite, and other similar symptoms, which increased till she could no longer work. When she came into the hospital she had a pulsating pain in the forehead, and a pale sallow face; her tongue was red at the margins; she had thirst, nausea, weight in the epigastrium, and in the precordial region; a feeling of anxiety and suffocation, and throbbing of the carotids; the action of the heart was increased in quickness and in impulse, the pulse *cardiac*, slightly *stomachal*, tense, and frequent, the skin hot. Diagnosis: *Angioitis lenta*, with slight gastritis. She was bled at once (the amount is not stated), and again on each of the three following days; digitalis was also given, and twenty-seven leeches applied to the epigastrium. After these means, the symptoms diminished in severity; but on the twelfth day after her admission they again returned, and there seemed to be a *conatus hemorrhagicus*, the pulse being cardiaco-uterine: leeches were applied to the groins, and castor-oil was given. Next day she was better: pediluvia of mustard, and pills with iron-filings, digitalis and hyoscyamus were ordered, to restore the catamenia; they, and nutritious food, succeeded in that intention, and the patient was cured." (pp. 77-8.)

This was, assuredly, only a common case of the effects of suppressed menstruation, which the means at last employed would probably have cured at the first. But between chlorosis and inflammatory fever, nearly every disease accompanied by much palpitation of the heart, and throbbing of the arteries, seems to be called and severely treated as angioitis. It forms, too, an important ingredient in many other diseases. Thus, in a number of the Transactions of the Medico-Chirurgical Society of Bologna, which we lately read, a case was detailed by Dr. Golinelli, in which a woman had a portion of intestine completely strangulated by a band of false membrane. She died with all the symptoms of strangulated hernia, and on examining the body, twenty-four hours after death, in

addition to acute suppurative peritonitis from the intestinal obstruction, the lining of the heart and aorta was found reddened, especially where some coagula of blood lay in it. Nearly the whole of the rest of the very long paper was occupied by a most learned discourse on the question whether the patient had died of the strangulated intestine, or of arteritis; and the author summed up rather in favour of the latter!

Now it is evident that the symptoms detailed in these cases are quite insufficient to establish the existence of an inflammation of the arteries; and the only other fact on which it is assumed is, that their interior is sometimes found red after death. The inadequacy of the evidence which this affords has often been remarked on; but, without quoting authorities on each side, the resultant of whose opposite opinions might still be uncertain, we would observe, that if it is considered how acute any inflammation must be, in a slightly vascular tissue, to make it red after death,—how, in many tissues, of which the inflammatory redness can be seen during life, it disappears when life ceases, as in the skin, conjunctiva, &c.—and how improbable it is that a tissue so little vascular as the middle coat of arteries, possessing as it does even fewer vessels than the tendons, should ever, by any extent of inflammation, become red—one might expect, that arteritis would not have redness at all for a sign, or at least would not have it without other coincident results of most acute inflammation, as effusion of lymph, thickening, &c. And when to these one adds those facts which have been more generally observed, that the arteries, and many other tissues, will assume an internal redness long after death, when they are exposed to blood, and that this same redness is often found only where blood is in contact with the wall of the artery, and very rarely unless several hours have elapsed after death; while sometimes, on the other hand, no very evident redness exists in cases in which the other more determined results of inflammation are present;—the conclusion seems certain, that the redness of the arterial lining is no evidence that the artery has been inflamed. The propriety or safety, therefore, of the copious bleedings and severe antiphlogistic treatment with which palpitations of the heart and throbbings of the arteries are met by the Italian physicians, may well be doubted.

We have next three cases of puerperal phlebitis, of which two recovered by copious and repeated general and local bleedings; in one, in which the symptoms of inflamed varicose veins of the leg were by no means very severe, the patient lost a pound of blood seven times in four days, and had thirty-six leeches applied!

The next section is on diseases of the heart, in which there is nothing remarkable, except a boldness of diagnosis of pericarditis in every case, without a complete assurance of its existence by the stethoscope, and a boldness of antiphlogistic treatment fully proportioned to the phlogistic doctrine.

The same ideas are prominent in the section on cerebral affections, and to all those of the spinal cord we have still the same formidable termination,—*itis*. Among the latter, there is a case of convulsive hysteria, under the name of *myelitis membranosa lenta cum orgasmo uterino*, and two cases of sciatica and lumbago, which are raised to the titles of *myelitis membranosa lumbo-sacra*, and *spinitis sacra*. In all, very liberal abstractions of blood were the chief or only means employed.

In short, throughout the whole work, we look in vain for anything but inflammations, and the severe though simple treatment which, if they existed, they would scarcely need. But these are faults, not of the professor himself, who seems an industrious and judicious teacher, a bold practitioner, and at the same time a well-read scholar, but of the time and country in which he lives. The doctrine of inflammation in everything, is almost universally prevalent in Italy, and a large majority of the physicians add to their faith in it, a most vigorous practice against it.

The result of this general adoption of an unproved theory is, we need not say, most mischievous in every respect; but in none—not even in its exclusion of useful remedies, because completing the theoretical circle, they are not thought anti-inflammatory—in none, more than in the obstacle which it presents to all open and unprejudiced enquiry. To the one wide-expanding and all-explaining word, every observation is made to bend, and by it the whole current of enquiry is perverted or restrained.

The Italian physicians appear still to be guided by that spirit of hypothesis which, from the historic origin of medicine to the middle of the last century, held almost universal sway in every medical school, and on which, indeed, the very existence of the schools, and the reputation of the schoolmen depended. They are still striving after some expression even more extensive than inflammation, that may conceal from themselves their own ignorance of the real truths of nature. They have an Archæus in full power, if not in name—excitement and excitability—sthenia, asthenia, and hypersthenia—and all the portentous titles from which, ingeniously woven into an hypothesis, (strangely flimsy, considering the weight of its materials,) they may reason downwards to the presumed explanation of some simple facts.

Unless it be that there is the same deficiency of inventive genius among the Italians now as in their better days, it is difficult to say why this spirit of the schools should yet find a stronghold among them, after it has been driven from its once firm seat in England, France, and Germany. It is the more remarkable, because to Italy we owe the very founders of that school of physiology whose pupils were destined to supplant alike both empirics and theorists. From Italy the spirit of scientific observation spread over the rest of civilized Europe, and though checked for a time, by the popularity of the doctrinal teaching of Germany, has obtained, at last, almost an entire control. In its birthplace alone has it lost any of its just domain, and been deposed by the plausible false wisdom of "the schools," which it has elsewhere conquered.

Could they but be induced fully to appreciate the value of observed or demonstrated truth, and clearly to see its superiority over any hypothesis, however wide, or seemingly sufficient, the physicians of Italy might at once occupy the highest rank in Europe, for then they could make that an important auxiliary, which is now too often a burden—their intimate acquaintance with the literature, not only of medical science, but of sound and refined philosophy—a kind of learning with which all their works run over, and in which their brethren of other nations cannot for a moment compete with them.

ART. X.

1. *Cure of Club-foot, Bent-knee, Wry-neck, Spinal and other Deformities. With Remarks on the late progress of Art, and on the necessity of a Public Institution for the Relief of the Poor, labouring under Deformities.* By GUSTAV KRAUSS, M.D., Member of several learned Societies. With Cases and Woodcuts.—*London*, 1839. 8vo, pp. 42.

2. *Beiträge zur subcutanen Orthopädie, oder über die Heilung angeborner oder erworbener Contracturen der Glieder mittelst Durchschneidung der verkürzten Sehnen und Muskeln unter der Haut.* Mitgetheilt von Geh. Med. Rath Prof. Dr. DIEFFENBACH, in Berlin.
Contributions to the subject of Subcutaneous Orthopædia, or the Cure of congenital or acquired Contractions of Joints, by division of the shortened tendons and muscles under the skin. Communicated by Dr. DIEFFENBACH, of Berlin, to the "Wochenschrift für die gesammte Heilkunde." Nos. for September 21 and 28.—*Berlin*, 1839.

As we have so recently presented our readers with a detailed account of the principles and practice of operative Orthopædia (see our last No.), a brief notice of the above additions to our stock of information is all that will be needed: and this may be regarded as an appendix to our former article.

First, with regard to Dr. Krauss's pamphlet: after a careful examination of its contents, and consideration of its pretensions, we are sorry to express our decided disapprobation of a work which, it is too palpable, would never have seen the light, but for the hope of some personal advantage accruing therefrom to the author. We are the more reluctant to appear thus harsh in our criticism, as the author is a foreigner sojourning amongst us; and because we are far from expecting that the time has arrived when the only really justifiable motive to professional men for becoming authors, holds a solitary sway,—we mean the well-grounded anticipation of adding to the general stock of information: but when an author appears before us without any novelty to communicate, whose book is full of reclamations and self-extolling paragraphs, and this under the cloak of purely disinterested, humane, and scientific motives, we do feel ourselves called upon to ask, at least, whether there can be here any thing like the prostitution of good ostensible objects to a really selfish end? This question refers chiefly to the appeal which Dr. Krauss makes, in the form of an appendix to his pamphlet, to the humane public in behalf of the afflicted poor suffering from deformities. Would it, we would further ask, be any blow to the doctor's disinterestedness, were such an institution as he desires, to be established without his being appointed one of the medical officers? A few brief quotations from the author's preface will account for our doubts respecting this publication: "In offering these pages to the public (says the author), I obey the dictates of my heart, and discharge a duty which I feel due to those of my poorer patients, whose misfortune it is to labour under deformities. A tour for purposes purely scientific brought me to England in the year 1837. . . . . With zeal and delight I continued my labours. Obeying the calls of the afflicted, I penetrated the remotest districts of London, unremittingly seeking to bring to perfection the powers of art in the

cure of deformities. . . . . The best proofs of my success are the cures I have effected, particularly those of congenital club-foot, of the highest degree, and in patients of an advanced age. It is far from my intention to exalt my own merits, or cast blame on others for not having been equally successful. The cause of my success is attributable to my having dedicated myself more to this subject, and taken peculiar pleasure in its study. Shrinking from no sacrifice where the welfare of my patients and the development of science were concerned, I proceeded steadily in my career," &c. &c. Dr. Krauss presumes to lay down as an axiom that " there may be excellent surgeons who are, nevertheless, no orthopædists :" and that " institutions for the cure of deformities ought to be under the superintendence of men who have particularly dedicated themselves to the subject." Now, *we* presume to deprecate *in toto* this exclusive system. What ! we would ask, is the noble discovery of which we treat—a scientific deduction from just premises by a scientific surgeon—to degenerate into a mechanical art, and be placed in the hands of the mechanic? Are not Stromeyer and Dieffenbach surgeons of whom Dr. Krauss's country has reason to be proud? We repeat, as we observed in our former article and in spite of all Dr. Krauss's egotism and vanity, that vast numbers of our surgeons are as capable of treating deformities, and have been as successful in the results obtained, as himself or any other exclusive orthopædist.

In concluding this our notice of Dr. Krauss's pamphlet, we must do him the justice to say that the cases he relates show that he is acquainted with the principles of his subject, and has been a successful operator.

We now turn to the more grateful task of laying before our readers a summary of Prof. Dieffenbach's vast experience, which he—mark the contrast—is satisfied to present to the public in the form of a paper communicated to a weekly journal :—not for want of materials to build a book,—but doubtless because he appreciates the merits of those already published, and is conscious that the only additions called for are the *general* results of extended experience. In the following paragraphs we shall communicate all the principal results recorded in the Professor's Memoir, and with little or no comment of our own ; and although they are not either a close or continuous translation of the original, we shall give them the ordinary form of translation :

" In the case of contracted joints, the division of a tendon does not in itself constitute the cure, but is only a necessary preparatory step to placing the limb in a condition by which the cure may be afterwards effected. Many cases which I had deemed cured by mechanical means alone, have presented themselves again to me with a return of the deformity.

" I have now operated on about three hundred club-feet, and sixty wry-necks: independently of which, a great number of contracted arms, fingers, hip and knee joints, feet and toes have been likewise cured by operation, under my care. In all cases the contracted tendons have been divided with the aid of a small, sickle-shaped knife, similar to the old penknives, though smaller. Neither dangerous hemorrhage nor division of any important vessel ever took place; but a drop of blood was usually all that followed the division of the achilles tendon or the sterno-mastoid muscle ; or, where more bleeding by chance followed, it was always arrested by the application of a bandage. Where suppuration ensued, which was rare, a small dilatation of the opening so as to allow the free escape of the matter was generally sufficient to bring

about the cure. Pressure of the apparatus on the outer border or dorsum of the foot not unfrequently produced troublesome swelling, and occasioned the necessity of intermitting the use of the mechanical means for a time. Symptoms of nervous irritation, whether mild or acute, whether trismus or tetanus, never once exhibited themselves; and death was in no instance, whether as an immediate effect or secondary result, the consequence of the operation. In six or eight cases only was the operation for club-foot unsuccessful; and in some of these the want of success was attributable to the carelessness or fractiousness of the patient; whilst others submitted a second time to the operation, and were then cured. Age made no difference in the results. The youngest child on whom I operated was but three days old, whilst the eldest was fifty-four. [We presume the former was an experiment, and scarcely to be recommended as a precedent.] In *wry-neck*, sometimes the sternal, sometimes the acromial portion was divided near to the insertion: and many of these patients had for a long time vainly employed mechanical means. In one case of this class, a curious optical phenomenon was observed, viz., that immediately after the division of the muscle, and return of the head suddenly to its normal position, the patient exclaimed, 'I see all things awry.'

"In simple *pes equinus*, the plan pursued was, to divide the tendo achillis about two inches above the heel, and then cover the orifice with a broad band of adhesive plaster; and if the blood ceased to ooze, in two or three days, Stromeyer's apparatus was applied, and the cases usually did well in a few weeks. But in extreme cases and irritable subjects, the following plan was adopted: the limb was carefully and evenly bound with a roller from the toes to the calf, and the bandage then saturated with boiled starch, or a solution of resin in alcohol. I then directed the patient to stand upright, and press the bound-up foot firmly on the ground, and remain in that position till the bandage had become stiff and dry; and this answered the purpose of an apparatus. This proceeding was repeated twice a-week until the cure was completed.

"In the *pes varus*, the division of the achilles tendon, with the after-aid of adhesive plaster, or a bandage prepared as above, or with thin plaster of paris, were in general sufficient to procure a successful result in young children; but Scarpa's boot or Stromeyer's apparatus were found necessary in cases of longer standing and in older subjects. The various tendons divided for this form of distortion were those of achilles, tibialis anticus, flexor pollicis longus, or the plantar fascia. If the case proved obstinate, the tendons were frequently divided again and again; thus the achilles tendon was cut two, three, or four times before the cure was complete. In one adult with enormous club-feet of the highest degree, those divisions were repeated twenty times before a successful termination was obtained. [A pretty good evidence of the reproductive power of tendinous fibre or its substitute.] In extreme cases of club-foot, it is requisite to divide all the opposing flexor and extensor tendons in turn: the division of the achilles tendon should then be undertaken last of all.

"In *flat* or *splay-foot*, I have in all cases which have come under my care, whether of children or adults, divided all the long extensors on the dorsum of the foot: plaster and bandage were then applied to the hanging foot, and a straight splint placed on the anterior surface of the leg, extending to the dorsum of the foot, so as to keep it in the position of pes equinus, a matter of no difficulty, when the extensor resistance was removed: the result of this treatment was equally successful with that which followed division of the flexor tendons in the other two forms above described. [This treatment is peculiar, and differs widely from the simpler treatment of Stromeyer,—see our last Number, p. 399. One would scarcely have judged *à priori*, that the arch of the foot would have been restored by dividing the extensor tendons—so much, however, for theory, as opposed to actual experience.] The same treatment is applicable in spasmodic and intermittent cases.

"The cases which required the division of the hamstring muscles were con-

ducted in the following manner: either two or three punctures were made, according to circumstances, the same little sickle-knife being employed: the limb was then extended, and a broad strip of plaster placed around it; the position being in some measure preserved by the aid of a compress or pad, and pasteboard splint; and when all inflammatory action had ceased, Stromeyer's apparatus was applied. I have also employed a more speedy mode of obtaining the desired position of the limb. Having divided the proper tendons, I flexed the leg on the thigh so as to approximate the heel and buttock, and then suddenly extended the limb with sufficient force to bring it at once straight: this action was accompanied with a loud cracking noise, as if something had given way. A bandage of flannel (on account of its elasticity) was then applied along the whole limb, and a long, light, padded splint placed along its posterior aspect, and retained in position by appropriate strips of linen.

"In bent-arm, after division of the biceps tendon, the extremity has been forthwith extended, and the cure thus effected in a fortnight. In bent fingers, resulting from rheumatic or other affections about the joints, the success depends upon rendering them moveable as well as straight; and in this I have been very successful in relieving and curing those who would otherwise have sacrificed the fingers to get rid of the nuisance, for such it is amongst the working classes. In bent toes, the treatment is the same, though they are more resisting and troublesome. In the worst cases, division of both flexors and extensors was found requisite.

"I have often reduced dislocations of long standing, which were only rendered reducible by division of shortened tendons: as, for instance, that of the great pectoral muscle in dislocation of the humerus: and likewise I have divided the tendon of the biceps in long-standing luxation of the fore-arm; or those of the flexor carpi radialis and ulnaris, in dislocation of the hand. In one long-standing case of dislocation of the foot forwards upon the tarsus, I was enabled, by division of the tendo achillis, to reduce the limb to its normal position. [These cases must be regarded as most interesting and valuable, whether we consider them as evidences of the bold practice of the operator, or in the light of precedents for future practice.] In spasmodic contractions of the upper extremities resulting from affection of the brain, considerable relief has likewise been obtained by division of the biceps and other tendons."

Art. XI.

Cases of Hydrocephalus, or Water in the Head; with Observations, and a Detail of a successful Plan of Cure. By J. F. Barnard, M.R.C.S. —*Bath*, 1839. 8vo, pp. 30.

Mr. Barnard has exposed himself, in the most marked way, to the severity of criticism, by the manner in which he has chosen to present this pamphlet to the world. To say nothing of the "popular" style of the title-page, what justification can he offer for totally suppressing all reference or allusion to authors who proposed and practised his "new and successful plan of cure" before the earliest of his cases was given to the world? Mr. B. cannot even plead ignorance of this fact; as the authors alluded to were severally pointed out to him in the Lancet of last year, in an annotation on one of his own communications. It seems hardly possible, after reading the whole pamphlet, to come to any other conclusion than that the author wished to make the public believe that his practice was both new and original, when he must know that it is neither the one nor the other.

In the notice in the Lancet (p. 376) both Riverius and Rosen are referred to as having recommended or employed compression of the head

in chronic hydrocephalus; but it is chiefly to the late Sir Gilbert Blane (also referred to in the Lancet) that we are indebted for calling attention to the practice in recent times, and for enforcing it both by precept and example. In the London Medical Journal for October, 1821, Sir Gilbert (who appears to have been unaware of the measure having been previously recommended by others) published a short paper "on the effect of mechanical compression of the head, as a preventive and cure in certain cases of hydrocephalus," which, like all the productions of that admirable writer, is marked by a happy admixture of ingenious but cautious theory and sound practical sense. As the notice taken of Mr. Barnard's pamphlet by the periodical press will probably call renewed attention to the subject, we shall here give some extracts from Sir Gilbert's memoir, previously to noticing the contents of the work before us.

Sir Gilbert, as already said, thought his proposal original, and justly regarded it as the more interesting, from its being one of the few practical truths that have been discovered on suggestions derived by physiological theory.

"In reflecting on the circumstances which characterize the history and description of hydrocephalus, some of the chief of which are, that it is very seldom met with but in very early life, and most commonly in infancy before the bregma is closed—that there is in most cases a preternatural size of the head, and that it is usually attended with a rachitic state of the bones and a general scrofulous flaccidity of the soft parts, and runs in particular families; it occurred to me that the distention of the head and bregma is owing to a want of firmness and due resistance in the bony *compages* of the skull, which consequently yields to that effort of pressure with which the brain in its growth acts on its parietes. In reasoning further on the subject, it appeared to me conformable to some of the most approved principles of physiology, that, as there is a certain degree of tension and pressure necessary to the sound condition and action of parts, the withdrawing of this, by inviting afflux and congestion, produces serous effusion; and that for the like reason, there may be a deficiency of that interstitial absorption upon which the healthy state of this and all other soft parts of the living frame depends.

"It was reflections of this kind, whether well or ill founded, which some time ago suggested to me that mechanical compression of the head might be of use in the cure of hydrocephalus, and which induced me to make trial of it in a case which occurred to me last year (1820). A child, aged thirteen months, had a head of a preternatural size almost from birth, and the bregma was preternaturally large. The conformation of the child was otherwise defective; for there was a visible curvature of the spine, indicating a rachitic diathesis. He had for several months been subject to drowsiness; and latterly it was evident, from his screaming, and from raising his hand to his head, that there were occasional paroxysms of headache. There had also been for some time past a dilatation of the pupils. The functions of the bowels were not so much disordered as is generally the case in this disease, which was still in an unformed state.

"I directed the head to be swathed with a roller, as tight as could be done without producing pain or uneasiness. The only other remedies were three leeches, applied once only to the temples and forehead, and a purge every two days of rhubarb and sulphate of potash: mercury was not used in any form. An immediate amendment took place, and continued, so that all complaint was removed in less than three months, except the curvature of the spine; and he has continued well till now, that is, for eighteen months.

"The only other case which has since occurred in my practice has been one which did not offer so fair a field for the trial. The subject was a child of three

years of age, with a head preternaturally large, and the bregma not yet closed. No symptoms of hydrocephalus had appeared, but only a state of general delicacy. The swathing has apparently been of benefit, and may probably prevent a disorder naturally to be expected from such a conformation."

This paper was reprinted in 1822, in the author's "Select Dissertations," with the addition of two short but very important communications from friends, confirming the success of the practice. The first from Mr. Costerton, of Yarmouth, and was clearly a case of constitutional hydrocephalus. The child was six months old, and the head had been enlarged three months. Dr. Girdlestone proposed the treatment to Mr. C. on reading Sir G. Blane's paper:

"Dr. Girdlestone wished me to try first, without the aid of medicine, the simple effect of pressure on the brain; and as I could not succeed in confining a bandage round the parts, I put a double strap of adhesive plaster round the head, which completely answered the purpose. The straps remained firm as they were originally fixed. The child gradually improved under the pressure; and the head, which was originally bald, began to be covered with hair, and to acquire more uniformity; and as the muscular strength of the abdomen increased, the ruptures disappeared. The teeth are cutting, the child is growing stronger, and may be said to be nearly well."

The other communication is from Dr. Thackray, of Cambridge, which we shall give in full:

"I am indebted to you for the idea of bandaging the heads of children, where that organ is larger than usual, and when the sutures do not seem ready to close in the usual time. In two instances I have seen great benefit from it. In one, where there seemed every proof of water being accumulated in the ventricles, the bandage seems to have effected a complete cure. The child (thirteen months old) had never been able to sit up, and was easy only in the horizontal posture, was seized with fever, screamings, and most horrid squintings, and very spinage-like stools, Leeches often repeated, and calomel in large doses, reduced the fever, but the squinting and inability to sit up continued. I now tried a bandage of adhesive plaster around the head, and almost instantly the *strabismus* left the child, and she has gradually progressed to a firmness of muscles, can now sit in the nurse's arms, and can bear to be danced about. She still wears the bandage, which has now been on above six weeks. I conceive the child owes its life to this practice."

It is curious that both his correspondents were led to adopt the use of adhesive straps in place of the cloth bandages proposed by Sir Gilbert Blane: this he states to be "a judicious improvement on his method."

In October, 1823, exactly two years after Sir G. Blane's first publication, the author of the pamphlet before us communicated another very interesting case in which the same treatment proved successful. This we shall give at length, as now republished:

"A child, about a year and a half old, was born to all appearance perfectly healthy, and continued so until six months old, when the head was first observed to increase in size. I did not see it until the disease was so far advanced that I almost despaired of its terminating favorably. The head was exceedingly large, weighing, I should think, nearly as much as two thirds of the rest of the body, and measuring in circumference twenty-two inches and a half. The child lay in a state of stupor, and was unable in the least degree to move its head. There was slight strabismus and a rolling of the eyeballs, and almost constant startings of the muscles of the whole body, but more particularly of the face. The countenance had a cadaverous appearance, and the skin was of a yellowish colour. The eyes were sunk in their sockets, and inclosed in a dark ring. The

flesh was flabby and seemingly hanging on the bones. The evacuations from the bowels were particularly unhealthy, sometimes green, sometimes blackish, but never of a healthy colour, nor indeed had they been healthy since half a year after its birth. The tongue was constantly covered with a thick white coat; when its head was moved it screamed, and seemed sensible of pain. I expected the child would survive but a few days. I therefore communicated my opinion to the parents, and told them the only chance I saw of saving their child was a plan which I shall describe, and which they readily assented to. I should say that it had been taking purgatives and mercurials without benefit before I saw it. I directed the head to be shaved perfectly clean, I then applied strips of adhesive plaster, about three quarters of an inch wide, completely round the head from before backwards, and so that the ends overlapped each other two inches behind, and covering the space from the eyebrows to where the hair commences, and as low down as the ears would permit; then, with cross strips, from one side to the other, over the crown of the head; and, lastly, one long strip, reaching from the forehead within half an inch of the root of the nose over the crown of the head, likewise to the nape of the neck. This gave effectual support to the parietes of the cranium. I ordered the whole head to be kept constantly covered with linen dipped in cold water, and that the child should take no other medicine than a little castor oil, should the bowels require it. Having thus decided on my practice, I patiently waited the result. Its good effects were evident; in less than a week the little patient could move its head much better, the squinting had disappeared, the secretions from the bowels were more healthy, and the startings of the muscles were less frequent. He had not screamed on rolling or moving the head since the bandage was applied. In a fortnight, the size of the head was reduced in circumference three quarters of an inch; the child was more lively and began to take notice of the persons around it; the secretions from the bowels were perfectly healthy, and evacuated regularly; the tongue nearly clean, and the skin of a natural colour; the countenance more composed and animated. Two months after the bandage was first applied, the child appeared in every respect healthy, but the head was still larger than it ought to be—measuring twenty inches and rather more in circumference; the flesh was firm, and the skin of a healthy mottled hue. The bandage was worn about two months longer, having been renewed about once a fortnight. The bones were then united, and the head firm, and the child well, only requiring time to bring its muscles into action which had been so long quiescent."

The practice detailed in this case is that adopted in all the other cases, with the omission of the cold water. Five more cases are detailed, characterized by similar symptoms and having the same favorable issue: and it would appear that Mr. Barnard's experience could furnish many more. "It would be useless (he says) to multiply cases which are so similar in their symptoms, progress, and cure, but here are sufficient to show the efficacy of a practice which I have never known to fail." Now, we think a tabular view of the cases, showing their nature, the name and age of the subjects, duration of the treatment, &c. &c., would have been by no means useless, and infinitely more satisfactory than the meagre and sweeping statement we have quoted. Mr. B. also refers to ten cases successfully treated by Dr. Engelmann, and one noticed in our last Number by Dr. Löwenhardt.

We are thankful to Mr. Barnard for lending his aid to call the attention of the profession to this very important mode of treatment, and we only regret that his merit in so doing is so greatly allayed by the unhappy manner in which he has put forth his experience.

Art. XII.

Report of the Malignant Fever, called the Pali Plague, which has prevailed in some parts of Rajpootana, since the month of July, 1836. *Prepared and published by order of the Government of India, under the direction of the Bengal Medical Board.* By James Ranken, m.d., Officiating Secretary to the Board.—*Calcutta*, 1838. 8vo, pp. 231.

We consider this an interesting Report of a destructive fever which first appeared in the principality of Joudpore or Marwar, at Pali, a considerable town, reckoned the emporium of the trade between Central India and the seaport of Guzerat. This fever was regarded as a "nova pestis" in the region in which it appeared, and, by the majority of observers, it was considered identical with the plague of the Levant. From this latter opinion, Dr. Ranken departs, and, as it appears to us, on sufficient grounds.

The following is Dr. Ranken's picture of the epidemic, drawn, as he expresses it, from the descriptions of all the medical observers:

"No sense of indisposition gives warning of the approach of this disease. It comes on with slight rigour, headache, nausea, and pain in the loins. The skin soon grows hot and dry, the pulse from 130 to 150 in the minute, is soft and easily compressible. The tongue is variously covered with white, light brown, or darkish fur, and these colours are sometimes intermixed. Vomiting and irritability of stomach occur, though rarely. The bowels are bound; the abdomen tumid and full, is seldom painful on pressure. The eyes appear heavy, maudlin, and bloodshot, and occasionally look as if injected with lake. The countenance is expressive of anxiety and inward pain, the respiration apparently unaffected by the predominant malady, is often impeded by concomitant inflammation of the lungs. Glandular swellings appear in the groins, armpits, and neck, most frequently on the left side, as also under the jaws and ears, and in the upper part of the thigh. They are generally perceptible on the first or second day, and rarely increase above the size of a walnut, but in some instances they become much larger, burst, and discharge purulent matter. The symptoms are sometimes all so mild, that the sick keep walking about till they gradually recover. In most of the cases observed, there was a visible abatement of the disease, every twenty-four hours, towards morning; but in the worst forms of it, intense fever continued night and day, without any remission; the patients could not rise from their cots, on account of extreme debility, and an attempt to raise them to the erect posture produced fainting. Hemorrhage from the lungs took place in a few, and was much dreaded. Delirium occurred rarely at the beginning, but coma generally supervened shortly before death. When violent, the malady ran its fatal course in three days; when mild, with or without the affection of the glands, it was protracted to fifteen or twenty days, like the ordinary fevers of the country." (pp. 15, 16.)

These symptoms (independent of discrepancies of no essential moment) constituted, we are told, in every place visited by medical officers, the Pali disease. Dr. Ranken proceeds to examine whether they suffice to determine that the disease was the real plague; and concludes in the negative, for the following reasons:

1st That most of the external appearances, which Russell and other original observers account pathognomonic of the plague, carbuncles, wheals (vibices), spots, and lividness of parts of the body after death, seem never to have been met with in this disease; and

2dly. That buboes, and other glandular tumours, so far from being

certainly diagnostic of the plague, do not appear at all in many marked cases of it, and these the most fatal; and that, moreover, such glandular affections were observed by Drs. Cheyne and Barker in the typhus, which destroyed so many of the lower orders of Irish between 1816 and 1819; by Dr. Rush, in the bilious yellow fever of Philadelphia in 1793; and, as Dr. Ranken informs us, are common in the destructive fever which has arisen occasionally in the remoter parts of the mountainous district of Kumaon for these last fifteen years, and which was particularly fatal in 1834 and 1835.

Any evidence for the foreign origin of the disease, and for its importation into Hindostan, derived from its correspondence in distinctive character with the plague of the Levant, being thus disposed of, the following very refined piece of speculation, to which the annals of the controversy on contagion could furnish many a parallel, receives all the investigation it merits:

"Zerawar Mull, the great loan contractor, banker, and merchant of Rajpootana, having gone to Guzerat, it was alleged, in prosecution of his multifarious business, returned with merchandise which had come from Bhaonuggur or Surat, and stopped several days at Pali, just before the appearance of the fever. Hence the inference confidently drawn, that as those seaports receive goods from the Persian Gulf, the Red Sea, and from Egypt itself, the home of pestilence, Zerawar Mull must have brought the plague to Pali. This wealthy personage, however, it turns out, was religiously and not commercially engaged in a pilgrimage to the temple at Teeruth. He neither bought or sold throughout his journey, and came back perfectly well. Besides, the importation of any articles whatever, from pestilential countries, has not been ascertained in this case: no unusual sickness existed at Bhaonuggur, Surat, or in the provinces connected with them; and the foreign merchandise, purchased in their markets for many parts of western, northern, and central India, was not even suspected of communicating disease anywhere, except at Pali. But numerous small traders, we were again told, availing themselves of Zerawar Mull's escort, came in his train to the emporium of Marwar, and though in perfect health themselves, they might confidingly have purchased unopened bales of Egyptian cloth at Bhaonuggur, and disposed of them to equally unsuspecting retail merchants at Pali, who being the first to unpack the tainted bales, would be the first to suffer from the foreign contagion. There is reason to believe that the only cloths received at the period in question were English cottons; but, at all events, the original purchasers escaped at the opening of the packages, and the printers, who must have subsequently got the pieces to ornament, were the earliest victims of the fever. I need not notice more allegations of the same kind, which nobody would now advance. So long a chain of improbabilities has seldom been devised to support an untenable hypothesis on any subject." (pp. 29, 30.)

Our reading has shown us, that "chains of improbabilities," such as this, are not so rare in controversies on the etiological branch of our art, as Dr. Ranken seems to imagine; but we trust that the diffusion of knowledge, by works like his own, will render them more rare than heretofore, and finally extinguish them.

Dissatisfied, as well he might be, with such an hypothesis on the origin of the disease as the foregoing, the author seeks, and has no difficulty in discovering, endemial causes, capable, under certain atmospheric conditions, of engendering pestilence in abundance. The following passage we consider to be written in the true spirit of medical philosophy:

"The people of the north-western provinces of British India, and of Marwar

more especially, are to be considered in varying stages of transition from anarchy and want to comparative plenty and order. They have advanced within reach of many improvements in the physical condition of society; but having made no corresponding acquisitions in intelligence, their minds retain the impress of former barbarity and misrule, and are yet unable to turn the advantages of a better system to account. Hence, animal instinct predominating over foresight and enterprize, population has augmented faster than increasing agriculture and commerce have extended. The hut, or one of the same dimensions, which was the unwholesome den of four persons, for example, fifty years ago, now shelters six human beings, within a space barely sufficient for the accommodation of one. These miserable habitations are often inclosed by an outer screen or wall, for the double purpose of confining the cattle and secluding females from the public gaze. Glass to let in light, or apertures to admit fresh air being unknown, the door, in order to keep out heat at one season and cold at another, is generally shut. The poor feel most at home in such dark places with their children lying round them on the floor, too much like hogs in a sty amidst their litter. The sense of insecurity for life and property, which deterred their ancestors from living near the fields which they cultivated, in farm-houses and cottages scattered over the face of the country, owing to hereditary habits of thinking, still perceptible to this day under the very cannon of Fort William, makes the peasantry everywhere accumulate their hovels within the narrowest limits, and the consequence is, that each dirty village, unventilated, and overcrowded with man and beast, exhibiting every sort of nastiness, is a focus of disease.

"The most obvious means of raising food for the augmenting numbers of inhabitants, involve the cutting down of jungle, the breaking up of waste lands, and the irrigation of new fields for grain, all of which are known by lamentable experience to occasion noxious exhalations. These operations, the necessary attendants on the establishment of a new station, which is a sort of European colony in our provinces, added perhaps to the injudicious choice of a locality, almost in every instance render it unhealthy at first." (pp. 35, 36.)

The reports of the medical officers, who visited the different parts of the infected district, all confirm the general abstract of its condition given by Dr. Ranken.

The author ascribes to malaria the origin of the disease, but admits that human intercourse, or, in other words, contagion, was instrumental in its diffusion. This contagious influence he seems to view as rather contingent on circumstances, such as crowding, imperfect ventilation, and filth, than as abstractedly potential; and hence he remarks that the epidemic depopulated the cottage, but spared the palace. These circumstances, however, existed in abundance in the wretched Indian villages, the original over-crowding being aggravated by the fugitives from Pali, regarding whom it is remarked, that wherever they took up their residence, the fever shortly afterwards appeared. It might be reasonably questioned, whether this influx of strangers, though from an infected district, did not tend to diffuse the disease rather by multiplying sources of malaria than by any direct transmission from individual to individual. The efficacy of certain sanatory cordons, in preventing the diffusion of the disease, furnishes stronger evidence of contagion than any circumstance connected with it.

We consider Dr. Ranken's report a very excellent one, and as bearing a very important and confirmatory relation to Dr. Bowring's paper on plague and quarantine, noticed in our Fifteenth Number.

PART SECOND.

Bibliographical Notices.

Art. I.—*Practical Observations, showing that Mercury is the Sole Cause of what are termed Secondary Symptoms.* By P. J. Murphy, m.d.—*London*, 1839. 8vo, pp. 107.

The substance, or rather the shadow of Dr. Murphy's book, is the following doctrine: "That ulceration of the throat, cutaneous eruptions, iritis, nodes, venereal swelled testis, &c., and termed secondary symptoms, are produced solely by mercury." (p. 103.) The title-page sets forth the same notion, but the book itself, somewhat strangely, contains no proofs of its truth, unless the author's earnest conviction and some loose reasoning, can be called proofs. Dr. Murphy seems to have been unconscious that the truth of such a theory can only be established by facts, and ignorant that a large number of facts utterly contradict it. Thus, within the last few years, 80,000 cases of primary ulcerations of the genitals have been submitted to anti-mercurial treatment in France, Sweden, and Hamburg, "and the proportion of secondary affections, where the primary symptoms have been treated without mercury, is, at its lowest estimate, reduced to ten, at its highest to twenty, in the hundred." (*British and Foreign Medical Review*, Vol. V., p. 7.) In this country, we thought everybody knew that similar experiments were made in our own army, twenty years ago. From the circular, published by the Army Board, dated 2d April, 1819, it appears that in 1940 cases of primary ulceration of the genitals, occurring between Dec. 1816 and Dec. 1818, treated without mercury, 96 cases of secondary symptoms occurred: during the same period, 2827 cases of ulceration of the genitals were treated with mercury, out of which but 51 had secondary symptoms. Much more evidence of a similar kind could be adduced; but this (which is of the strongest kind, affirmative, not negative), sufficiently proves, that secondary symptoms do occur, when no mercury whatever has been given; and thus Dr. Murphy's notion, that secondary symptoms are produced solely by mercury, is totally disproved. We would suggest to Dr. M., before he theorizes and writes again, to make himself well acquainted with the history of the disease he undertakes to elucidate, and with the opinions and practice of others. This he has not done in the present instance. We have no inclination to criticise more minutely his book, lest we should ourselves run the risk of being classed with those modern critics, who have been described as "butchering the defenceless rabble of the lame and halt, who venture with such courage, in our days, into the literary tilt-yard."

The propriety of the non-mercurial treatment, which Dr. M. recommends, does not depend on the truth of his hasty generalization. The absolute *necessity* for mercury, in the majority of cases, has been disproved; the question still remains open as to its *expediency*.

Art. II.—*Nuovo Trattato completo della Gotta in genere, e sue specie, cioè Podagra, Chiragra, Gonagra, ed altre Malattie Gottose.* Opera di Giuseppe Ciani, Medico Romano, Dottore in Filosofia e Medicina, &c. Tom. I.—*Roma*, 1838. 8vo, pp. 212.

A new and complete Treatise on Gout, and its Varieties—Podagra, Chiragra, Gonagra, and on Gouty Diseases in general. By Joseph Ciani, Physician in Rome, Doctor of Philosophy and Medicine, &c. Vol. I.—*Rome*, 1838.

This is a work which comes into the world at least a century too late: it should have appeared when mere hypotheses, the work of imagination, were received with equal reverence as generalizations of facts, or rational theories legitimately deduced from them. Although it cannot be received as a fair sample of the general mode of philosophizing now prevalent among physicians in Italy, it must be admitted, that its visionary character will make it appear less singular, and cause it to obtain more countenance from learned men in that country than would be the case in England, or even in France or Germany. However, while our own literature is disgraced by the writings of Dr. Ramage and Dr. Dickson, regularly-educated physicians, we are not justified in regarding the absurdities of Dr. Ciani as national, but merely as individual and personal delinquencies.

Dr. Ciani's theory of gout is, he says, altogether new, never having been previously imagined (è del tutto nuova, non fu da alcuno sinora ideata), although it is evidently a spurious breed between the physiological or Broussaian and the modern chemico-medical doctrines. There must be, he says, certain predisposing conditions in gouty subjects before the causes of the disease can take effect; and these conditions are a sanguineous or sanguineo-bilious temperament, a laxity and atony of the muscular system, a plethora *ad vasa* of the venous system of the abdominal viscera, and, lastly, a superabundant accumulation of caloric in the same. If in subjects so predisposed, the digestive processes become disturbed from excessive or too stimulant diet, a superfluous quantity of the phosphate and subphosphate, and also of the carbonate of lime is generated, and not being absorbed from a deficiency of solvents (the phosphates more particularly), they are deposited on the mucous membrane of the gastro-intestinal tube, where, becoming also associated with other heterogeneous principles, they excite an irritation which ends in a true phlogosis of the same membrane, which phlogosis is the more vehement from the previous hypersthenic condition of the affected parts. Sometimes the disease proceeds no further, but remains a circumscribed gouty gastro-enteritis, in which case it constitutes the first variety—*idiopathic gout;* sometimes, the primary inflammation remaining, there is excited, by sympathy, inflammation of the joints, and this is the second variety —*sympathetic gout;* lastly, the primary gastro-enteritis entirely disappearing, is transferred to the joints, or some other part—and this is the *metastatic gout.*

We know not if it will suprise our readers—it certainly did not at all surprise us—to learn, that Dr. Ciani adduces no proof whatever of the presence of his phosphates in the alimentary canal in gout, or of the

presence of the primary inflammation said to be excited by it; all is assumption and presumption from begining to end.

The author's therapeutics, as may be supposed, are in due accordance with his theory. The *lactic acid* being the special solvent of the phosphate of lime (the last being only deposited on the intestinal mucous membrane, from defect of the former in the chyme or chyle), becomes, consequently, the special or specific cure of the disease originating in this. "Thus," he says, "eschewing empiricism, and subjecting the gouty to my scientific plan, supported, as it is, by reasoning and confirmed by experience as not merely innocuous but beneficial, we not only need apprehend no sinister result, but may promise a certain cure." But it would be doing injustice to Dr. Ciani, if we did not state that his scientific plan includes much more than the administration of the lactic acid. Under the head of the radical cure of gout, he judiciously prescribes the important hygienic means, of change in the general mode of living, rigorous sobriety, and constant bodily exercise. In the application of his lactic acid, too, for the palliative cure, he requires rigid indications, holding it to be absolutely necessary, before the physician prescribes, that he should be satisfied of the precise cause of the disease in the case before him, that is, whether the gouty matter deposited is the free phosphoric acid itself in excess, the phosphate of lime, or lime by itself. This, however, may seem easier said than done; but Dr. Ciani sees no difficulties in the case, regarding the presence of a sense of heat in the stomach, thirst, flatulence, &c., as indicating the presence of free phosphoric acid, and the absence of these as announcing the existence of the others! We will not trouble our readers with the varieties of the modes of treatment founded on these various indications, but conclude with a brief account of the mode of administering the grand specific—the lactic acid. This is given in the form of lemonade, made by dissolving half a drachm of the acid in eight ounces of water, and sweetening it with six drachms of syrup. This quantity the patient takes every morning fasting, and, again, after supper; and after dinner and supper, he also takes three or four pastilles, prepared with the same acid, and which he allows to dissolve slowly in his mouth. This practice is to be continued, uninterruptedly, for *several years*, with the single exception of omitting the pastilles during the months of July, August, and September, and taking, during their omission, two drachms of cinchona (china), in a little water, before dinner, daily. And thus pleasantly does Dr. Ciani cure his patients, after having pleasantly cured himself of a gout of ten years' standing. We recommend his erudite treatise and his agreeable lemonade to the particular attention of Sir Charles Scudamore, and all other fashionable gout-doctors who may do us the honour to look into our pages.

ART. III.—*The Modern Treatment of Syphilitic Diseases, both primary and secondary: comprising an Account of the New Remedies, with Numerous Formulæ for their Preparation and Mode of Administration.* By LANGSTON PARKER, M.R.C.S.—*London*, 1839. 12mo, pp. 158.

FROM the title of Mr. Parker's work, "On the Modern Treatment of Syphilitic Diseases," we had expected a summary of the opinions of British as well as Foreign surgeons on this important point; but the

author has almost wholly confined himself to the treatment adopted at present by the French. This task Mr. Parker has accomplished most creditably. His book is carefully compiled, concise, clearly expressed, and well adapted to put the English reader in possession (in a short compass) of the opinions and practice of the principal surgeons attached to the venereal hospitals of Paris. Cullerier, Desruelles, and Ricord are the chief authorities quoted, also the late Dr. Wallace, of Dublin; at the same time, Mr. Parker has "added the result of his own experience to the weight of others." We had marked for extract Mr. Parker's excellent account of M. Ricord's experiments on the inoculation of pus from venereal sores, but we are constrained, by want of room, to omit them. They add considerably to our precise knowledge of the natural history of the disease, and merit the attention of all surgeons.

Mr. Parker himself is inoculated with the French love of leeches. They are to be applied to the centre of venereal ulcers, to their thickened edges, to the glans on each side of the prepuce in gonorrhœa, indeed to all manner of places which have usually been avoided, and on this account seem to have been selected. "The only objection," says Mr. P., with much naïveté, "to this practice, is the inoculation of the leech-bites, and, consequently, the formation of fresh phagedænic sores." (p. 97.)

For details of modern practice in general, this work is a useful guide; it contains numerous formularies for the new combinations of mercury and iodine, and other recently-introduced medicines. Some valuable practical hints may be gathered from it, on the expediency of the mercurial treatment.

In the French venereal hospitals, mercury is not used in the treatment of primary venereal sores (p. 12); but yet Ricord, Cullerier, and Desruelles, surgeons of those hospitals, admit the superior efficacy of mercury in hastening the cicatrization of a primary venereal ulcer, and in diminishing the risk of secondary symptoms. They are all three partisans of the simple treatment (the non-mercurial); but they recommend mercury "when the sore is indolent, does not cicatrize under the simple plan; when its edges are hard and elevated, or the sore leaves behind it, in healing, an indurated cicatrix." We believe that this is much the practice of judicious surgeons in this country; it is certainly that which is recommended by the highest surgical authorities. The simple plan, we believe, has been confined almost to hospitals, where the patients can be compelled to rest and low diet; but in private practice, where this is often impossible, all surgeons have found the necessity of some more active measures; and mercury, although it may be denied by some to be a specific against syphilis, yet is allowed by all to be one of the most powerful therapeutic agents we can employ.

If a mere routine practice is pursued, without any reference to the kind of sore, it is not improbable, that less harm will be done by the non-mercurial treatment than the mercurial. Riddled and honeycomb skulls, looking as if they had been eaten of worms and gnawed by dogs, are now, fortunately, rarely to be seen, except in the glass-cases and drawers of surgical museums, or in Cheselden's plates;—venerable relics of a past age, out of date, like the silk coat and full ruffles of the contemporaneous doctor. These were the consequences of a profuse abuse of mercury, which is now rarely attempted. But still, simple excoriations, herpes

præputialis, and superficial sores, the joint product of abrasion and want of cleanliness, are often treated by mercury; the healthy constitution being thus cruelly subjected to the long-continued action of a powerful poison. Such instances have often fallen under our own observation, and are doubtless common. There is another abuse, which is perhaps more common, from mercury being given irregularly, or in insufficient quantity, without any attention either to the habit of body, which may be either weak or scrofulous, and without precautions in preserving the patient from wet and cold. An obstinate disease is thus generated, which John Hunter, with great clearness and brevity, has defined as the joint effect of the poison, the constitution, and the remedy. By the routine adoption of the non-mercurial plan, these cases would not occur; and if we may believe the experiments, although secondary symptoms would follow more frequently, they would be less severe and intractable. But this supposes the abuse of mercury, not whether its judicious employment, due regard being had to the kind of sore, the constitution, and to precautionary measures, is not, as a general rule, the most expedient in practice. We believe that it is, and that in private practice especially, no other can be adopted.

Art. IV.—*Tea; its Effects, Medicinal and Moral.* By G. G. Sigmond, M.D., F.S.A., F.L.S., Professor of Materia Medica to the Royal Medico-Botanical Society.—*London*, 1839. 8vo, pp. 144.

The progress of civilization, in the course of which we have learned how to poison ourselves with opium, or to end its attendant happiness by hanging and drowning, has taught us to employ atmospheric pressure, by means of the stomach-pump, and the use of artificial respiration; and the same civilization, bringing with it various new and minor ills incident to humanity, has, for many of these, supplied the antidote of tea. Manifold are the opinions, both public and professional, as to the utility and wholesomeness of tea; and various the preference given to black or green. Cobbett is said to have asserted, that he never enjoyed health until he renounced the "slop kettle," and betook himself to good beer; the student constantly finds that tea is essential to any available activity of his brain; tea relieves the languor and drowsiness of the habitually heavy feeder, and somewhat diminishes the intoxicating effects of a little too much wine; washerwomen and card-playing old ladies depend on it for much of their success in life; and so do thousands besides, whose fatigues or excitements are beyond their natural or acquired capabilities of endurance, and who find in tea the real elixir vitæ, if not the vital principle itself.

The professional opinions on the propriety of tea depend on various causes, and in part, perhaps, on the doctor's own organization. The hard, phlegmatic doctor, whose nervous system was omitted in his constitution, believes that tea is a great mistake: this is the man, without imagination or reflection, who pins his faith on lecturers and books, and, because it was the fashion in the day when he had the misfortune to gain the little knowledge or ignorance which he has acquired, he still sees "diseased liver" in all human ills; and tea is "bilious" in other people's

stomachs, and in his eyes. Another class approves of something more warming, and considers the appetite for drinks to have been given for a wiser purpose than to satiate it on tea; to these, the taste acquired in their students' days has grown with their growth and strengthened with their strength, and they are well acquainted with the merits of good Cognac. Their prescriptions, doubtless, abound in tinctures and essential oils. Others condemn or approve the use of tea, because they find it injurious or beneficial to themselves, without sufficiently reflecting on the circumstances which render it good or bad. There is, besides, an eclectic class, happily the largest.

It is probably correct to say, that, considering that civilized mankind is a machine generally used in a manner but little consistent with health, tea is one of the oils of its worn and rusted wheels. Had we to write a book on the moral and medicinal effects of tea, we should first endeavour to ascertain what we mean by tea. An examination of the moistened leaves of our teapots, the tea having been bought of sellers of the ordinary reputation, often reveals the fact that it is not an infusion of the tea-leaf alone which we have had the pleasure of drinking. The poorer classes constantly drink, under the name of tea, a beverage, dark, offensive, and, without a large qualifying addition of sugar, intolerable. This is frequently prepared from what is called "tea-dust," the powder of the tea-chests, with the addition of some unknown dirt. By many, also, this vegetable matter (if vegetable it be) is boiled, in order to extract from it all its virtues. Others, again, occasionally obtain the well-infused leaves, which would otherwise be employed by the housemaid in laying the dust on the floors, and we have heard of the infused leaves having been dried and re-employed. In these, and in various other ways, it is very difficult to ascertain the nature of the fluid which finds its way into the bowels of the rich and poor under the name of tea; and it is, consequently, a very difficult matter to write anything satisfactory on the subject. Had we to write a book on the moral and medicinal effects of tea, premising and making proper allowance for the difficulties and doubts above alluded to, we should endeavour to gather our information from all quarters, from the healthy and unhealthy, the rich and poor, the young and old, studious and empty-headed, idle and laborious, temperate and the reverse; and we should scarcely think we had gained much that was worthy of publication, until the information which was thus extensively derived had been judiciously and carefully prepared. Dr. Sigmond's book has entirely disappointed us. Its title is attractive, its contents the reverse. We eagerly sought for information, and, except in its extracts from other works, we found scarcely any. We were prepared for good Souchong or Pekoe, and all that was offered us was exceedingly weak Bohea. As in duty bound, we drank the Bohea, but really cannot recommend to our readers a similar draught. There was scarcely a lump of sugar in the cup.

The following remarks are worthy of attention:

"Tea, as a morning beverage, when breakfast forms a good substantial meal, upon which the powers for the day, of meeting the various changes of life, depend, provided it be not too strong, is much to be recommended; but when individuals eat little, coffee certainly supports them in a more decided manner; and, besides this, tea, without a certain quantity of solid aliment, is much more

likely to influence the nervous system. Some persons, if they drink tea in the morning and coffee at night, suffer much in animal spirits and in power of enjoyment of the pleasures of society; but if they reverse the system, and take coffee in the morning and tea at night, they reap benefit from the change; for the coffee, which to them in the morning is nutrition, becomes a stimulus at night; and the tea, which acts as a diluent at night, gives nothing for support during the day." (p. 111.)

Those who suffer from the feelings termed nervousness, and who are in the habit of adding spirits to their tea for their relief, are recommended "to renounce tea altogether, and to substitute for it a beverage, half coffee, half warm milk, and, if possible, to acquire the habit of taking a substantial breakfast, which alone can dissipate this symptom of uneasiness." (p. 112.) The evil effects of green tea, on certain idiosyncrasies, are forcibly pointed out; and we believe that its effects upon the nervous system, when taken as a strong infusion, are, very generally, decided and injurious. There is some little inconsistency, in Dr. Sigmond's facts, in favour of the total abstinence from fermented liquors, and in his subsequent commendations of a little good wine or beer. For our own parts, on this subject, we doubt the majority of facts, as they are called. Did we believe them, we could not, as we heartily do, agree with the author, that "good wine is a good cordial, a fine stomachic, and, taken at its proper season, invigorates mind and body, and gives life an additional charm. There can be found no substitutes for the fermented liquors, that can enable man to sustain the mental and bodily labour which the artificial habits of society so constantly demand. . . . . If, on the one hand, disease and sorrow attend the abuse of alcoholic liquors, innocent gaiety, additional strength, and power of mind, and an increasing capability of encountering the ever-varying agitation of life, are amongst the many good effects which spring from a well-regulated diet, in which the alcoholic preparations bear their just proportions and adaptation." The argument against the use of spirits, which we have commonly found to be employed, is the argument against their abuse. And looking medically at the question, the same arguments would lead to the conviction of the propriety of total abstinence from purgatives or bleeding, or any other mode of treatment of well-ascertained efficacy.

We have not said that Dr. Sigmond's book is chiefly occupied by the natural and commercial history of tea. It is beside our purpose to enter at all into the subject, but Dr. Sigmond's account of it is well worthy of perusal.—Although we are not in the habit of alluding to theological matters in our pages, we cannot abstain from asking a question, prompted by Dr. Sigmond's curious dedication,—what are the "intellectual qualifications," which not only "give to existence its richest charm," but "promise happiness in the life to come?"

Art. V.—*Outlines of Military Surgery.* By Sir George Ballingall, M.D., F.R.S., Regius Professor of Military Surgery in the University of Edinburgh. Second Edition.—*Edinburgh*, 1838. 8vo, pp. 542.

This is an improved edition of a work which ought to be in the hands of every naval and military medical officer. It is divided into three parts: "The first embraces numerous topics connected with the formation, discipline, and economy of armies. . . . The second com-

prises those surgical accidents and diseases peculiarly incident to military and naval men. . . . The third embraces the consideration of the most important diseases incident to troops on foreign stations." Of these departments, the second is by far the most important and complete. It forms one of the best practical manuals of all the important *incidents* of surgery anywhere to be met with; and contains the most recent and best information on all the subjects treated of. The first division is also full of interesting matter, essential to be known by the young officer. The last part is the most imperfect, and might, almost, be omitted without loss. If the author wishes to improve the details in this branch of his subject, he will find invaluable materials in Major Tulloch's Reports.

ART. VI.—*Practical Observations on the Causes and Treatment of Curvatures of the Spine.* By SAMUEL HARE, Surgeon.—*London and Leeds,* 1838. 8vo, pp. 151.

THIS is a handsome volume; and the plates, exhibiting, it is said, exact portraits of various incurvations and the ameliorations produced by treatment, are really beautiful. Mr. Hare's book is, however, by no means a scientific one, and claims little notice at our hands. It contains a description of certain machinery for the remedying of spinal curvatures, which is said to prove most serviceable in the author's hands, and narrates a few cases which attest the success of Mr. Hare's particular management. We cannot, however, say that the work before us has added anything of the least value to our previous knowledge.

ART. VII.—*Elements of Physiology.* By J. MÜLLER, M.D., Professor of Anatomy and Physiology in the University of Berlin, &c. *Translated from the German, with Notes,* by WILLIAM BALY, M.D. Second Edition, with Corrections and numerous Additions. Part I.—*London,* 1839. 8vo, pp. 471.

WE do not notice this new edition of Dr. Baly's translation of Müller's classical work, with the view of analysing, or even criticising it, but merely for the purpose of recommending it to such of our readers as are still unacquainted with it. It is a production of such uncommon excellence, that, whatever difference of opinion may exist among physiologists respecting certain views and doctrines advanced in it, there can be only one as to its unequalled merits as a system of physiology for the advanced student. And this character applies to it better in its present than in its original form; inasmuch as the translator has not only preserved all that is of value in the German edition, but has placed several things in a better point of view, and has added many others of no small importance; while he has contrived, throughout, to achieve the very difficult task of giving a faithful version of the original language, in a perfectly good English style. The appearance of this new edition of the work, at this early period, is in itself a proof of its intrinsic value, and of the merits of the translator and editor. The present Part contains, General Physiology; the Blood and Circulating System; the Lymph and Lymphatic System; Respiration; Nutrition, Growth, and Reproduction. We recommend the volume, in the strongest terms, to all our readers, and to every student of physiology.

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

Further Experiments in the Inoculation of Animals with the Blood of Cholera Patients. By Signor Novati.

In our Ninth Number we gave an account of a series of experiments performed by Dr. Namias, of Venice, upon dogs and rabbits with the blood of cholera patients, in almost all of which the disease appeared to be communicated, and was followed by the death of the animals. These experiments have been repeated by Dr. Campelio Calderini, of Milan, with nearly similar results; by Drs. Borsani, Freschi, and Novati, of Bergamo, without producing any injury to the animals; and upon four rabbits, four guinea-pigs, and two dogs, by Dr. Semola, of Naples, with no greater effect; and a commission of enquiry was instituted by the Royal Academy of Sciences to repeat these experiments, and report upon them. This was done upon eleven animals, rabbits, guinea-pigs, and dogs, and the result having been the same, the original Memoir of Semola has been inserted in the acts of the academy, and in the civil annals of the kingdom. Previous to this, however, Dr. Novati, not being satisfied with the first series of his experiments, resolved to repeat them upon a larger scale. The following are the results:

Two inoculations performed upon rabbits with the blood of living cholera patients produced no effects. Of twenty-one rabbits inoculated with human blood taken after death, eleven died between three and seven days afterwards, two of them having been inoculated with the blood of a fœtus of eight months, whose mother was affected with the disease; and the greater part of the survivors exhibited symptoms that, if not pathognomonic, at least are always observed in cholera. Of twenty-one other rabbits inoculated with the blood of one of the subjects of the former experiment, nineteen died between the first and fourth day, and the two others suffered in the same manner as the survivors from that experiment; thus proving, as had been before shown by Dr. Namias, that transmission through several individuals does not weaken the effect of the poison; and that it is more powerful when communicated from one animal to another of the same, than of a different class. The blood of a living cholera patient was injected into the mouth of one rabbit, and into the rectum of another, but neither of them experienced any ill effects. Another made to swallow some blood from a corpse, did not suffer more than it might have done from the mere operation.

Blood from a patient and a corpse was injected into the veins of two rabbits, the former died upon the spot, the latter in thirty hours; and the blood of the former rabbit injected into eight others destroyed seven of them very speedily. Another, whose femoral vein had been tied, was inoculated on the inside of the thigh without injury.

Fluid human dejections were inoculated with a vaccination needle, without

effect; but those of a rabbit, inserted into incisions in the skin of three others, destroyed two, one in twelve hours, the other on the third day, the third rabbit escaping with difficulty.

A large rabbit was enveloped in the shirt of a cholera patient imbued with perspiration, but, except extreme prostration, the animal suffered no bad effects. Guided by the analogy of the effects of arsenic and the cholera, Novati mingled the tritoxide of iron, the newly-discovered remedy against this poison, with cholera blood, and injected it, when it caused death in nineteen hours. Arsenic itself produced death in a still shorter time. Common salt mingled with cholera blood was fatal in sixteen hours.

Besides these experiments, Sig. Novati performed many others upon rabbits, by wounding them severely, and injecting the blood of persons affected with various diseases, but without ever being able to produce similar effects. We have only given the results, but every minute circumstance connected with the experiments is noted in the original memoir.

Annali Universali di Medicina, 1838. Vol. lxxxv.

Petrifaction of the Tissues.

The account which was given in our Number for Oct. 1836, of the conversion of the tissues of the human body into a substance of a strong hardness, by a mode of preparation discovered by Signor Segato, of Florence, is fully confirmed by M. Moreau, in the *Journal des Connaissances Medico-Chirurgicales.* Segato died three years since, without having revealed the secret of his art; but a large collection of his works is still exhibited in Florence. It consists of a great number of entire and variously dissected animals, fish, reptiles, birds, and small quadrupeds; of different organs, as the liver, spleen, placenta, brain, heart, &c.; of dissected limbs, clots of blood, &c. All the animal substances are converted into a material so hard that it can scarcely be filed, but will receive as fine a polish as marble, or any hard stone. Their colour, form, and size are unaltered; the minute vessels can be traced in them; and, but for their hardness, they might be taken for the real parts recently removed from the body.

Journ. des Conn. Med.-Chir., Avril, 1839.

[Signor Segato's process is not new, though he has employed it to a greater extent and with more success than Ruysch, to whom the same or a similar mode of preparation was well known. In his answer to the second letter of Gaubius, (*De artificiosa Scroti Humani induratione.* Sc. Op. omn. vol. i.,) Ruysch says, "You ask me to tell you of the preparation in which the scrotum is hardened like a stone. That is done by an artifice so singular that I laboured at it for more than four and thirty years. I hold nothing so difficult as to prepare parts of the human body, and especially the scrotum, so that their natural colour and size being preserved, and no unnatural wrinkles being formed, they should be hardened like stone, and yet that the course of their vessels should be far more distinct than in a living or in a recently dead body. But I cannot yet comply with your wish, nor can I be induced to make that preparation publicly known; and I am sure you will not require it of me when you consider with me how many there are who, like the Æsopian crow, delight to boast in others' plumes."]

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Lithotripsy performed by the water of the Royal Fountain of Recoaro. Memoir presented to the Society of Sciences in Modena. By Professor Brera.

This memoir contains three cases of the lithotriptic powers of the above waters, not in producing the dissolution of the calculi, but such a weakening of their cohesion, that they passed off with the urine either in fragments or in the form of fine sand. The quantity of water taken was two pints every morning, and it was continued for two or three months, or as long as the attack lasted, where there was no perfect calculus formed. In two cases there was chronic

inflammation of the bladder, together with a loss of its contractile power in the third, from which the patients perfectly recovered by the use of the water. The calculi were of the lithic acid kind in each case. No analysis of the water is given, but the following analysis by Peregrini, of the water of St. Catherine's well in the Valtelline, is said to resemble that of Recoaro:

Besides much carbonic acid it contains

Sulphate of soda - - - -	50·85
Chloride of sodium - - - -	28·63
Sulphate of magnesia - - -	9·83
Carbonate of do. - - - -	53·17
Bicarbonate of zinc - - -	778·89
Silicate of alumina - - - -	36·06
Carbonate of iron - - - -	277·03

The quantity of water analysed is not mentioned.—*Omodei, Annali*, vol. lxxxvi.

Remarkable Tendency to Hemorrhage in a family. By Dr. DU BOIS, of Neuchâtel, Switzerland.

THIS affection, which consists of an extreme fluidity of the blood, or a weakness of the capillary vessels, which are ruptured by the slightest violence, and have but little contractile power, is not uncommon in the west of Germany, and in the Rhenish provinces. It is hereditary in a striking degree; though only males are affected by it, to whom it is frequently transmitted by their mother, who is free from its influence. In some families scarcely a single male arrives at maturity, from this cause; and the person thus diseased bears the significant name of *Bluter* or Bleeder.

A robust gardener of Neuchâtel married a stout, healthy woman from Nassau, in whose family, according to her account, no Bluter had been known. By her he had a family, consisting of five boys, and one girl who never exhibited any symptoms of this complaint, and died in convulsions when three years of age. Of the five boys, one died of convulsions on the day of his birth; three died of hemorrhage; and the last, now seven years old, will probably soon follow his brothers, from the same cause. The symptoms exhibited the following course in all. A fortnight after birth, which was natural, ecchymoses began to appear in different parts of their bodies, spontaneously, or from the slightest pressure, and slowly disappeared, leaving a yellow tint behind them. About the end of the first year, but especially after the third, they were seized with violent epistaxis. The slightest puncture caused a great loss of blood; coughing produced hæmoptysis, and diarrhœa bloody stools in clots. The fourth bit his tongue at play, and died in a few days from the hemorrhage, which it was impossible to restrain. All were subject to frequent attacks of pain and swelling, with ecchymosis of the wrist, ankles, and knee-joints, attended with fever; the complaint usually lasted about a fortnight, and then disappeared with the subsidence of the swelling and removal of the ecchymosis. On one occasion, two leeches were incautiously applied to the knee of the eldest, the bites of which continued to bleed for three days, and were only stopped by the twisted suture. Except varicella, which the survivor has had, none of the others were attacked by infantile diseases, though prevalent in the neighbourhood. Dentition took place pretty early, and in a healthy manner. A tendency to diarrhœa, that was followed by bloody evacuations, was the sole affection of the organs of digestion to which they were liable. Their complexions were fair, with clear blue or brown eyes, and a skin remarkably fine and white. Their gums were always firm, and they never had ulcerations of the mouth or skin. Their intelligence was quite conformable to their age. Their urine was usually clear and limpid, but they had great tendency to perspire. The eldest died of epistaxis, at the age of twelve; the second died at the age of eight, of hemorrhage from all the mucous surfaces; and the third, as before mentioned, from biting his tongue, when twenty months old. The blood from these hemorrhages was very fluid, of the ordinary colour, and coagulated like other blood.

The surviving boy has undergone the same complaints as his brothers; he is seven years old, of ordinary stature, delicate, rather thin, light complexioned, with light brown eyes, that are quick and intelligent; his skin is extremely white and transparent, with very little appearance of veins, that are very small, even on his hands and forearms; his face is exceedingly pale, but his nose, like that of his brothers, is of a bright red. His pulse is eight or ten beats quicker than in boys of his age, his breathing is normal, his gums are firm and sound, and the ends of his fingers exhibit no peculiarity. Respiration is puerile; there is slight hypertrophy, and the beats of the heart are very strong and smart (secchi), a circumstance also observed by his mother in the other children. The sounds of the heart are natural; and none of the children were subject to palpitation nor dyspnœa. No enlargement of the liver or spleen can be detected.

Omodei, Annali, vol. lxxxv., 1838.

Singular Case of Absorption of Milk. By Dr. D. Rasi.

A young lady of sound constitution married at fifteen, and removed from a damp and impure atmosphere to one remarkable for its freshness and purity. Not thirty days elapsed, before she had various nervous and uterine symptoms which indicated pregnancy. At the seventh month she had a severe pneumonia, with aggravation of the previous symptoms. The convalescence from this disease had scarcely commenced, when she was delivered of a healthy and mature child. Her health was for some time afterwards very delicate, and was only improved by a temporary removal to her native air. On her return thence, she again became pregnant, and had again the same nervousness and debility; but at the full period she bore another healthy child, which, though very weak, she was anxious to suckle. She was prevented, however, by a singular circumstance. Shortly after her delivery she had a severe fever, during which her breasts became very large and hard; the nipples were swollen and firm, and there was evidently an abundant secretion of milk; but neither the sucking of the infant, nor any artificial means could draw a singledrop of milk from the swollen glands.

It was clear that the lactiferous ducts were closed, and as the breasts continued to grow larger and more painful, purgatives and other means were employed to check the secretion of milk. After three days, the fever diminished somewhat, and was replaced by a constant cough, which was at first dry, but soon after was followed by the expectoration of simple mucus. After this, the cough diminished in severity, and the expectoration became very easy; but the sputa were no longer mucous, but composed of a liquid which had all the physical characters of genuine milk. This continued for fifteen days, the quantity of milk expectorated amounting to three ounces or more in the twenty-four hours. The breasts gradually diminished in size, and by the time the expectoration ceased, they had regained their natural dimensions.

The same complete obstacle to the flow of milk from the nipples recurred after the births of four children successively, and after that of the sixth she had similar symptoms of fever, but this time they were not followed by bronchitis or the expectoration of milk. She had in their stead copious sweatings, which with other severe symptoms reduced her to a cachectic state, and terminated fatally in a fortnight. *Bulletino delle Scienze Mediche, Aprile,* 1839.

Case in which a Salivary Calculus was Developed in the Submaxillary Duct, or Canal of Wharton. By M. Maisonneuve, Doctor in Surgery.

This case is interesting, on account of the infrequency with which earthy concretions are met with in the salivary ducts. Not above a dozen cases are on record, which are principally dispersed through the foreign journals.

The subject of the present case, a medical man, perceived in the month of June, 1839, a degree of painful swelling in the region of the right submaxillary gland, which diminished or disappeared at one time, and increased again

at others, it being particularly troublesome when eating. This led the patient to expect that there was some obstruction impeding the flow of saliva (which is secreted in large quantities during mastication) through the submaxillary duct. In the course of the following month, a small tumour appeared under the tongue on the right side, which continued much of the same size till the month of September, the retention of saliva up to the time becoming greater and more troublesome. After every meal, the submaxillary gland was generally swelled, and could be plainly felt of an unusual size through the integuments. Pressure on the gland was painful, and, if exerted to a considerable degree, forced out a large quantity of thickened saliva into the mouth, mixed with white flocculi.

At this period the patient consulted M. Maisonneuve, who on examination discovered a small oblong tumour, about the size of an almond, beneath the tongue, on the right side of the frenum. On finding the orifice of Wharton's duct, and passing into it a small probe, a quantity of thickened and viscid mucus escaped, and on carrying the instrument a little further, it grated against a rough body, which was immediately suspected to be a calculus, and which M. Maisonneuve extracted, after considerable difficulty, the rough surface of the stone adhering closely to the walls of the duct. After the removal of the foreign body the unpleasant symptoms quickly disappeared, and the patient rapidly recovered. The calculus was of a dull white colour, and covered on its surface with a number of red points; it was of the shape and about double the size of a grain of barley, and rough and unequal. Its chemical composition was not ascertained.

[Cases of this kind, as the author has remarked at the commencement of the paper, are not at all common, and therefore they are worth recording; but the principal point of interest is wanting in the foregoing observation, namely, the chemical constitution of the calculus. In animals, as the ass and horse, in which salivary concretions are more common than in man, the composition of these calculi has been found principally to consist of carbonate of lime, mixed with phosphate of lime and animal matter, and sometimes carbonate of magnesia, none of which salts appear to be natural ingredients in saliva. A few years ago we ourselves met with a case in which several small calculi were developed either in the duct of the lachrymal gland, or in the folds of the conjunctiva at the upper and outer part of the eye, from which situation they escaped with considerable pain; their chemical composition was the same as that which seems to be most general in salivary calculi, only the phosphate of lime rather predominated over the carbonate. The causes which lead to the formation of the concretions are totally unknown.] *Gazette Médicale, Sept.* 1839.

On the Influence of Mercurial Preparations in Smallpox and Cowpox.
By Dr. BRIQUET.

THE author of this paper has shown in a preceding memoir (*see British and Foreign Medical Review.* Vol. VII., p. 552,) that mercurial plasters and ointment have a great effect in modifying the characters of variolous eruptions, in favorably influencing the progress of smallpox generally, and in preventing deformity from cicatrices. In the present article M. Briquet has endeavoured to ascertain in what manner mercurial preparations exert this favorable influence; and by means of performing a number of experiments, he has been enabled to establish the following facts:

1. Mercurial preparations, applied to the skin previously to the application of blisters or other irritating preparations, do not in any manner diminish their power of irritating the skin.

2. Mercurial preparations, if mixed with irritating substances, and applied together with them to the skin, in no way interfere with the action of the latter.

3. Mercurial plasters, if applied in various inflammatory eruptive diseases,

as erysipelas, shingles, eczema, acne, furuncles, &c., very seldom if ever produce any diminution of inflammation, or in fact any effect whatever.

4. When applied after vaccination, preparations of mercury exert a marked influence over the course of cowpox. The vaccine virus frequently produces no effect at all; at other times a very small pustule forms, or only a simple vesicle filled with a whitish fluid and occasionally only a simple pimple or tubercle is developed. Thus the action of mercury in smallpox and cowpox seems to be identical. Mercury applied to the vaccine eruption after the fourth day has no influence over its course; and our author has shown in the preceding paper, that in smallpox, " in order that the plasters may have the effect of modifying the eruption, they must be applied before it has become pustular."

M. Briquet draws the conclusion from these facts, that mercurial preparations have no effect in simple inflammation of the skin, but only over some peculiar specific forms, in which the mercury does not exert its influence over the inflammation, but the virus itself, which it renders less active, or even inert. He asks, whether it is not probable that mercury taken into the system during the latent period between the reception of variolous infection and its development may not have the effect of preventing the disease altogether, or of greatly diminishing its intensity? He seems even to advise the administration of mercury to persons who have never been vaccinated, nor had smallpox during the prevalence of an epidemic of the latter disease. [One interesting result of these experiments is, that they afford an additional argument in favour of the idea that smallpox and cowpox are only modifications of the same disease.]

Gazette Médicale, 12*th Oct*., 1839.

Case of Ileus cured by Injection of Air.

A CURASSIER, who suffered occasionally from colic, had a very severe attack in the beginning of August, 1838, in consequence of having eaten very freely of raw bacon, and afterwards drinking cold water. Vomiting ensued, but without relief to the pains, which continued to return in the umbilical region with considerable violence. Symptoms of decided enteritis followed; the vomiting became more severe and fecal; and the obstinate constipation of the bowels could not be overcome, even by the administration of pure mercury. In this state a quantity of air was thrown into the large intestines, and copious evacuation of the bowels followed with instant relief of all the symptoms, the constipation having lasted for eleven days.

Medicinische Zeitung, No. xxx. 1839.

Disease of the Cæcum; Ileus and Gastrotomy. By M. MONOD.

A WOMAN, aged twenty-five, habitually in good health, only she complained occasionally of pain at the epigastrium. These pains, at length, were accompanied by vomiting; notwithstanding, her general health was good. About a year ago she was hurt about the ileo-cæcal region, and afterwards the pains were increased: since (March, 1838,) they have been felt towards the hypogastrium. No vomiting; but colic and diarrhœa. From the third day of the last attack, a large and hard tumour appeared near the ileo-cæcal region. Instead of diarrhœa, constipation and vomiting of a greenish and transparent fluid; with progressive emaciation and paleness. The tumour had an ovoid figure, about four inches long and two to three across; it was hard, elastic, scarcely sensible to pressure. Examinations, per vaginam et rectum, disclosed nothing unnatural, except a hardness towards the right side of the pelvis.

M. Monod, after due consideration, resolved to have recourse to gastrotomy. He first made an incision about three inches over the tumour, from above downwards, and from without inwards, and penetrated by degrees as far as the peritoneum; this being cut, a fold of the intestine presented itself, which being recognized by its band of longitudinal fibres to be a portion of the colon, was replaced. M. Monod then brought out by his finger a fold of the small intestine, which was red and swelled, but not remarkably sensible; into this he made an

incision, with scissors, about an inch and a half; immediately a considerable quantity of fecal matter escaped, with sensible relief to the patient; a thread was then passed through the mesentery, which was secured by strips of plaster, and the dressing completed with cerated lint and charpie. Two days after the patient died. *Dissection.* Inflamed peritoneum; sero-purulent fluid in its cavity; the intestinal folds within the pelvis covered with half concrete pus. The incision was made in the ileon about eight or nine inches from the cæcum; the cæcum very adherent behind; the superior folds of the small intestine above the artificial anus are a little dilated. The obstacle to the exit of the fæces was found in the superior and posterior part of the cæcum, at its junction with the ascending colon. The cæcum there presented a considerable contraction, so that only a female sound could be introduced; the little finger was found too large. At this place the cæcum, everywhere adherent to its surrounding parts, was connected to a scirrhous mass, about the size of a nut, of a very hard, gritty, and whitish substance.

This interesting case is, nevertheless, a very puzzling one; since no diagnosis of such fatal mischief could be had beforehand. The evil was too far gone to be remedied. Then why resort, it may be asked, to the painful and dangerous operation of gastrotomy? It is enough to reply that, when stercoral vomiting manifests an internal strangulation, *under such circumstances as these,* there is nothing *else* to be done; and from a mere artificial anus and peritoneal inflammation, some have recovered. *Gazette Méd. de Paris*, No. xli., 1838.

On the Structure and Site of Condylomata. By GUSTAV SIMON.

I. CONDYLOMATA, although usually divided into two varieties, the broad and pointed, are, anatomically considered, analogous in structure. When examined by the naked eye, they are seen to be composed of two substances: an external coat and an internal homogeneous mass. Microscopical examination shows that the external coat is a membrane nearly allied to the epithelium of mucous membranes; and that the internal mass is composed of numerous cells of various shapes, which, in the older part of the condyloma, are united into fibres, and these again into bundles. It has been shown by Schwann that all the animal tissues are, like those of vegetables, originally formed from cells, which are more or less modified, according to the stage of advancement at which the tissue has arrived; and Dr. Simon has found that the structure of cellular tissue, as described by Schwann, corresponds with that of the internal mass of condylomata.

Müller's Archiv. Heft i. 1839.

II. *Condylomata under the Tongue.* In a recruit to the fusilier battalion of the 31st Prussian infantry regiment, there were found condylomata under the tongue, which formed two rows of cockscombs, like lentils placed on their edges, extending on either side from the frænum to the point of the tongue. The patient's organs of generation were sound, and he affirmed that he never had chancre, but had suffered twice from gonorrhœa, two years ago. The patient was completely cured in four weeks by the internal use of sublimate, and by the external application of a solution of the same preparation, with extractum conii and honey.

Militair-Medicinal Berichten.—Medicinische Zeitung. May 15, 1839.

[*Query.* Was not this merely an enlargement of the crest or fold of the mucous membrane on the lower surface of the tongue, where the ducts of the sublingual glands open?]

Case of Neuralgia, after Amputation, cured by Acupuncture. By FILIPPO LONGHI, M.D.

TERESA LONGHI (sister of the reporter), aged forty-nine, a Portuguese, had suffered amputation of the thigh for necrosis of the leg and white swelling of the knee-joint. A few months after the operation, severe pain, at irregular intervals, sometimes accompanied by fever, attacked the nerves of the stump. These pains were sudden and severe during the night, and appeared to the poor sufferer to

recall her former agonies along the whole limb, previous to the amputation. Several external and internal remedies were tried without effect. At length, on the 19th of July, 1838, after six months' calm, she was again attacked by agonizing pains, not only in the stump, but extended to the opposite thigh, accompanied by convulsions and fainting, without any diminution of the suffering. In this state two needles were planted in the course of the sciatic nerve, from the nates downwards, towards the stump. These were productive of no effect: but scarcely had the third been introduced, when the patient exclaimed, "the doctor has struck the toe of my foot." Sr. Longhi then pressed the needle still deeper, so as to pass through the nerve, which was immediately followed by complete cessation of all pain. He then applied a fourth needle, without effecting any change. After the needles had been in half an hour, they were removed, and the patient enjoyed a profound sleep, and awoke free from pain. About twenty days after, the pain recurred, and the needles being again applied, again produced immediate relief.

Bulletino delle Scienze Med. Ottobre, 1838.

On the Use of Acetate of Morphia in Arthritis, and Nervous Affections. By M. Vincent Cristin, Physician to the Hospital of St. John, Turin.

The following is Dr. Cristin's method of administering the acetate of morphia in these affections. Dissolve one grain of acetate of morphia in four ounces of distilled water, and add one ounce of syrup of gum arabic. A spoonful of this mixture to be taken every hour. When the pains are relieved, or sleep is about to commence, it should be given every two hours only, or suspended altogether; depending upon the narcotic effects produced. During its administration the patient should avoid fluids.

A woman, aged sixty, was afflicted with arthritis in the extremities; there was acute pain upon motion, hard pulse, burning heat of skin, &c. &c. The acetate of morphia relieved the pains, procured sleep, removed the fever, caused profuse perspiration and diarrhœa, with abundant secretion of urine. Opium excites perspiration, but diminishes the other secretions. Acetate of morphia is therefore the preferable agent.

Nervous pains, such as frontal neuralgia, sciatica, cephalalgia, and syphilitic pains, have all yielded to this remedy.

Repertorio Medico-Chirurgico del Piemonte, Jan. and Feb. 1838.

Epilepsy treated with Belladonna. By M. Jules Picard.

Since the 9th of September, 1837, twenty-two patients, afflicted with epilepsy, have been treated with belladonna in the wards of M. Ferrus. In six it produced symptoms which indicated the propriety of its discontinuance. In eight it was employed during a space of time, varying from forty days to four months and a half. In these it was discontinued, either on account of its producing no effect, or because the patients were tired of the treatment, or wished to leave the hospital. The remaining eight continued the treatment. Three commenced with 4 grains, fourteen with 6 grains, one with 9 grains, and three with 12 grains. The largest dose given was 18 grains. In three cases the epileptic seizures were not so frequent. *Revue Médicale, April,* 1838.

SURGERY.

On the Treatment of Varix of the Inferior Extremities. 1. *By Pins.* 2. *By Caustic Potass.* 3. *By the Combination of these Means.* By M. Bonnet, First Surgeon of the Hôtel Dieu de Lyon.

[We doubt not that at the present time, when much is said about various methods of treating varicose veins, an attempt such as the following, to ascertain the value of a particular treatment, not only from its immediate but from its subsequent effects, will be very acceptable to many of our readers. In this, as in some other operations of surgery, a partiality may be acquired for a particular

method of operating, from something which is attractive in the operation itself and its immediate consequences, too little regard being paid to its ultimate effect upon the disease, the complete remedying of which should be the paramount object of all operations.]

In 1834, I commenced, says M. Bonnet, to study the subject of the radical cure of varices. In the following year, I employed pins in sixteen cases, in which the veins were in relief beneath the skin, and caustic potass in two cases of females, in which the dilated veins were lost in a large quantity of fat. The last two were cured, as well as all those who were treated with pins, and who were in such a condition as to render a radical cure certain. Both methods appeared to me equally useful. But as hemorrhage had occurred through the eschars at the time that the caustic had opened the veins, and as no serious accident had followed the use of pins, I came to the conclusion that the operation by pins was to be preferred. But I determined to watch the progress of these cases; and the consequence was, that I was disappointed in the effect of the pins. All the patients whom I saw again, after having remained well during a time varying from one to six months, were reaffected with varix, with as much intensity as before the operation, and this not only in the secondary divisions of the veins, but in the trunk of the saphena, on which the larger number of pins had been applied, and where it appeared that the obliteration would be permanent. In the two cases treated by caustic potass, there was no return of the disease: one of these was seen fourteen months after the treatment, the other several years afterwards. From these facts I was led to infer, that the pins produced but a temporary obliteration; but that caustic potass determined a permanent closure, its use, however, being attended with risk of hemorrhage. I considered, therefore, that if I placed pins at intervals upon the saphena, and cauterized the vein between them, I should obtain a temporary obliteration by the pins, such as would prevent hemorrhage, and a permanent obliteration by caustic, such as would effect a cure. I employed this treatment on nine patients, in 1837, but these cases I never saw again. But, in 1838, I treated a man in whom the ulcerated varicose veins gave rise to abundant hemorrhage. I had lost confidence in pins, and found the combination of pins and caustic too complex. I had, therefore, recourse to caustic alone, believing that bleeding might be controlled by compression and position. The result of the practice, in this case, having justified my expectations, I resumed a practice, which I immediately employed on a new series of twelve cases, the course of which more and more confirmed me in the idea, that the treatment of varices by caustic alone is, of all the methods which I employed, the most simple in its application, the least uncertain in its effects, and that which secures the most complete and the most permanent cure.

Before further considering the question of treatment, which is the main object of this paper, I would notice a fact in morbid anatomy relative to varices, and a symptom from which their importance and the effects of treatment upon them may be inferred. I speak of those tumours which stand in the same relation to veins that spontaneous aneurisms do to arteries, and of the undulation which may be communicated to the blood in varicose veins, in a contrary direction to that which happens when the valves are entire, and the blood takes its normal course.

Varicose Tumours analogous to Spontaneous Aneurisms.—I removed one of these tumours from the course of the internal saphena vein: it contained a quantity of liquid blood, with soft and blackish clots; its fibrous walls were perfectly smooth on their internal surface, and its cavity communicated with that of the vein by an opening, three or four lines in diameter, which surrounded the small portion of the vein which I had detached. This tumour was like an aneurism; its walls were continuous with those of the vessel on the side of which it was situated, and the cavities of the one and of the other communicated by a narrow aperture. I have since met with this affection in two instances, and each time on the crural portion of the internal saphena vein. On percussing one of

these tumours, the blood was made to flow backwards in the dilated vessel, and on placing the hand upon the other, a distinct pulsation was felt, which pulsation was also visible. This reflux, this pulsation, seems to me to be the distinctive character of the tumours; because it shows that the fluid contained in them communicates freely with that in the veins. This form of varix has been very little mentioned by authors. The two cases just mentioned were treated simply by passing a pin through the bloody mass: it was secured by the twisted suture, and not removed, in either case, for eight days. The blood began to coagulate on the second day; on the eighth, the swelling had become hard, and in about a month it was very hard, and of the size of a small nut. This I regard as a successful issue of the treatment of these tumours by means of pins. The undulation of the blood caused by percussion of varicose veins should be very carefully attended to, both as a mean of judging of the difficulty which may attend the radical cure of varix, and especially to ascertain whether or no, when pins have been employed, they have been employed properly; for if the pins properly compress the opposite sides of a vein, the undulation of blood stops at the place so compressed. This sign should also be attended to at the conclusion of the treatment, as a mean of judging if the treatment requires to be renewed.

Treatment of Varices by Pins alone.—Velpeau, Davat, Jobert, who have written on the treatment of varices by pins, have not specified the cases in which they necessarily fail, and in such cases as failure does not happen, to what extent the cure is permanent; nor have they insisted on the principles on which this treatment is founded. These defects it is my intention to supply.

In the treatment of Varices, it is necessary to obliterate the veins in several points, separated from each other by short intervals.—The necessity of this is admitted, depending, as it does, on the numerous anastomoses of the venous trunks. It is easy to find, in various works, instances of continuance or recurrence of varix, after the obliteration of a vein in one place alone, whether this obliteration was spontaneous, or artificially effected. On the other hand, in addition to written evidence of an opposite character, all the observations which I have made on the obliteration of veins, either by pins or by caustic potass, have but confirmed me in the opinion, that veins should be obliterated in several points. I have always observed, that the coagulation of the blood, and the contraction of the vein, took place only in the vicinity of the obliterated points; that the divisions of the veins distant from those which the operation had rendered almost impermeable to blood. remained almost as dilated as before the operation. I consequently have been in the habit of obliterating at as many points as possible, placing from four to ten pins on the same individual, some on the course of the internal saphena, from three to four inches apart, others at the point of junction, or upon the course of the principal divisions connected with it.

Methods of placing the Pins.—There are three principal methods. That of M. Davat consists in plunging the pin upon the middle part of the vein which is to be obliterated, passing it through from the superficial to the deep part, and again in a contrary direction. When the point of the needle protrudes, it is fixed by means of a thread, twisted as in the twisted suture, and it is allowed to remain until the parietes of the vein, in contact with one another at the divided part, become inflamed and adherent. This plan is easily enough effected by means of common pins, when the part to be transfixed takes an oblique direction in relation to the axis of the limb. In this case, after having transfixed the vein, the head of the pin being depressed, its point is passed for two or three times behind the vessel, which is again transfixed with ease, from within outwards. But when the vein runs parallel to the limb, as is commonly the case in the thigh, it is difficult, without giving an oblique direction to the pin, to pass it properly, so that after having transfixed it inwards, the pin may not enter it in coming outwards, but may pass behind it. The method of M. Velpeau consists in passing a pin transversely beneath a vein, without wounding its parietes, securing it afterwards by means of the twisted suture. M. Fricke simply transfixes the vein with a pin, and allows it to act as a seton in the production of adhesive inflam-

mation. The method of M. Davat appears the most efficient of these three: it ensures inflammation, compression, and the contact of the parietes of the vein, in those parts which the pins have divided. M. Davat considers that it is very important to slightly divide the membranes of the vein, before putting them into contact. He thinks M. Velpeau's method insufficient, that it simply produces coagulation which may not be durable. He dissected the jugular vein of three dogs, which he had operated on in the manner described by M. Velpeau. In one case, the pins were withdrawn on the eleventh day; in the other two cases, on the ninth. In the first case, the vein was thickened for the extent of an inch, a fibrous cylinder occupying its cavity, which was contracted but not obliterated. In the other two cases, the course of the blood was re-established, the veins being simply thickened. Other experiments of M. Davat show that obliteration depends upon adhesive inflammation, that the cicatrices are durable, and not dissipated with the other effects of the inflammation.

Period during which I have left the pins in place.—I have never waited for the establishment of suppuration before removing the pins. When the red tumefaction and pain around the pin commenced, I cut the thread, and as soon as the swelling and redness were very marked, and the pain such as to disturb sleep, I removed the pin. The time required for the production of these symptoms varied from three to fifteen days. In one case only, having neglected to take away a pin which caused great pain, and which had produced, on the fourth day, a swelling, the surface of which was somewhat larger than a sixpence, an active inflammation occurred in the upper half of the leg. This was of a phlegmonous character, requiring very active means to prevent suppuration. This, and other cases, induced me to remove the pins before suppurative inflammation took place.

Results obtained by treatment of Varices by Pins alone.—These cases may be thus classed: 1. Varices without ulceration of the vein, without œdema, the vessels being easily visible, and capable also of being felt throughout their whole course. 2. Varices very much folded on themselves, resembling, in this respect, the intestines. 3. Varices accompanied by œdema, and losing themselves in a quantity of fat. Eleven patients belonged to the first class; two of these were more than sixty-three years of age, and were debilitated. The cure was not even momentary, the blood did not coagulate in the interspaces of the pins, and before they left the hospital, the obliteration, the existence of which was known by the stoppage of the undulation, even after the removal of the pins, was completely destroyed. These facts are not astonishing. In advanced life, the blood is indisposed to coagulate, adhesions are indisposed to form; and no attempt should be made to cure varices after the age of sixty years. The other nine were, with one exception, under fifty-four years of age: they left the hospital quite cured; the blood was coagulated in the whole course of the varicose vessels; these were diminished in size, and a very long walk did not cause swelling of the veins. But, of these nine, I saw five again. In two of these, the veins began to enlarge and to become permeable, as soon as the patients resumed their occupations; in two others, two or three months afterwards, and in the fifth, six months after his having left the hospital. One or two months more were required in each case, before the dilatation was as much as before the operation. The patients of the second class were almost completely cured: some few venous branches only swelled whilst walking. In one of these, fifteen months afterwards, the internal saphena and its branches had become as large as before the use of the pins. In the two other cases, in which the veins were very large, lost in a great quantity of fat, and spread in large number over the whole leg and instep, although in one of these I applied twelve pins, and in the other fourteen, which were not withdrawn until from the eleventh to the fifteenth day, only a momentary benefit was obtained. One of these patients was sixty, and the other forty-eight years of age. The same failure occurred in two patients, whose varices were imbedded in fat, and were attended with œdema. Of the twelve patients regarded as cured on leaving the hospital, the six whom I saw afterwards suffered a re-

lapse, after a longer or shorter period. There were among these some whose veins were most acutely inflamed, and where the cure appeared to be complete.

It is an important question, *How do veins once obliterated become again permeable to blood?*—Phlebitis obliterates veins: 1. By infiltration of serum into their surrounding cellular tissue, and into the substance and on the surface of their proper tunics. 2. By the secretion of organizable matter in the same parts. 3. By coagulation of blood. It is evident that a diminution of inflammation will account for the re-establishment of the current of blood. But M. Davat asserts, that by his method an inflammation is excited, which gives rise to adhesion of the opposed venous surfaces, by a fibrous tissue, and that this adhesion is indestructible, as is shown by experiments on dogs. But it is not certain that the adhesion which takes place in the veins of dogs will also occur in varicose veins. These no longer possess their normal texture; they are changed into a fibrous tissue, having but little disposition to secrete organizable matter, and consequently contract to adhesions; the inflammatory phenomena occur rather in the healthy cellular tissue around than in the substance of the veins themselves. These considerations explain clearly the want of adhesion, and the merely temporary obliterations which, at first sight, appear somewhat unaccountable.

Varices treated with Caustic Potass.—Caustic has been long employed in the treatment of varicose veins; but the principles of its correct application have not been laid down. The following are the rules which have guided me in the treatment of varix by caustic potass:

1. *It is necessary to apply several morsels of caustic potass on the course of the dilated vein, and at a distance of three or four inches from each other.* This is a repetition of the principle already laid down in speaking of the treatment by means of pins.

2. *The caustic potass should only be applied to the veins at such points as these correspond to the muscles.* Other situations than these are unfavorable to cicatrization, and if cicatrization takes place, the ulcers are readily renewed. The situations whieh I prefer, are,—1, for the thigh, at the height at which cauteries are commonly applied, but a little backward, in consequence of the situation of the vein; 2, for the leg, at the height commonly chosen for cauterization; 3, the middle of the thigh, or the middle of the leg, if three applications should be necessary. In each case, it is supposed that the vena saphena is alone affected.

3. *It is necessary to apply the caustic at least twice, in order to reach the vein.* This supposes, that in order to obliterate the veins, it is necessary to open them by the caustic. All the cases which I have observed, have convinced me that this is a fact. The undulation of blood has never ceased after the first application, which has only implicated the skin and subjacent cellular tissue. The conversion into a hard and impermeable cord has never happened, until after a second application. Would it be better to destroy sufficient at once, or by making two successive applications, to make the second application in the centre of the eschar produced by the first? The reply is not doubtful, if it be considered that it is sufficient to open the vein, and to destroy it to the extent of a few lines, and that any further destruction is useless. A single piece of caustic, which would reach the vein, would make a large eschar of the integument; but with a small piece, twice applied, the part may be hollowed out without extensive destruction of the surface. We should wait three or four days, before applying the second piece of caustic. The plan then is, to make a crucial incision through the eschar, and to insert the caustic into this incision. After the second application, the blood escapes. It is possible, that even a third application may be required to open the vein, but this has always been sufficient. In this way I have applied the caustic, both when I employed it alone, and when I conjoined with it the use of pins. I have employed this treatment in fourteen cases, with different results as it regards the cure of the disease, but always without serious effects.

We pass over M. Bonnet's observations on the cases in which he employed both caustic and pins, because he gives the preference to the treatment by the former alone.

Caustic potass applied over veins does not expose the individual to phlebitis, i. e. to that kind of phlebitis which extends from the part operated on towards the trunk. Of course a certain amount of local inflammation is essential to the cure. In advancing the above proposition, I am supported by the fourteen cases in which the potass was employed alone, and by six others where it was combined with pins. Here are twenty cases, in none of which was the inflammation disposed to pass along the veins, although three or four applications of caustic were made in their course, most of which opened their cavities. The patients were simply confined to bed, taking their usual diet at the same time. General facts are also in support of the opinion which I derive from my experience. These facts show that cauterization limits all inflammations which are disposed to extend.

The application of caustic potass is followed by a circumscribed inflammation, and by ulcerations which are slow to cicatrize.—In the cases where several pieces of caustic are applied very near each other, the inflammations which surround each eschar run together, and the consequence is a true phlegmon.

The application of caustic potass exposes to hemorrhage; but this hemorrhage may be easily avoided, if the patient keeps his bed, and if a slight compression is exercised around the limb.—It has been already said, that it is necessary to apply the caustic until a slight escape of blood shows that the vein has been opened. There may be an actual hemorrhage instead. How long should a patient remain in bed, to render him safe from all chance of hemorrhage? This cannot be fixed by days, but must depend on the condition of parts. When the vein is opened, and this is known by the escape of a few drops of blood, the blood coagulates beneath and above the perforated part; the vein becomes hard, and percussion communicates no undulation to the blood. These signs, which demonstrate the obliteration of the vein, show that there is no further chance of hemorrhage. Four or five days commonly suffice for the accomplishment of these phenomena. In order to second the effects of rest, a bandage should be applied around the limb, immediately after the application of the second piece of potass. The neglect of this was, in one case, followed by hemorrhage, one hour after the application of the second caustic. But a roller immediately stopped the bleeding. No other patients treated with caustic potass, who kept their bed, and to whose limbs a roller was applied, suffered from any hemorrhage worthy of the name; the effusion of a few drops of blood alone announced that the vein was opened.

In the varices which are limited to the internal saphena and its divisions, and which affect persons within sixty years of age, caustic potass produces a complete and permanent cure.—The fact of the completeness of the cure rests on fourteen cases. Those which are related, leave no doubt of the superiority of cauterization compared with the employment of pins, as a mean of effecting the obliteration of veins. In one case only was there not a complete interruption to the current of blood in the vein.

The cure of varices by caustic potass, or by any other means, should not be attempted when the internal and external saphena veins are dilated.—In the only two cases in which the caustic was applied to varices of both saphenæ, it was observed, that whilst the internal saphena was obliterated, the external saphena acquired an increase in size, and that varices previously but little apparent, became more voluminous, so as to substitute a new disease for that which had been cured. Advanced age, and the thickness of the coats of veins, such as renders their approximation difficult, even with the pressure of the finger, are likewise circumstances unfavorable to the use of caustic potass.

The treatment of varices by caustic potass hastens the cicatrization of ulcers which co-exist with them.—This may in part be explained, by the revulsion caused by the numerous artificial ulcers, and by the cure of the varices.

In what cases should cure of varicose veins be attempted?

1. Whenever varices ulcerate, and give rise to hemorrhage.
2. When varices exist with ulcers so extensive as to require rest of six weeks, two months, or upwards.

Setting aside these complications, I think we should attempt to diminish the swelling by a stocking. If there are no ulcerations, or if these are but little extensive, if they may get well in one or two weeks, it is unnecessary to keep the patient in bed for above a month; this time is necessary for the separation of the eschars formed by caustic potass, and for the cessation of the pain which remains for some time afterwards in the deep ulcerations. In this case, the remedy would indeed be worse than the disease. M. Bonnet concludes the above highly interesting and useful memoir, by an eulogium on the superiority of the treatment therein recommended over every other, and by a prediction that it will gain the credit which it deserves.

Archives Générales de Médecine. Mai, Juin, 1839.

Neuralgia of the Inferior Dental Nerve cured by Section and the Actual Cautery. By Dr. Sani.

G. L., aged fifty, of a delicate habit, was brought to a state of extreme sensibility, by frequent child-bearing, and various severe diseases. At the cessation of menstruation she was seized with neuralgia of the inferior dental nerve. The pain began at the left mental foramen, with torpor and formication, and very quickly became pulsatile and extremely acute, darting like a flash to all the branches of the nerve, and then extending to all the other divisions of the fifth pair, and of the cervical plexus, with which they are connected. There were redness of the skin, and contractions of the muscles corresponding to the parts that were in pain; the secretion of saliva was increased, and there was difficulty in speaking and masticating. The attacks of pain, which were at first slight and occasional, became intense and very frequent, in spite of the various means that were employed; e. g. anodynes, leeches, extraction of the teeth, tonic and astringent lotions, &c.; and the patient lost her rest and her appetite, and became emaciated.

Dr. Sani, therefore, decided on dividing the nerve. An incision was made along the fold that connects the lip to the gum, from the fourth molar to the first incisor tooth, and was continued in depth till it exposed the inferior dental artery and nerve emerging from the mental foramen. Some attempts were made to separate them, but in vain; they were therefore both divided together, close to the foramen, and a portion of each, two or three lines long, was removed. The division of the nerve caused the most acute pain, and hemorrhage took place from the artery, to which it was found necessary to apply the actual cautery. The simplest means were afterwards employed. On the eighteenth day, the cicatrization was complete, and, except for some slight creeping pain, which the patient felt from the eighth to the eleventh day after the operation, no inconvenience was afterwards suffered. The parts supplied by the divided nerve remained completely insensible, but their motions were unimpaired.

Bulletino delle Scienze Mediche. Gennajo, 1839.

Case of Idiopathic Mydriasis. By Dr. Kochanowski.

The dilatation of the pupil was in this instance so considerable, that the iris formed only a very narrow ring. The strongest light produced no effect on it. After unsuccessfully trying a variety of medicines, it struck Dr. Kochanowski that the ergot of rye might be useful. On the twenty-second day of the disease, if it may be so termed, the exhibition of this medicine was commenced, in doses of three grains four times daily. On the following day, the patient (a woman aged thirty-three, long subject to hemorrhoids and menstrual derangement,) became conscious of a change in the state of the part; she felt something move, as it were, in the eye, under the influence of light. On examination, the pupil was found to have contracted a little. The dose was now increased to fifteen

grains: the improvement advanced. During the menstrual flow the administration of the drug was discontinued, and the dilatation of the pupil returned. After the disappearance of the menses, the dose was raised to eighteen grains, or a scruple; at the end of a few days the pupil had recovered its normal dimensions, and the iris its usual contractility.—(*Memoirs of the Medical Society of Warsaw.*) *L'Expérience. Septembre*, 1839.

On a new Method of Making an artificial Anus in the Lumbar Region.
By M. AMUSSAT.

A QUESTION which has lately been exciting considerable interest among the surgeons in Paris is, the practicability of forming an artificial anus in the lumbar region, in cases where a cancerous or other disease causes an insurmountable obstruction to the natural course of the fæces. Callisen recommended this operation, but its propriety has been generally disputed by modern surgeons. M. Amussat, however, meeting with a patient in whom such an operation was required, has revived Callisen's proceeding, with some modifications, and has successfully practised it in two cases.

The first case was that of a woman, forty-eight years of age, who had been long subject to constipation. In the beginning of May, 1839, the constipation became more obstinate than usual, and resisted the use of baths, injections, and the most active purgatives. After the ineffectual employment of these remedies for twenty-six days, a consultation was determined on, and MM. Barras, Amussat, Fouquier, Breschet, Récamier, and Puyoo, were called in. On examination of the rectum, a hard, round, and almost immovable tumour was felt at a considerable height in the intestine, about double the size of the neck of the uterus. The only resource, in this case, seemed to be the formation of an artificial anus, which was accordingly made on the 2d of June, by M. Amussat, in the following manner: The patient was placed on her face, with her abdomen supported by pillows; and a transverse incision was made in the left lumbar region, where the accumulation of fecal matters caused a considerable projection. The incision was commenced at the external edge of the sacro-lumbalis and longissimus-dorsi muscles, and was extended outwards for four inches and a half, at the distance of two fingers' breadth above the crista of the ilium. The skin and muscles being divided, the adipose tissue surrounding the kidney was brought into view, on which the posterior part of the lumbar colon rests: on cutting through this fat, the colon immediately appeared between the edges of the wound, in a very distended state. A loop of thread having been passed through the most projecting part, for the purpose of holding it in its situation, a trocar was first plunged into the intestine, when a quantity of gas and liquid fæces escaped through the canula. An incision of an inch and a half in extent was then made with a bistoury, in the transverse directionof the colon, and an immense quantity of fæces was discharged, the exit of which was encouraged by injections of warm water into both ends of the intestine. The edges of the wound in the colon were, lastly, fastened by stitches to the margins of the most anterior part of the external wound, and the whole was covered with a poultice.

Very little hemorrhage occurred during the operation, and the relief of all the symptoms was immediate, the patient passing a good night afterwards. On the following day, slight symptoms of inflammation showed themselves, but they were removed by the application of some leeches. The external wound rapidly healed, and, at the period of four months after the operation, the patient was quite well, and had regained her strength; the artificial anus presented a regular rounded orifice, through which the fecal matters passed in a solid form, once or twice a day. A simple bandage fixed round the body was sufficient to retain them, which was taken off when an inclination to void the fæces was felt. Nothing, except gas, passed by the natural anus. [No mention is made of the state of the disease of the rectum, after the operation.]

The other case was very similar to the preceding. The operation was here

formed on a man, aged sixty-two, who was affected with cancer of the rectum, which entirely obstructed the canal. This case turned out equally successful; the man's health was much improved by the operation, and the progress of the disease of the rectum seemed to be arrested.

The formation of an artificial anus in the groin, in certain cases, was proposed formerly, by Littre; and eleven cases are on record in which it was performed, of which number six died. Eight of these cases were in infants, and only three in adults, out of whom one died. M. Amussat considers that his operation (which differs from that of Callisen, in the incision being transverse instead of vertical,) possesses several advantages over that of Littre, the principal of which is, that the peritoneum is not wounded; this part of the intestine possessing no mesocolon, according to M. Amussat, in any instance where it is distended with gas or fecal matters. Another advantage of it is, that an artificial anus in the lumbar region is less affected by the contractions of the abdominal muscles than in the groin, and consequently prolapsus of the intestine is not so liable to occur.

[The expediency of performing this operation, in any case, has been questioned, for it has been said that the disease which causes the obstruction in the rectum is almost always of a malignant character, and will, sooner or later, destroy the patient; and the operation, if successful, can only prolong a life of misery for a very short period. These arguments, however, ought to have no influence over the measures of the surgeon, whose duty it undoubtedly is, to prolong life to the greatest extent, under almost all circumstances, particularly in adults, the continuance of whose existence, even for a few months, may be of the utmost value to themselves and families. The propriety of forming an artificial anus in the loins or groin of an infant, in whom the rectum is deficient or malformed, may perhaps be reasonably doubted.]

Bulletin Gén. de Thérapeutique, 15 *et* 30 *Oct.* 1839.

Treatment of External Cancer by Ligature of the Vessels and Division of the Nerves supplying the diseased part. By M. Jobert.

Feeling persuaded that the increased afflux of blood and heightened nervous sensibility, which are the consequences of disease, exert a great local influence in cancerous affections, M. Jobert has adopted a new plan of treatment; namely, that of tying the principal arterial branches and dividing the nervous filaments which are distributed to the affected part. He has seen this proceeding followed by a favorable change in the aspect of the ulcers, and by their ultimate cure. He has obtained this successful result in four cases of cancer of the lip, and in one of the tongue.

M. Jobert is of opinion that the vascular system has a much more important share in the development of cancerous affections than the nerves of the part; therefore he considers that tying the arteries will have much more influence in checking the progress of the disease than the division of the nervous filaments.

Revue Médicale, Sept. 1839.

On a new Method of Detecting Changes of Form in the Prostate Gland. By A. Mercier, M.D.

[M. Mercier having pointed out some defects in the modified catheter employed by M. Leroy d'Étiolles for the above purpose, describes the instrument invented by himself.]

The instrument I have employed for upwards of three years, with marked success, is extremely simple, being nothing more than a sound, which is perfectly straight, except at the vesical end; here it is curved at a right angle at about six or eight lines from the point. Once this sound is introduced into the bladder, it may, on account of the shortness of its curved extremity, be moved about the neck without the least difficulty. The external extremity is provided with an oval or polygonal plate perpendicular to the curved part of the instrument; an index on one of the surfaces of this plate shows on which side the point of the

sound lies at any given moment while the operator rotates it. There is no particular difficulty in introducing this sound, especially in old people, who are peculiarly subject to the diseases it is designed to detect. The prostatic portion of the urethra would be the part most likely to present the obstacle to its passage; now Hunter has remarked that in cases of hypertrophy of the lateral lobes, the antero-posterior or recto-pubic diameter of the urethra invariably undergoes considerable increase: I have myself seen it measure fourteen or fifteen lines. When the instrument reaches the prostatic division of the canal, the free extremity must not only be lowered, but pushed straight on towards the neck of the bladder: by these means the point will be prevented from catching against the anterior wall of the canal.

Method of detecting prostatic tumours protruding into the bladder.—When the sound has been passed into the bladder, the next thing to be done is to place it in such position that its straight portion shall be nearly parallel to the axis of the body. The curved point is then drawn against the anterior border of the neck of the bladder, and then turned to the right and left, and carried round the circumference of the opening of the urethra. When this surface of the prostate is in a normal state, the point of the sound may be swept round the whole extent without obstruction. But if there be a tumour connected with the neck, the instrument is checked in its progress, and in order to carry it over the obstructing body, it becomes necessary to raise the point proportionally to the size of that body. The plate already referred to shows *on what side* of the prostate the stoppage has occurred: the arc of a circle described in the rise and fall of the point, as it passes over the obstacle, represents the *width* of the tumour, and its *height* is measured by observing at the extremity of the glans how many lines the instrument has risen. Finally, from the greater or less degree of abruptness with which the point rises and falls, we learn whether the prominence is *pedunculated or provided with a broad base.*

[The reader will readily conceive the modifications required in the mode of manipulating, when the neck of the bladder or the prostatic portion of the urethra are to be examined. M. Mercier, after stating these, continues:]

Such are the results obtainable with this instrument. By it we are enabled to discover the organic cause of numerous cases of paralysis of the bladder, erroneously considered essential. The consequence of a correct diagnosis is here of no mean importance, in a therapeutical point of view; for even if the affection we thus discover be regarded as incurable (which is a most serious error,) we shall at least learn the necessity of abstaining from the use of irritating injections, blisters, friction with the tincture of cantharides, &c. For it is evident that a plan of treatment of this kind will be the more seriously injurious the more fully it realizes the effect intended in its employment—the more it increases the contractile power of the bladder, which can only exhaust itself in vain efforts to overcome a permanent obstacle.

The instrument is also available in the diagnosis of tuberculous disease of the prostate occurring in youth, of inflammatory swellings and abscesses, and in the discovery of calculi concealed behind prominences of the neck of the bladder.

Archives Générales de Médecine. Juin, 1839.

MIDWIFERY.

Case of Metritis with Epilepsy, in which Separation and Expulsion of a great part of the Vagina and of the Neck of the Uterus, followed by perfect Recovery, took place. By Dr. ANTONIO LONGHI.

ROSA GATTI, æt. 27, was admitted, May 11, 1835, to the Milan Hospital. Her health had, with the exception of occasional epileptiform attacks, hitherto been excellent. The catamenia had appeared early, and were extremely copious, appearing sometimes twice a month. In February, 1835, suppression took place, without causing any serious inconvenience to the patient, and still existed at the

time of her admission. Immoderately addicted to drinking and sexual indulgence, her propensity for the latter had of late been additionally excited by pruritus of the genitals. On the 8th of May she was seized with an epileptic fit of much greater intensity than usual; for this she had been twice bled, previous to her admission. When examined at the hospital, there was total loss of sensation and motion, with a comotose state; uterus somewhat enlarged, and found per vaginam, to be of the size usual at the third month of pregnancy; fetid yellow discharge from vagina; violent convulsions brought on by abdominal pressure. Copious bleeding was immediately ordered, and repeated on the 12th and 13th; the frequency of the epileptic paroxysms was thereby decreased, but all the other symptoms remained unchanged. After the application of eighteen leeches to the head, of four blisters, cupping at the occiput, &c., the coma had, on the 17th, in a great measure disappeared, and the patient answered some questions. The abdominal tenderness, however, increasing on the 18th, twenty-four leeches were applied to its most painful part, the epigastrium, and a grain of tartar emetic given in an emulsion, which was immediately rejected by vomiting. There still remained an increasing febrile movement; the pulse was hard and small; and lancinating pains were felt in the abdomen. On the 21st, in straining, as if to discharge the contents of the rectum, she felt a voluminous body pass through the vagina, which, as she fancied herself pregnant, she took for a fœtus. This body, on examination, was found of pyriform shape, with a semicircular aperture at the apex, in size somewhat larger than the healthy unimpregnated uterus, fetid, blackish, of moderate consistence at the base, from which part its cohesion diminished towards the apex. Carefully washed, it appeared of a dirty white colour, and covered with soft cellular membrane, the base rounded and smooth, with a small central opening. On introducing the finger within the great aperture at the apex, a large cavity was felt, terminating at the opposite end by a fleshy body; and on slitting up the walls of this cavity, they were found to consist of a part of the vaginal parietes, and the fleshy body of the neck of the uterus. The patient, still believing she had miscarried, obstinately refused all medicine. Emollient vaginal injections, however, were constantly employed, and a milk diet ordered. Under this simple treatment the discharge from the vagina gradually decreased, as did also the fever and abdominal symptoms; and on the 18th of June the patient was discharged in perfect health, with the exception of a slight yellow inodorous discharge. On this day, a careful examination, per vaginam, was for the first time made. The external parts were natural, the vagina notably contracted, especially at the upper part. At about the ordinary height it ended in a firm cartilaginiform ring, that scarcely allowed the passage of the index finger. Beyond this ring the finger entered a cavity, of considerable width, supposed to have been formed by the separation of the parts passed by the vagina. The finger, pushed still higher, touched two unequal small prominences, divided by a transverse fissure. The entire cavity was surrounded by a wall of lardaceous consistence, without callosity or ulceration. Immediately on leaving the hospital the patient returned to her old habits; but the pain and hemorrhage attending copulation were so great, the first day, that she was obliged to desist. The vagina, however, gradually yielding, ceased to interfere with the gratification of her desires. Towards the end of June the menses reappeared, and flowed with the utmost regularity. The parts discharged are preserved at the Milan hospital.

Giornale delle Scienze Med.-Chir. No. xxii.

Case of Impacted Head, with great subsequent Loss of Parts.
By M. Cazenave, of Bordeaux.

The patient, a primipara, æt. twenty-five, of diminutive stature, had always enjoyed good health. As the labour did not advance, she was bled, had a bath, and took some ergot of rye; but in spite of pains, which continued for thirty-six hours, no change was produced. After repeated attempts to apply the forceps, a second practitioner was called in, and he not succeeding better than the first, a

third, and afterwards a fourth, were summoned to her assistance. They having tried in vain to apply the forceps, it was determined to turn the child, and this was effected with great difficulty, from the violence of the pains, and liquor amnii having escaped thirty-six hours previously. The body and arms were extracted, but the head remained impacted, in spite of every effort to disengage it. A fifth practitioner was now added, but he was not more fortunate in his attempts. It being now midnight, they determined to let her rest till the morning, hoping that fresh contractions would flatten the head and expel it. Accordingly, the next morning, the head was found in the vagina, and easily extracted. The labia swelled, and the woman was attacked with metro-peritonitis and diarrhœa; general sloughing followed, large portions of ragged slough came away, and continued to do so until the 20th day. On seeing her nearly a month afterwards, M. Cazenave could find no traces of the labia, nymphæ, vagina, perineum, or anus; one large opening extended from the meatus urinarius to the coccyx, and showed that the recto-vaginal septum and lower part of the rectum were all destroyed; the urine did not pass by the urethra, and the fæces continually accumulated in the cloaca which had thus been formed. Further examinations showed, also, that about two inches and a half of the rectum had been destroyed, and that the neck of the bladder was obliterated. Under the use of topical bathing, injections, and mild nourishing diet, the health improved considerably, and the parietes of the huge cavity contracted somewhat; she was also enabled to pass a little urine by the urethra; still, however, the constant passage of fæces and urine by the same opening produced severe inflammation of the part, with all the sympathetic derangements attendant upon such irritation, the health began to fail, and in this state of suffering she has remained up to the present time.

[We need hardly say that perforation was most strongly indicated here, and would have doubtless spared her such dreadful suffering and injury: what object could possibly be gained by avoiding this operation? It could scarcely be any rational expectation of saving the child's life after so long and violent a labour.] —M. Cazenave has extracted a very curious and somewhat similar case from the American Journal of Medical Science for August: " A stout woman, ætat. thirty, was delivered of her first child after a labour of thirty-six hours' duration, during which she was bled, narcotized with opium, and lacerated with a pair of bad forceps: in four days after, the labia were found in a gangrenous state, for which cinchona and charcoal were ordered; this was followed by tympanites, which yielded to castor-oil, ablution with tepid water, &c. On the 19th day the fundus uteri descended into the vagina, and presented itself at the os externum, and with a little assistance on the part of the nurse the whole uterine apparatus was removed. The rectum separated at some inches above the pubes, and its lower portion depended between the labia; this the nurse also drew down in the presence of the practitioner, and it separated at the sphincter ani. On examining the parts, he found a single opening, extending from the coccyx to the pubes. Some time afterwards, the end of the intestine descended several inches, and hung loosely on the concave surface of the sacrum; a sponge was introduced into the cavity to support the rectum and prevent the access of air. The destruction of the parts was so complete, and the opening so large, as to bring within view the whole inner surface of the pelvis; in spite of which, after protracted suppuration, the wound cicatrized from behind forwards; her health gradually returned, and she would now feel quite well, but for the inability to retain her urine and fæces. In six months after labour the secretion of milk established itself, and lasted two months, without influencing the other functions."

L'Expérience. No. lxxxiv. *Fevrier* 7, 1839.

Case of Occlusion of the Neck of the Uterus during Parturition.
By FELIX HATIN, M.D.

MADAME D., ætatis thirty-six, pregnant of her first child, felt labour-pains during the day of July 12, 1837. I was sent for at ten o'clock, p.m., and found

the water had escaped, not in a single gush, but in small quantities, with successive pains; the uterine contractions were still slight, and occurring at distant intervals. I found the cervix flaccid, but much more prominent than usual, and could not discover any trace of orifice. At five, a.m., the cervix was effaced, but still no opening was to be found. I felt distinctly a round, smooth, voluminous mass plunging into the cavity of the pelvis, but nothing more. As no bad symptom appeared, I determined to wait until the tumour referred to should come down lower; it did so, and proved to be the fœtal head invested by the uterus. I felt convinced of the absence of the uterine orifice; but before practising the vaginal Cæsarian operation, I made the most minute search for the fissure through which conception and the escape of the liquor amnii had been effected. I felt a small spot, a little less resisting than the rest of the surface, and passed a female catheter up to it along the finger. By pressing gently, I carried this through the thin part, and then, substituting my finger for the instrument, widened the breach; the sensation produced was precisely that of a thin membrane tearing. The neck then dilated, the head presented in the first position, and, half an hour after, the labour terminated naturally.

[There is a deficiency of detail in this narrative; but the case furnishes a good example of the success of the treatment particularly recommended by Nägele.] *L'Expérience. Mai,* 1839.

MEDICAL JURISPRUDENCE, MEDICAL POLICE, TOXICOLOGY.

Case of a Person found Hanged, with Marks of Violence on the Head. By Dr. Heyfelder, of Sigmaringen.

On the 24th July, 1838, the deceased, who was confined in prison, was found dead, hanging in his cell, when only a few hours before he was seen to be in good health and spirits. The key being in possession of the keeper, no one could have had access to the cell. The cell, which was of large size, had one window, secured by an iron grating, about two feet from the floor.

On the sharp projecting part of the window-sill, traces of dried blood mixed with hair, corresponding to that of the deceased, were found: some streaks of dried blood extended from this point to the floor.

The deceased was hanging to one of the door-posts, his feet in contact with the floor, his head inclined on the thorax; both arms rigid and placed over the abdomen, and the fingers curved inwards. He had used a silk handkerchief for the purpose of suspension; and crossing this round the nape of the neck, had lodged the ends between the door and the post. The deceased's mouth was blocked up by his own pocket-handkerchief. He was dressed in the prison dress, and neither on this nor in the cell around were any marks of violence or resistance perceptible. On the forehead and left side of the head were traces of dried blood. He was about thirty-two years old.

The following appearances were found on making a medical inspection of the body:

In the middle of the left parietal bones there were two wounds, with lacerated and swollen edges, surrounded by dried blood. One of these wounds was an inch and a half long; the bones beneath were uninjured. On the right side there were several smaller wounds, all having a lacerated character, and one extending through the cartilage of the right ear. The mark produced by the ligature on the neck was about three fourths of an inch wide, the skin was brown, and here and there corrugated. This mark was situated between the os hyoides and the larynx: there was no ecchymosis in its course. The trachea and its main branches were filled with a white frothy mucous liquid. The lungs and right side of the heart were congested with liquid blood.

The medical opinion given, was, that the deceased had died by hanging, and that he had hanged himself. This opinion was founded on the following facts:

The wounds on the head, which were evidently inflicted during life, were too

unimportant in their character to be connected with the death of the deceased. They were superficial, and neither the bones nor the contents of the cranium had suffered from the violence which had produced them.

The state of the trachea, lungs, and heart indicated death from asphyxia, brought on, undoubtedly, by hanging, assisted by the occlusion of the mouth with a handkerchief. The operation of this double cause would have rendered death more rapid than in common cases of hanging. The absence of ecchymosis, from the mark produced by the ligature, might lead some to doubt whether the deceased had really died by hanging: but it is now well ascertained, that an ecchymosed mark is by no means a necessary accompaniment of suspension during life.

It now remains to be explained, how the facts warranted the idea of suicide:

On the supposition of this having been a case of murder, it must be assumed that the deceased was violently seized by his assailants. his head knocked repeatedly against the projecting window-sill: and when thereby stunned, he must have been dragged to the door, strangled with his own cravat, and afterwards hanged. Against this view there were the following circumstances: 1, The prison was so secured, that no one could possibly obtain admittance without the knowledge of the keeper; 2, the absence of all marks of resistance or struggling in the prison or on the person of the deceased, who was in the prime of life, and well able to resist any murderous attack; 3, the tranquil state of the countenance, with the course of the ligature around the neck. Strangulation, while in the erect position, the feet being supported, might seem to militate against suicide, did we not know that there are many cases of suicide on record, in which the deceased have been thus found.

The wounds on the head were doubtless inflicted by his knocking this part of his body against the sharp edge of the window-sill in previous attempts at suicide. Not succeeding in these, he suspended himself with his cravat, having placed the handkerchief in his mouth, in order to accelerate death.

Henke's Zeitschrift. 1 H. 1839.

[REMARKS.—We agree with the reporter of this case, that it is well deserving of being made generally known; since, had the body of the deceased been discovered under other circumstances, a well-founded suspicion of murder might have arisen. When a man is found hanged, and circumstances show that he has not died by hanging, this is alone presumptive of murderous interference. How cautious, then, should we be in judging from circumstances! There are, perhaps, even now, professional men who would declare, from the non-production of ecchymosis by the ligature, and the fact of a hanged man being found with his feet in contact with the floor, that such a person could not have died by hanging. An opinion of this kind would not certainly be expressed by one who had attended to the subject, for these questions have long been settled by experience: but there is little doubt that many, without being aware of their error, would not hesitate to give this opinion at inquests. Wounds found on the head, and a handkerchief filling up the mouth of a deceased person, are, however, circumstances which might mislead even an experienced practitioner. The wounds, in the case before us, were, it is true, not of any great extent; but they were in an unusual situation for self-infliction, and did not possess the characters of those commonly found on the persons of suicides. The other fact was still more striking: we do not remember to have ever met with a case of suicide by hanging, in which the deceased stopped up his own mouth so as to prevent respiration. In some future criminal trial, a witness may be asked whether such an act as this is possible in suicidal hanging? This case informs us, that it is not only possible, but has actually been accomplished.]

Signs of Death from Hanging. By M. Orfila.

In our last Number we gave some account of M. Devergie's observations on this subject. M. Orfila has now answered them in the First of a Series of Six Memoirs on subjects of Medical Jurisprudence, which he has presented to the Royal Academy of Medicine. The question he proposes is this: Is it true that we can determine, by the state of the genital organs, whether suspension took place during life or after death? The facts which M. Orfila advances to prove the negative are full of both physiological and legal interest.

You have not forgotten, says M. O., that M. Devergie has read to the Academy a note relative to two signs of suspension during life: viz., the presence of semen in the urethra and the congestion of the genital organs. The observations, on which I deny the sufficiency of these signs, are as follow: 1st. Not only is it possible to find the zoospermata in the urine, which is *first* passed an hour after seminal emission, but even eight, ten, or twelve hours after. Semen, therefore, remains in the urethra of those who have had an emission, till they pass urine, and hence in a case of natural death, or of death by poison, &c., if, for the purpose of deception, the body were hanged after death, and the patient had not passed urine between the time of a seminal emission and death—in any of these cases, the presence of semen in the urethra would be of no value in deciding whether the hanging preceded or was subsequent to death.

2d. I have examined the urethra in the bodies of five persons who had died of different diseases, and had remained lying on their backs, and I have constantly found semen in it. A man, æt. sixty-eight, was examined four hours after death from cancer; the urethra was full of a viscid, lightly amber-coloured liquid, in which there were a great number of seminal animalcules. A man was examined eight hours after assassination; on pressing the urethra, three drops of a milky liquid were obtained, in which a considerable number of animalcules were discerned. In a man of twenty-one, who died of consumption, and was examined thirty hours after; in another, aged forty-six; and in a third, aged sixty-two, examined three hours after death, animalcules were also found in the mucus of the urethra. In five other subjects, only mucus and urine were found in the urethra. The preceding facts sufficiently prove, that the animalcules may be found in the urethra of persons dying of various diseases, and that they are, therefore, of no value as signs of death from hanging.

The next series of examinations relates chiefly to the congestion of the organs of generation. A man, æt. twenty, was examined seven hours after death. The skin of the penis and scrotum was scarcely coloured; the penis, close by the scrotum, was three inches and five lines in circumference. The body was placed upright, and kept in that position for seven hours; the lower extremities were then found very much injected, the penis and scrotum deep violet-coloured, and the former had increased seven lines in circumference. A man, æt. fifty, was hung four hours after death from phthisis, and remained hanging for sixteen hours. Before suspension, the trunk and limbs were still warm and pale; the genital organs were pale, except the prepuce, which had a violet tint; the penis was four inches and a quarter in circumference. After the suspension, the legs were violet to above the knees; the vena dorsalis penis and its principal branches were very evidently distended; there was no erection, but the circumference of the penis was increased four lines. There was a large drop of turbid liquid at the orifice of the urethra, which contained a number of dead seminal animalcules. A man, æt. fifty, was examined three hours after death; the lower extremities were pale, the penis three inches and one line in circumference, and, as well as the scrotum, uncoloured; the orifice of the prepuce was full of a viscid liquid, containing a prodigious number of seminal animalcules. The body was hung, and eight hours after, the scrotum and penis were violet, the circumference of the latter was increased seven lines, and the meatus still contained animalcules. In a fourth case, a man, æt. forty-nine, was hung five hours after

death, and remained suspended three hours and a half. The penis, which had before been slightly turgid, was now *erect*, and formed almost a right angle with the abdomen: it had increased nine lines in circumference, it was violet-coloured, and all the veins about it were very much distended. The vesiculæ seminales were very full, and at the orifice of the urethra there was a drop of viscid fluid, containing a great number of animalcules, of which many were alive.

The conclusion which M. Orfila adopts from these facts is, that although it is true that in cases of hanging during life, semen containing some living animalcules is most commonly found in the urethra after death, and the genital organs are so congested, that there may even be erection, yet we should be cautious of concluding, as M. Devergie would, from these circumstances, that the body has been hanged during life; for it is not rare to find semen in the urethra of those who, after dying of various diseases, have remained lying on their backs: and, on the other hand, we may, by hanging bodies even three or four hours after death, and leaving them in that position for some hours, produce a considerable congestion of the genital organs and even erection, and may find animalcules in the urethra, many of which may still be alive. This sign, moreover, is of the less value, since it has been already observed after several other modes of death besides that from hanging.

Bulletin de l'Académie Royale de Médecine. Sept. 15, 1839.

Case of Poisoning by Rotten Eggs. By Dr. MARCHAL.

DR. MARCHAL was summoned, at eight o'clock one morning, to a carpenter, who was supposed to have had an attack of apoplexy. He learnt that the man had eaten freely on the previous evening, and that since midnight he had been in a state of coma, from which it was impossible to arouse him. He was at this time lying on his back; his face was rather livid, especially about the eyes; his lips were blue; his eyes open and fixed; there was no distortion of the mouth; the limbs were flaccid; the pulse small and regular; the respiration slow. His wife, his brother, and one of his sons were affected, though in a less degree, with the same symptoms, complaining of vertigo, weight and pain in the head, pains in the limbs, and disinclination to move. When questioned on the suspicion of their having taken some narcotic poison, the patients stated that on the previous evening they had all eaten of a pudding, made with frozen eggs, (the thermometer was, at the time, 8° below zero, Centigrade,) and to these they attributed all their symptoms. They added, that the internal surface of the shells was greenish, and that the eggs had a disagreeable slightly putrid smell. It was only a short time after their meal that they all felt indisposed. The man himself was largely bled from the arm, but without relief. Leeches and ice were applied to his head, and large mustard poultices to each calf; but it was not till after three hours that he began to arouse from his state of narcotism, and to recognize those around him. He had afterwards no recollection of what happened; he only remembered that at the beginning of his sleep he was tormented with frightful dreams. He recovered completely, but rather slowly; and a slight paralysis of the left little finger remained for a considerable time.

On the evidence of four persons being similarly affected, after eating eggs in which putrefaction had commenced, though it was probably retarded by their being frozen, and the resemblance of their symptoms to those which are known, from the observations of Chaussier and others, to result from the application of sulphuretted hydrogen gas, there seems little reason to doubt that the gas generated in the decomposition of the eggs was the common cause of the simultaneous seizure of the patients. [We beg leave to question this explanation.]

Gazette Médicale. Juin 22, 1839.

Suspected Poisoning:—a warning case. By Dr. Steinbeck, of Brandenburg.

Several years ago, says Dr. S., a case occurred to me of great interest in a pathological, but especially in a medico-legal point of view; for if a little less accurate examination of the body had been made, a judicial murder might have been very readily committed. A young and strong ploughboy was taken, while out washing sheep, with the most frightful pain in the region of the stomach, and died in *eight hours* with symptoms which led to the suspicion of poisoning. This was the more readily entertained, as the young man had two sweethearts, and had long wavered in his choice. She who was slighted was known to be a very violent and revengeful girl, and it so happened that this very person gave the patient some drink shortly before he was taken ill. The chemical analysis of what the patient vomited and of the contents of the stomach and intestines showed, however, no trace of poison. But there was found in the stomach a hole, the size of a shilling; and it appeared that the stomach had been adherent to the spleen, and that by forcible separation of this adhesion, the hole in the stomach was caused, and death thereby brought on. It was now told that the deceased had challenged another to box, that he had again refreshed himself from the young woman's pitcher, that then he fought with everybody, and was thrown to the ground, whereupon he cried out. After this, he got up without saying a word, pale (with anger, according to the bystanders), and went to his work. On account of the raging thirst which now set in, he drank several times from the said pitcher,—a circumstance which he himself gave as the cause of his illness to the judicial authorities shortly before his death. *Casper's Wochenschrift. May* 18, 1839.

On the Poisonous Effects of Copaiba. By M. Sandras.

A man, æt. 25, contracted a gonorrhœa, for which he was treated by an advertising quack. In a few weeks the discharge ceased. He entered La Pitié on the 13th August, for a pulmonary affection, which was soon cured; but his urethral discharge returned. The *potion de Chopart*, which contains copaiba, was prescribed for him; but it was discontinued, after two days' use, and an enema was ordered, containing half an ounce of the balsam. This was soon followed by pain at the stomach, vomiting, and general uneasiness; on the following day epileptic convulsions occurred, to an alarming extent; for three days he was unable to speak; it was necessary to tie him in bed, to prevent his sustaining injury during the convulsion. Cupping-glasses and moxas were applied by the side of the cervical vertebra; ice to the head; sinapisms to the feet and hands; and calomel was administered freely. Under the use of these means the convulsions ceased, and the nervous symptoms subsided. Twenty-five days after the attack he was detained in the hospital merely by the suppuration of the moxas. He went out perfectly well at the end of two months.

In this case no other cause could be assigned for the invasion of the symptoms than the administration of the copaiba. We must, therefore, place it among those instances of peculiar idiosyncrasy in which we find substances, frequently the most innocuous, become poisonous to particular individuals.

Bulletin général de Thérapeutique. Feb. 1837.

II. THE AMERICAN AND COLONIAL JOURNALS.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

On the use of Arsenic in Chorea. By D. M. Reese, m d., Professor of Medicine, Albany College, New York.

These results of experience have led me successively to test the comparative merits of the various other remedies which have been reported as successful, as the rubigo ferri, cuprum ammoniacum, sulphate and flowers of zinc, oxide of bismuth, camphor, electricity, nitrate of silver, &c., and after very considerable opportunities, I have learned to rely upon the tonic powers of *arsenic* in preference to any and all other medicines of this class; and having never known it to fail in effecting a radical and permanent cure, I feel great confidence in recommending it to the profession in all those cases in which the tonic plan is decided upon, either with or without preliminary medication. The arseniate of potash, as existing in the formula of Fowler's solution, is the preferable mode of exhibition, and its dose should be graduated according to the ability of the stomach to receive it without nausea. In the most numerous subjects, varying from seven to sixteen years of age, six or eight drops should be given night and morning, gradually increasing its dose and frequency. Adults may take ten drops increasing to fifteen, and even twenty, three times a day, and in all cases it should be persevered in for a week or more after all the spasmodic symptoms have disappeared. The only unpleasant effects are nausea or vomiting if the dose be too large, and occasionally a tumefaction of the head and face if too long persisted in. On either of these effects being produced the medicine should be discontinued for a few days, and then resumed in a diminished dose. With these precautions, I have, for a number of years, employed this remedy in over two hundred cases of chorea, without ever having witnessed any of the untoward results upon the constitution, said to follow the exhibition of arsenical preparations. So far from any of the sequelæ, which are apprehended by the alarmists, my patients have, under its use, not only recovered from chorea, but many of them seem to have since acquired vigorous constitutions and improved health. I have used it in infancy, in delicate females during pregnancy, and under circumstances supposed to forbid its employment by many, and I have never once seen cause to regret its exhibition.

But while I thus dwell upon the value of arsenic as a remedial agent in chorea, I have not failed to superadd auxiliary treatment, and hence nutritious diet, active exercise, cordial drinks, and the cold bath, both plunging and showering, have all been usefully employed. And, in the examples, comparatively few, in which counter-irritation has been indicated, I have obtained most favorable results by the aid of rubefacients, blisters, setons, issues, and the eruption of the tartrite of antimony, applied to the spine. But I have never had occasion for active depletion even by purgatives, although when constipation exists in connexion with chorea, or where there has been present the suspicion of worms, I have interposed a cathartic or anthelmintic, while, at the same time, persevering in the tonic treatment. *New York Journal of Med. and Sur. Oct.* 1839.

SURGERY.

Report of Cases of Un-united Fracture, treated at the New York Hospital. By John S. Heard, m.d., late House-Surgeon.

The chief object of the following report is to offer to the profession an account of a new mode of treating un-united fractures of the extremities, first practised in the New-York Hospital, and, so far as my knowledge extends, still confined to the practice of that institution.

[The author, after stating the general causes of non-union, and the objects of treatment, passes in review the different methods heretofore employed: viz., blisters; friction of the extremities of the bones; rest combined with pressure; salivation; seton; introduction of a silver wire between the fragments; injection of stimulant liquids; application of caustics; sawing off the extremities.]

All these different methods having occasionally failed in effecting bony union,—it occurred to Dr. J. Kearny Rodgers, one of the surgeons of the New-York Hospital, that if, after removing the extremities of the fragments and exciting their periosteum, he could keep them in apposition for a certain period,—firm osseous union would ensue. He accordingly, after sawing off the extremities of the fragments, drilled a hole in the end of each, and passing a silver wire through, drew the bones together, and kept them in co-aptation until firm union was effected. Subsequently, Drs. Mott and Cheesman, colleagues of Dr. Rodgers, were likewise successful in inducing bony union by the same operation. This operation has not, we believe, been adopted in any other place than the New-York Hospital. Indeed, the only notice of it that I have been able to meet with, is in a report of a clinical lecture by Mr. Liston, in which he makes these remarks: "A proposal has, I think, been made, to denude the ends of the bones, drill holes in them, and connect them by a silver wire. It is too artificial a mode of proceeding, and I should fear, that from exposure and bruising of the ends necessary for accomplishing the object, (for perforation cannot be made unless by denuding the bone freely, and holding it securely by some contrivance until the object is accomplished,) that necrosis, instead of union, would be apt to follow." Now, what Mr. Liston means by "too artificial a mode of proceeding" is not very apparent: in what is it more artificial than Mr. White's method? In the simple passing of a silver wire: and is an operation, although successful, to be abandoned, because it is artificial? Surely this rule would have a very limited application in surgery. And as regards "necrosis instead of union" being more likely to follow, it will be sufficient to state, that this operation has hitherto never failed of success. And are we to refuse to adopt a sure plan of treatment, although it be severe, for one which, though less so, still, occasionally, is of no avail?

[The paper is illustrated by the narration of several cases. We can only afford room for the first two.]

Case I.—George Westerfield, aged 15, admitted into the New-York Hospital, under Dr. Rodgers, July 25, with an un-united fracture of the right os brachii, about two inches above the elbow-joint. This accident was the result of a limb of a tree falling upon his arm in Ohio, in December, 1824. From his account, it appears that a medical man reduced the fracture immediately, and placed splints on the arm, which was borne in a sling. At the expiration of twelve days, the dressings were removed, and the union, which he seems to have expected, not having taken place, the ends of the fractured bone were rubbed together, and the splints re-applied. This treatment was persevered in several months. In June, 1825, a seton was passed between the ends of the bone, and withdrawn in a week; splints were worn for four weeks, but no union ensued. He remained without treatment from this time until his arrival in New-York, in June, 1826; in the latter end of June, a seton was passed by a surgeon of this city, and retained for six months, motion being prevented by splints applied to the arm. The seton having been fairly tried, and removed without any beneficial result, the patient almost despaired of ever recovering the use of his arm, but was willing to submit to any expedient which might be proposed likely to bring about union. It appeared to Dr. Rodgers, that but one other mode offered any prospect of success, and that was Mr. White's plan of sawing off the ends of the bone, and reducing the parts to the state of a recent compound fracture. On the 31st of July, Dr. R. made an incision three inches in length, through the integuments of the arm, on the outer edge of the biceps muscle, down to the bone. The lower end was easily turned out of the wound, and half an inch sawed off. The upper end could not be sawed off in the same way; but having guarded the soft parts by a thin slip of wood passed down on each side of the bone, about half an inch was removed by a circular saw. It being found impossible to bring the ends of the bone in contact, in consequence of a slip of muscle passing between them, this was divided; but the fragments, although placed in apposition, soon regained their former bearings an inch and a half asunder. Apprehensive that he should be foiled if the fragments were

allowed to remain thus far apart, Dr. R. drilled a hole into the medullary cavity through the shell of each end; through these holes a silver wire was passed, and the ends of the bone retained in co-aptation. The ends of the wire were drawn through a canula, which remained in the wound. The os brachii was much softer than natural, and it was feared that this state would prevent any ossific deposit. The wound was dressed with adhesive straps; and the arm, enveloped in splints, was placed on a right-angled splint properly hollowed out.

August 15th. The canula fell from the wound with the loop entire, so that the bone must have broken away; the bones however continued in proper position; not expecting bony union for some time, the arm was not examined for a month; during this time the wound had almost healed.

October 8th. Sixty-nine days after the operation, Dr. Rodgers was gratified to find that the bone had united, but he was unwilling to test its strength by force.

October 16th. On examination to-day the limb was found to have united perfectly. The patient was not allowed to move from his bed for two months after the operation.

December 3d. The patient was presented to the clinical class, with every motion of the arm perfectly regained, the os brachii being two inches shorter than its fellow.

CASE II.—Charles Grill, born in Germany, aged 26, a cutler, was admitted into the New-York Hospital, September 28th, 1838, with a simple fracture of the radius of the right forearm, about two inches above the wrist joint. The injury was caused by his hand being caught in machinery and bent forcibly back. There was considerable contusion of the whole forearm, and some laceration of the hand. The fracture of the radius was very oblique, and the lower end of the upper fragment projected nearly through the integuments. The fracture was reduced, and the patient being placed in bed, the arm was laid in a flexed position on a pillow, covered with oil silk, and lot. evap. applied. As soon as the state of the arm would allow, the requisite pads and splints were applied, and the arm supported in a sling. At the expiration of four weeks from the application of the splints, they were removed, but no union had taken place. It was now suggested that a strip of muscle interposed between the fragments was the cause of non-union. A splint, with a hand-piece setting off from the body of the splint at an angle of about 110°, was now applied upon the palmar side of the forearm, in order to elevate the lower fragment. The interosseous pads being only applied between the ulna and this lower fragment, the usual splint was placed on the dorsal side. The arm was thus supported in a sling for six weeks; at the expiration of which period, upon removing the apparatus, there was still no union. The same apparatus was replaced, and the patient put under the use of mercury, but it produced profuse diuresis, and was discontinued. The splints were allowed to remain applied until January 10th, when Dr. Rodgers made an incision about two and a half inches in length, over the seat of fracture on the palmar side of the forearm; upon exposing the fragments, the radial artery was found running between them; the lower fragment was drawn down to the ulna, while the upper one was separated near an inch from it, a slip of the extensor carpi radialis muscle intervening. The radial artery was divided and secured at both extremities, the intervening muscle was severed, and the ends of the bone removed with the bone pliers. A hole was now drilled into each extremity, and a silver wire being passed through, the bones were approximated by twisting the wire, the end of which was allowed to hang from the wound. The edges of the wound being brought together by adhesive straps, the ordinary pads and splints were applied, and the arm suspended in a sling. A slight attack of erysipelas occurring, required the confinement of the patient to bed and the removal of the splints; upon the subsidence of the erysipelas in a few days, the splints were re-applied.

February 2d. The wire came away to-day the loop being entire. The splints and bandages were continued until April 2d, when, upon removing them, the union was found to be perfect. He remained in the hospital, using passive motion, until April 15th. *New York Journal of Med. and Sur. Oct.* 1839.

III. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING NOVEMBER 30, 1839.)

ANATOMY AND PHYSIOLOGY.

On the Influence of Madder on the Bones of growing Animals. By JAMES PAGET, Esq., Demonstrator of Morbid Anatomy, and Curator of the Museum at St. Bartholomew's Hospital.

[THIS paper is valuable, not so much for supplying fresh materials to physiology, as for enabling us (a thing of almost equal importance) to make a proper use of former materials. We can only find room for the introductory and the concluding paragraphs.]

The value of experiments made for the purpose of observing the growth of bones, and more remotely the general process of nutrition, by feeding growing animals on madder, has been, at different times, very differently estimated. They have been supposed by some to afford the only certain proof of a constant change of particles in nutrition, while by others they have been considered so vague and uncertain as to admit of no profitable application; and it would be easy to quote from the highest physiological authorities expressions of such doubt, with respect to the conclusions to be drawn from them, as would at once justify any attempt to explain their difficulties and apparent contradictions.

The general result of these experiments, then, is that which Du Hamel stated—that the increase in the length of bones is effected by the growth of those parts which are not yet hardened. Ossification commencing in the middle of the shaft, and proceeding rapidly towards its extremities, it is only in very young animals that any increase in the middle can be perceived; the later increase of length is due to the addition of osseous matter to the parts not yet hardened; and at the latest periods of growth is effected entirely by the ossification and reproduction of the intermediate cartilage between the shaft and the epiphysis.

In their applications to the explanation of the growth of bones, experiments with madder are thus proved to be conclusive: with a knowledge of the principles on which they depend, and with a careful avoidance of some sources of fallacy that have been pointed out, they may be employed with entire confidence; they afford to the observer the means, as it were, of branding with his own mark every particle of phosphate of lime that is deposited during a given time.

Their applications to the general doctrine of nutrition are less certain: for, in the first place, it is not clear that the laws which govern the production and maintenance of the earthy and inorganic matter of bones are applicable to the nutrition of soft parts; and if they be, the result of the madder experiments is at present, in this point of view, entirely negative, if not opposed to the idea of a constant mutation of particles. Seeing that the blood containing madder has no evident influence on the phosphate of lime already deposited in the bones, and that, on the other hand, blood not containing madder does not possess any chemical power to abstract the colour from the coloured phosphate of lime, it would appear as if there were no influence exerted during the state of health between the contents of the capillary system and the earthy matter already existing in the bones. And as, moreover, the bones of old animals are coloured by madder only to that degree which appears explicable by their increase of density, it would seem as if, in the state of health, or in what I would call the state of *nutritive equilibrium,* the earthy matter of bones undergoes no change.

London Medical Gazette. Nov. 15, 1839.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

On Alkaline Indigestion. By R. D. Thomson, M.D.

[Abstract of a paper read before the British Association at Birmingham.]

The author stated that he discovered this form of dyspepsia in 1835, and had communicated the results of his observations to the Medical Section of the British Association at Bristol. Since that period he had continued his researches, and had confirmed the accuracy of his first results, by the examination of a very large number of cases. It has been long known that in stomach complaints fluids are frequently ejected from that viscus into the mouth; and it has been by examining the chemical constitution of those fluids that the author has been enabled to simplify, in some considerable degree, one of the most disagreeable forms of dyspepsia. Dr. Thomson divides the fluids which he has detected in these complaints into *acid*, *alkaline*, and *neutral*.

1. The *acid* state is familiar to most persons. In the natural, there is no doubt that during a certain period of the process of digestion, the contents of the stomach exhibit an acid reaction; that is to say, that litmus paper, dipped in the fluid existing in the stomach, becomes red; that the fluid tastes acid, and that when distilled over, a quantity of pure water having been previously added, the fluid which passes into the receiver exhibits a faint acid reaction. This does not occur, according to Schultz, however, during the first half hour or hour of the process of digestion. The acid would, therefore, appear to be generated by the process. The discussion with respect to the nature of the acid, Dr. Thomson stated that he would reserve for the Chemical Section. When this natural acid, however, as it may be termed, accumulates to a certain extent, symptoms of disease exhibit themselves, in the form of a burning sensation at the pit of the stomach, with acid eructations, which do not, however, alleviate the pain. This is the characteristic symptom of *acid indigestion.*

2. The second form of indigestion, indicated by the fluid ejected from the stomach, Dr. Thomson terms *alkaline indigestion.* It is characterized by violent pain in the region of the stomach, accompanied, frequently, with headach and faintness, with a sensation of spasm or contraction in that viscus; the sensation goes on increasing, till it frequently becomes intolerable, and, at last, when the agony is complete, the patient is suddenly roused by a determination to the mouth of a large quantity of fluid, which must be immediately evacuated, to give place to a succession of similar occurrences; at last, however, the flow of fluid becomes so abundant as to constitute an actual stream; it continues to flow for some time, but gradually diminishes in quantity, and at length ceases, and with it the pain in the stomach. The latter is the characterizing symptom of *alkaline dyspepsia*, or *pyrosis*, as it has been frequently termed. But hitherto it has always been confounded with other forms of indigestion. Dr. Prout has published an account of his examination of the fluid of *pyrosis*, and has stated that it was acid; the fluid, however, was not procured by himself, but was sent him from one of the hospitals, where the mistake was very likely to occur. This form of indigestion occurs much more frequently than is generally imagined. Dr. Thomson stated, that out of forty or fifty patients daily seen at the Blenheim-street Dispensary, in London, he generally met with one or two affected with symptoms of this description. It frequently occurred in coincidence with affections of other organs—as of the uterus, liver, &c., and was often of such a pressing nature, that it required more of the skill of the medical man than the original disease. Certain it was, that it was absolutely necessary to treat it with as much care as the original complaint; and if the action had been allowed to go on for some time unchecked, the secondary affection became as firmly fixed as the original disease which had induced it; so that after the removal of the latter a second disease, as firmly rooted as the first, required to be taken under

the physician's care: the treatment consisted of the administration of acids, tonics, and narcotics, which required to be prescribed with care, otherwise the *acid indigestion* was frequently induced, which was as difficult to eradicate as the alkaline form.

3. The last form of indigestion, as indicated by the fluid ejected by the mouth, which the author had met with, was a *neutral* state, which was of much rarer occurrence. Dr. Thomson had, however, met with several cases, and had succeeded in overcoming the disease by the use of tonics. *Lancet. Sept.* 21, 1839.

Observations on the Treatment of Acute Rheumatism by Opium.
By D. J. Corrigan, m.d., Physician to Jervis-street Hospital.

[Ever since the publication of Dr. De Roches' paper on the subject, in the first Number of the Edinburgh Medical and Surgical Journal, (April, 1805,) we have been advocates for the use of opium in certain cases of acute rheumatism; and we could adduce some striking cases in favour of this treatment. Still, our experience does not justify our recommending it as always successful, or always the best practice. Dr. Corrigan's paper contains important additions to our previous experience. We can only find room for some of the practical remarks, and one of the eight cases recorded in it.]

The treatment by opium shortens the duration of the disease; it enables the patient to pass through so painful a disease with comparatively little suffering; it husbands his strength, so that when convalescent he rises from bed with comparatively little depression of vascular energy and muscular strength; and lastly, it diminishes very remarkably the tendency to the occurrence of such complications as pericarditis, endocarditis, &c.

The most important rule to be remembered in employing opium for the cure of acute rheumatism is, *that full and sufficient doses* shall be exhibited. I have heard of opiate treatment having disappointed some who have tried it. On enquiry, I have learned that in those cases it has been given only to the extent of a grain every fourth or every sixth hour. This is not "the treatment of rheumatism by opium;" it is making the patient worse than before; it is inflicting on the patient the mischief arising from the stimulant effects of the drug, and withholding from him all the benefits of its sedative influence. The opium should always be increased in dose, both in frequency and quantity, until the patient feels decided relief; and should be then kept up at that dose until the disease is steadily declining. The first indication that tells the practitioner he has reached the proper dose, is, the statement of the patient, who in reply to an enquiry as to how he has passed the night, probably says that he has not slept, but that he is free from pain and feels comfortable. This effect having been attained, the opium may then be continued in repetitions of the same dose as to frequency and quantity.

I think about ten or twelve grains in every twenty-four hours, will be found the average quantity required. The tolerance of the remedy is a remarkable feature in the treatment, and may, I think, be fairly adduced as an argument in favour of its propriety. The head is not affected by the large quantity of opium administered. This is remarkably shown in Dr. Aldridge's case, where the head was not injured by the opium, even though there had been previously a tendency to derangement of cerebral functions in all previous febrile affections. There is another singular circumstance connected with the exhibition of the opium. It is the occurrence of diarrhœa, while the patient is using the opium even in full doses; in some instances, the diarrhœa becoming so troublesome as to require starch enemata, or chalk mixture, with kino. It is seldom necessary to purge the patient while administering the opium; indeed the pains are sometimes brought back by the administration of a purgative, either from the patient catching cold in rising from bed, or from the irritability produced by the action of the purgative. The patient's bowels, if they have not been constipated in the

commencement of the attack, may be not only safely, but with benefit, not disturbed more than once in two days.

CASE VI.—On Wednesday, 24th April, I visited my friend Dr. Aldridge, in a very severe attack of the disease. Nearly all the large joints were swollen and acutely painful, and the pains were shifting from joint to joint. The pulse was 120; the tongue was very foul, but moist: the want of rest, from the agony of the pains, was most distressing. The attack was of three days' duration when I first saw him. I immediately put him on the opiate treatment; he first got one grain every two hours; the quantity was then increased to a grain every hour; and this was continued for thirteen days, with the administration of an occasional purgative. On the fourteenth day he began to take the mist. guaiaci c. sulph. quinæ; and on the fifteenth day he was walking about his parlour, complaining only of not being as strong as usual, but free from pain and swelling; and he described the treatment as most grateful to his feelings, the pains being so much lulled by the opium, that he passed through the attack with very little suffering. In reply to some inquiries of mine, as to his impressions relative to the opiate plan of treatment, he favoured me with the following:

"*Mount Michael; Glasnevin.*

"Dear Doctor,—You ask me for my recollections of rheumatism; they are not very agreeable, but I owe you the performance of a task, more disagreeable than that of recalling the remembrance, as some return for the comfort your treatment afforded me when labouring under the reality.

"I suffered eight years previously, under two successive attacks of rheumatic fever, from each of which my recovery was tedious, extending through a period of some months.

"During the month of April this year, my strength was much exhausted by the exertions of the preceding winter. I was low-spirited, weak, and hypochondriac. The first warning of a coming attack consisted in a sensation of occasional faintness; a feeling of uneasiness about the region of the heart, as if its throbbings were about to cease; then came, during the two following days, coldness, shiverings, general soreness, and the other agreeable forerunners of fever. I had recourse to my usual panacea for all ills—a warm bath—but found myself much worse after it. Acute pains began to nestle themselves, particularly in the small joints. The next morning I drew some blood from my arm; but getting no relief, I requested that evening your attendance.

"You came, and found my ankles and wrists swollen enormously; while I writhed under a succession of exquisite aches, now in one joint, now in another, to which the pain of toothach is elysium. You afforded me, however, some comfort by telling me my heart was untouched; and the primæ viæ requiring to be cleansed, you directed me to take some purgative medicine; which having performed its office, you desired me to commence taking one grain of opium every second hour.

"I confess that I was somewhat afraid of what appeared to me very large doses of this powerful drug, especially as my head always had a tendency to be affected whenever I had fever of any kind. It was therefore with some misgiving I obeyed you, but soon had reason to congratulate myself on the effects of your advice, for during the remainder of my illness, i.e. from the second day after being forced to succumb, the pains, although they visited me occasionally, were by no means so intolerable; I slept much, my intellect remained clear, except when occasionally I took an overdose of the opium, (for as soon as I began to experience its good effects, I became quite enamoured of it,) and, in fine, I was enabled to walk down stairs the fourteenth day after taking to bed. During another week, I rubbed such joints as were occasionally painful with a liniment made with sulphur and camphorated oil, and took internally quinine and guaiacum; but since then, now during a period of four months, I have not had the slightest return of the disease. As nearly as I can recollect, I swallowed during my illness about two hundred grains of opium.

"Such are my reminiscences of rheumatism. To the influence of large and repeated doses of opium in relieving its agonies and shortening its duration, I gladly and gratefully bear testimony; but I must confess, that whatever may be my veneration for the 'lights of science,' I have not the slightest ambition of again personally testing its efficacy.

"I remain, &c. &c., J. Aldridge."

Dublin Journal. Nov. 1839.

Outlines of a Case of Acute Anasarca and Convulsion after Scarlatina.
By Marshall Hall, M.D., F.R.S., &c.

The patient was a boy, aged twelve. Sixteen days before, he had gone through scarlatina in its very mildest form; he had scarcely been confined to bed, and had not suffered from the nimia medici diligentia; he had appeared quite well. On this Sunday morning he was seized with swelling of the face, which came on and increased equally suddenly and rapidly. With this symptom there was the appearance of sudden and serious collapse, and soon afterwards convulsions followed by coma.

When I first saw my patient, there were convulsions followed by deep coma; wine and brandy on the table indicated sufficiently the previous state of the case. I felt persuaded, in spite of these appearances, that the only hope was afforded by relieving the vascular system within the head, and yet that the measure was not unattended with danger. This view was freely explained to the boy's father, who very sensibly said, he confided his son's life to the hands of his medical advisers.

We placed the patient upright, and Mr. Duffin opened the jugular vein. I kept my finger on the pulse, whilst we allowed twenty ounces of blood to flow! The convulsions ceased, and the coma diminished, but did not disappear. We then ventured to open a vein in the arm, and abstracted seven more ounces of blood! In less than an hour, the little patient knew his parents. We prescribed calomel and purgative medicine; a cold lotion to the head, and fomentations to the feet; afterwards leeches were applied; but the bloodletting was *the* remedy to which the amendment was obviously due. The little boy recovered forthwith, and what is important, without the least symptom of the morbid effects of loss of blood.

I must here remark, 1st, that acute anasarca and convulsions may, and frequently do, follow the mildest cases of scarlatina, perhaps because, after such cases, less precaution is usually taken to clear the bowels; 2d, that in all cases of acute anasarca there is the danger of an affection of the head, arachnitis, coma, or convulsion; and 3d, that in such cases the remedy is bloodletting,—bloodletting until relief and security are obtained. *Lancet. Nov.* 30, 1839.

Clinical Remarks on some Cases of Liver Abscess presenting externally.
By J. G. Malcolmson, M.D.

We recommend the perusal of this paper to all our brother practitioners in India. It contains many valuable observations on the pathology and treatment of one of the most frequent and fatal diseases of that country. The principal objects of Dr. Malcolmson are, to point out the not unfrequent occurrence of gangrene in cases of hepatic abscess, when opened by the surgeon; and to enter his protest against the very injurious practice, so prevalent of late years, of giving calomel in too large doses, and under circumstances when its action is most injurious, as when abscess is fully formed. Dr. M. also clearly shows that this practice, as also the equally injurious one of extreme depletion by the lancet, was too common with practitioners in India, in the early part of the present century. *Edinburgh Journal. October,* 1839.

SURGERY.

Case of Popliteal Aneurism, successfully treated by Pressure. By E. G. BRUNKER, M.D., Surgeon to the County of Louth Infirmary.

OWEN MATTHEWS, aged thirty-two years, was admitted into the Louth Infirmary, on the 20th August, 1839. His occupation for some years past had been very laborious, having been employed on board a ballast vessel in the Thames. His attention was first accidentally attracted to a pulsating tumour in the left ham, fifteen months ago, but he cannot recollect any particular sensation at its first appearance. He continued to labour as usual for some time after. On admission, he had all the appearance of rude health, and on close examination, no disease of any internal organ could be discovered. Respiration natural—pulse 72, regular—cordiac sounds healthy—has not had palpitation.

On examining the ham, the entire space was observed to be completely filled up, by a pulsating tumour, beating synchronously with the heart—soft—compressible—but becoming distended on the removal of the pressure. The pulsation was very perceptible in the whole extent of the tumour. No *bruit de soufflet*, or other morbid sound was discoverable. Pressure being made on the femoral artery in the groin, the pulsation in the tumour instantly ceased, and as immediately returned on its removal. During the pressure, the size of the tumour was sensibly diminished, but resumed its former dimensions when the circulation was restored. No pulsation discoverable in either anterior or posterior tibial arteries. The patient did not experience any pain in the tumour, but merely an uneasy sensation in some of the toes and foot, which were occasionally cold and numb. He was ordered to remain in bed, and was placed on low diet.

Two days after admission, a piece of dry sponge was placed over the aneurism, and retained in situ by a roller rather loosely applied.

On Tuesday, 27th August, the infirmary was visited by Dr. Foss, of the 38th regiment, to whom I showed the case. On removing the bandage and sponge, I was greatly surprised to find that the tumour was much diminished in size, and that no pulsation whatever could be discovered, either by the hand or the stethoscope. The tumour itself was much firmer, and did not fill up the popliteal space, as it did when last examined, two days previously, by me as well as by my friend, Dr. Browne, of this town. The pressure was ordered to be continued, and the utmost quietness in the recumbent posture observed. The patient, a few days after this, left the hospital, contrary to my wish and advice.

September 12th. I this day saw Matthews, who says he has kept himself very quiet since he left the hospital, and has continued the use of the sponge and bandage. On removing them, I could scarcely discover any traces of the former tumour in the ham, and no pulsation whatever was to be felt. There is a very obscure pulsation in the anterior tibial artery, which could not before be discovered on very careful search. The uneasy sensations about the foot, give him now very little annoyance, and he walks without pain or lameness.

I have no doubt in my own mind, that the moderate pressure occasioned by the sponge and bandage, have caused the total obliteration of the sac of the aneurism, and thus this patient has been saved the risk inseparable from the application of a ligature to the femoral artery, which operation I should have had recourse to, had not the more simple means I thought it but fair to make trial of in the first instance been attended with the success I have mentioned.

Dublin Medical Press. Oct. 16, 1839.

STATISTICS.

Statistics of Poisoning in England.

[We are indebted for the following important digest to a respectable provincial newspaper: it is taken from a parliamentary document, entitled, "Returns from the Coroners of England and Wales of all Inquisitions held by them during the years 1837 and 1838, in cases where Death was found, by Verdict of Jury, to have been caused by Poison." We regret, with our contemporary, first, that the Returns are not complete, some Coroners having neglected to comply with the request of the Commons; and, secondly, that the Coroners who have made Returns have, in many instances, omitted particulars of great moment. The following particulars are derived from the returns of 41 counties and 31 boroughs only. The total number of deaths, it will be seen, are 543, viz. 282 males, and 261 females.]

Arsenic 184
- Taken by a girl, disappointed in love 1
- By a girl, in a fit of passion . 1
- By a girl, in a fit of jealousy . 1
- By a girl, who had robbed her master's son 1
- By a girl, seduced and deserted by a married man . . . 1
- By a girl, subject to fits and despondency . . . 1
- By pregnant girls, to destroy themselves 5
- By a pregnant girl, to procure abortion . . . 1
- By a pregnant girl, deserted by her lover, who was suspected to have procured her the poison . 1
- By a pregnant girl—how or by whom administered not known . 1
- By a wife, separated from her husband 1
- By a young woman, married unhappily, and separated from her husband . . . 1
- By a cook maid, distressed by the death of a friend . . 1
- By an insane mother and two children—administered by the former 3
- By five children, to whom it was administered by an insane mother 5
- By a man, embarrassed by debt, and disordered in mind and body 1
- By men, through reduced circumstances, pecuniary embarrassments, &c. . . . 6
- Taken through drunkenness 12
- By a farmer and innkeeper, who, having had a handsome legacy left to him, spent it in riotous living—got into debt—and took poison to escape his creditors . 1
- Through poverty . . . 3
- Through despondency . . 1

Arsenic.
- In lunacy . . 52
- In food, by accident . . 7
- In mistake, by young people, in food prepared for vermin . 5
- In mistake, by a married woman, who, having mixed it with oatmeal for vermin, was innocently supplied by her husband with food prepared from the mixture . 1
- By accident, the deceased having tobacco and arsenic loose in the same pocket . . . 1
- In mistake for cream of sulphur . 1
- Administered to a child in mistake for magnesia . . . 1
- ——— Queen's cordial . . 1
- Taken inadvertently, in various ways 3
- Administered wilfully . . 5
- How administered not known . 2
- Felo de se . . 20
- Taken without cause assigned in the report . . 36

Opium 42
- Overdose, taken by adults in ignorance . . . 11
- Overdose, administered to children by mothers and nurses . . 8
- Administered to a child in mistake for other medicine . . 1
- Supplied by a deaf druggist for manna, and administered to a child by an ignorant nurse . . 1
- Administered to a child, found dead in the Trent, extensively bruised, (the poison and the wounds both sufficient to account for death) . 1
- Taken by a child in ignorance . 1
- Taken through drunkenness . 2
- Through lunacy . . . 9
- How administered not known . 1
- Felo de se . . . 2
- Taken without cause assigned, &c. 5

Laudanum 133
Administered by mistake . . 2
——— for antimonial wine . 1
——— for paregoric . . 2
——— for Godfrey's cordial . 2
——— for syrup of buckthorn . 1
——— for tincture of rhubarb . 4
Sold at a druggist's for antimonial wine—the druggist not bred to his trade, and kept two shop-girls, one of whom (the coroner ascertained) gave twice as much laudanum for a penny as the other . 1
Taken by adults as medicine 11
An overdose, taken by a drunken surgeon 1
Taken by mistake for a surgeon's draught . . . 1
Administered to children in mistake 2
Drunk by a child, within whose reach the phial had been left . 1
Given by a child to an infant, to allay coughing, in the mother's absence 1
Overdose to infants by mothers and nurses . . . 26
Taken inadvertently . . 7
Through despondency . . 4
Through drunkenness . . 9
Through dissolute conduct . 1
Through lunacy, induced by want . 2
Through do., from various causes 30
Through loss of situation . . 1
Wilfully administered . . 2
How administered not known . 3
Felo de se 4
No cause assigned, &c. . 14
Cough Syrup . . . 1
Overdose, given by a mother to her child 1
Syrup of Poppies . . . 5
Overdose, administered to children by mothers and nurses . . 5
Godfrey's Cordial . . . 12
Overdose, administered to children by mothers and nurses 10
Administered to children by mistake for syrup of rhubarb . 2
Infant's Mixture (most probably a preparation of Opium) . . 1
Overdose, given by a mother to her child . . . 1
Morrison's Pills . . . 1
Taken as medicine . . 1
Tartar Emetic . . . 2
Three drachms, taken to cure the ague 1
Overdose, given to an infant . 1
Colchicum 3
Overdose, taken for the gout . 1
Taken as medicine . . 2
Mixture for vermin . . . 2
Taken by children, within whose reach it was left . . 2

Hellebore 1
Taken by a "temporary lunatic" . 1
Mercury 2
Taken by a "temporary lunatic" . 1
Felo de se . . . 1
Bichromate of Potash . . 1
Eaten ignorantly by a child . 1
Aquafortis 2
Drunk by a child within whose reach it was left . . 1
Taken in temporary lunacy . 1
Oxalic acid 19
Taken by a woman, who had quarrelled with her husband . 1
By a person of defective intellect 1
Through lunacy . . . 8
Through drunkenness . . 1
Through want of employment . 1
By a young woman, on the emigration of her brother . . 1
By a child within whose reach it was left . . . 1
Without cause assigned . . 5
*** It is singular, that nearly the whole of the cases of poisoning by oxalic acid occurred in Middlesex
Nitrate of Silver . . . 1
By a child (swallowed percussion caps) 1
Castor-Oil Seeds . . . 1
Taken inadvertently . . 1
Fungus 4
Eaten for mushrooms . . 4
Rum 1
Ignorantly given to a child for inflammation of the bowels . 1
Extract of Lead . . . 1
Found in solution by a woman, and given to her child in mistake for ginger-wine . . . 1
Essential Oil of Almonds . . 4
Taken in lunacy . . 2
Without cause assigned . . 2
Prussic Acid and Arsenic . . 1
Taken in lunacy . . . 1
Arsenious Acid . . . 1
Taken in mistake for a purging powder 1
Acetate of Morphine . . . 2
Administered in mistake for other medicine . . . 2
Strychnine (the active principle of Nux Vomica) . . . 2
Taken by a child, to whose father it had been sent as a medicine . 1
Lunacy 1
Nux Vomica 3
Taken in ignorance of its effects . 1
Procured by a girl of weak intellect, and given to her father, who had sent her for an emetic . . 1
Taken without cause assigned . 1

Wolf's Bane		1
Eaten by a child, who found it in his father's garden	1	
Black Ashes		1
Procured for washing, and eaten by a child	1	
Sulphate of Iron (Copperas)		1
Taken to procure abortion	1	
A Vegetable Poison		3
Taken by two children (brothers)	2	
By an adult	1	
Hicrapicra		1
An overdose, taken in gin	1	
Monk's Hood		1
Gathered by a poor old man, and eaten in mistake for celery	1	
Savine		1
Taken to procure abortion	1	
Infusion of Hemlock		1
Overdose, taken by a woman	1	
Laudanum and Prussic Acid		1
A case of lunacy	1	
Potash		1
Taken by a child	1	
Medicine		1
Administered to an infant—intended for an adult	1	
Muriate of Tin		1
Taken by a child in mistake for vinegar	1	
Cantharides		1
An embrocation, containing tincture of cantharides, administered to a child in mistake	1	
Laudanum and Aquafortis		1
Lunacy	1	
Carburetted Hydrogen Gas		2
Inhaled during sleep, through an accidental escape of gas	2	
Belladonna (Deadly Nightshade)		2
Taken by mistake	1	
Without cause assigned	1	
Paregoric Elixir		2
Overdose, administered to children	2	
Decoction (nature not exactly known)		1
Taken by a pregnant girl, with the supposed intention to procure abortion	1	
Nitrous Acid, with Aloes		1
Taken without cause assigned	1	
Cayenne Pepper, &c.		1
Cayenne pepper, essential oil of Cayenne, and bark, taken in alcohol, as a remedy for the ague	1	
Turberth Mineral		1
Taken in mistake	1	
Sulphuric Acid (Vitriol)		32
Swallowed by children, ignorantly	9	
——— for ginger beer	3	
Administered to children, for Godfrey's cordial	4	
———a child for castor oil	1	
——— for syrup of rhubarb	1	
——— for some medicine not named	1	
Accidentally sold for Godfrey's cordial, and given as such to a child	1	
In a drunken fit	1	
Through insanity	5	
Through family quarrels	1	
By a woman, who thought herself forsaken by God	1	
Without cause assigned	4	
Hydrocyanic (Prussic) Acid		27
Taken by surgeons, depressed in mind by reduced circumstances	2	
By a surgeon, delirious from scarlet fever	1	
By a surgeon, addicted to drinking	1	
By a surgeon, in a fit of frenzy	1	
By druggists, deranged	2	
By a medical student, affected by over-study	1	
By a child, in ignorance	1	
By a gentleman, reduced from affluence to poverty, and deranged	1	
Through disappointment in love	1	
Through lunacy	9	
Without cause assigned	6	
Corrosive Sublimate		12
Taken incautiously as medicine	1	
By mistake, for cider	1	
In a fit of passion	1	
Through despondency	1	
Through lunacy	5	
Felo de se	2	
Without cause assigned	1	
Poisons not Specified		14
Taken accidentally	2	
By a drunkard, in mistake	1	
Case of miscarriage, the mother having received some noxious drug	1	
Taken without cause assigned	2	
Through lunacy	7	
How administered not known	1	
		543

The Gateshead (Newcastle) Observer, Nov. 2, 1839.

PART FOURTH.

Medical Intelligence.

MEDICAL REFORM.

For upwards of thirty years has the attention of the medical profession been directed, with little intermission, to the necessity of obtaining from the legislature some reform of its condition regarded as a branch of civil polity. About the year 1807, Dr. Harrison, then of Horncastle, in Lincolnshire, excited very general attention to the subject of medical reform; and from that period to the present it has been the theme of discussion. This must suffice to show that the propositions for reform made at the present day cannot justly be regarded by any as taking the profession by surprise. If there be any who can now feel surprise at this subject being agitated, the regard of such persons for the general welfare of the profession must be slight, else they could not be ignorant, either of the manifold evils which beset it, or of the reiterated endeavours made to unite its members in an appeal to the legislature for the reforms needed. For the diversity of opinion which prevails on the necessity that exists for any reform, a brief explanation may suffice. They who thirty years ago advocated reform are either dead or superannuated, or, through long despondency of success, are become indifferent to what ceases to be to them a subject of any personal interest. A new generation has sprung up to whom the subject is less familiar, and many of whom have acquired predilections and preferences for certain corporate institutions by which their judgments are biased, and which disincline them to any change by which their favourite colleges would be lowered in estimation or influence, or their own private interests affected. Many of the latter, it is to be feared, will, on the present occasion, coalesce with the anti-reformists, or so coldly espouse their own cause as rather to clog the wheels of reform than to expedite their movements. In order that all may have the opportunity of viewing the subject in its several bearings, we purpose presenting to our readers a succinct but, we trust, accurate sketch of what requires to be known in order to form any rational judgment on the subject of medical reform, namely, the nature and extent of the evils which exist; the endeavours hitherto made to correct those evils; and the proposed measures by which it is contemplated to effect their extinction. With this summary, which, we trust, will put all concerned in possession of, at least, the elements of this question, we purpose dismissing it, for a time, from our pages;—leaving the regeneration of the profession to that body whom it most concerns, and who, if inclined to support their own interests, must be fully competent so to do—namely, the collective profession.

In order to have a clear conception of the present condition of the profession, it is necessary to look back for some centuries to the time when anything like a legalized medical profession was first constituted in these kingdoms. And this retrospect is the more necessary from the extraordinary fact that, by laws passed in the fifteenth century, does one most important branch of the profession continue to be governed at the present day! To the establishment of the London College of Physicians by the charter of Henry VIII., and the confirmation of this by subsequent statutes, may we assign the first commencement of a legalized medical profession in these kingdoms. At this period physic was paramount, and surgery only a subordinate branch surrendered to the fostering care of the barbers. Yet was not the latter wholly disregarded, for the charter of Henry gives to the college a power of licensing in surgery as well as in physic. On the policy pursued by this body in administering its chartered powers, it would be needless and irksome here to dwell. Suffice it to say that, instead of combining and consolidating even that branch of the profession which it was

specially appointed to superintend, it limited the powers and privileges of the corporation to a few, placing the great mass of physicians who consented to acknowledge its authority in the subordinate and degraded class of Licentiates, endued with no corporate rights, and deriving from their connexion with the college only leave to practise their art, which the license of the college graciously permitted them to do, "saltem in nonnullis curationibus." A large portion of regularly-educated physicians have at all times dispensed with its license, and, upheld by the public, have practised without its authority, although in so doing they have acted illegally, and rendered themselves liable to pains and penalties on the information of any *qui tam* informer. So far as this branch of the profession is concerned, it assuredly needs some reform.

Surgery, as a distinct branch, was much later in receiving civic recognition and corporate rights than physic, for not until 1745, were the surgeons separated from the barbers, by 18 Geo. II. c. 15; and their incorporation as a Royal College did not take place until 1800. Much has been done by this body to improve the profession of surgery, but its chief efficiency has resulted from the moral influence which it possesses, beyond which it has no power, having no authority to interfere with the surgical practice of any one who, however unqualified, may choose to practise the art. Such defect alone furnishes decisive evidence of some reform being necessary in this body also.

Both these corporate bodies, in attending to what they deemed the duties and interests of their respective faculties, overlooked a class of the profession more numerous than both the others united, so essential to the public as to be of rapid increase; and, though not recognized by any chartered company, yet firmly established in the free exercise of their calling; the peculiarity of this class being that they dispensed medicines as well as prescribed. This class, now well known under the denomination of General Practitioners, was composed of all varieties; of surgeons who were compelled, in self support, to combine pharmacy with their more immediate art; of apothecaries, equally obliged to prescribe as well as dispense; even physicians have found it expedient to hold the doctorate in abeyance, and engage in the more certain and more profitable occupation of general practice. But this class being open to all who choose to enter it, no qualification being legally enjoined, its unprotected state excited general interest, and a legal constitution was given to it by the statute of 1815, called the Apothecaries' Act. The history of this measure claims grave consideration. The parties who originally sought it were anxious to procure the sanction and co-operation of the established medical institutions, and, accordingly, they applied to the Colleges both of Physicians and of Surgeons, to aid their endeavours; but both declined, the views which then prevailed causing both these bodies to disdain any connexion whatever with pharmacy. The preposterousness of this objection on the part of the surgeons was sufficiently egregious, for their own body had long supplied a large proportion of the general practitioners of the kingdom. In yielding to a false pride on this occasion, too, they inflicted a signal injury on the whole body of their own licentiates; for these, who, previously to the passing of the Apothecaries' Act, in 1815, had full power to practise pharmacy, were, after its enactment, restrained by it from all practice of pharmacy unless they possessed the license of the Apothecaries' Company, which license could only be obtained through apprenticeship, a given course of study, and formal examinations. It is clear that if the College of Surgeons had deemed the subject worthy of their notice, they must have had sufficient influence to procure the insertion of a clause into the Apothecaries' Act exempting their licentiates from its operation. Having neglected so to do, the consequence has been that all who have become general practitioners since 1815 have been obliged either to undergo a double examination, in order to obtain the license of the Apothecaries' Company as well as the diploma of the College of Surgeons, (legal necessity always requiring the former, and regard to public opinion frequently the latter,) or to content themselves with the Apothecaries' license only, the obtaining of which involves no examination in

surgery, and the possession of which, consequently, implies no necessary knowledge of this branch of the science! The proportion of general practitioners who have entered into practice since 1815, who are licentiates of the Apothecaries' Company only, is very considerable, as every one knows. Such being the present state of this department of the profession, it must be allowed that it, too, stands in need of some reform.

But if each department of the profession thus prove to be defective, and in need of amendment, it is difficult to conceive the grounds on which any member of the profession could venture to assert that no reform is required. Every member must belong to some one of the departments mentioned; in his own department, at least, he must be sensible that all is not perfect; and unless his judgment be that acknowledged imperfection ought not to be rectified, he must unequivocally admit that some amelioration ought to take place. The hesitation occasionally evinced to acknowledge that reform is at all needed, proceeds, we suspect, not from any want of conviction of some reform being required, but from disinclination to acknowledge any plan of reform as suitable, save that which would aggrandise the special department to which the individual belongs. Each would readily allow reform to be expedient if the reform were confined, or even more especially directed, to the evils which beset his own department; but finding more comprehensive views prevail, and the interests of other departments advocated as claiming just support, he shrinks from bringing into hazard the rights and privileges which his own department possesses, and, unable to foresee all the consequences of change, he decries all reform. Many ingenuous minds yield to this feeling, and are perfectly honest in so doing. They, by a very natural partiality, are accustomed to deem their own department the most important of the whole; so regarding it, they as naturally consider that any damage sustained by it must be an injury to the collective profession, nay, to the whole community; and, from their best feelings, and with conscious integrity, they denounce all measures for a general reform as rash and hazardous. We trust that by showing each department of the profession separately to be imperfect, and in need of amendment, we have sufficiently established the main principle on which all reforming projects require to be founded, namely, that some reform of the profession is needed.

Our next enquiry might be into what each department has yet done towards perfecting itself even within its own limited sphere, to which alone have any endeavours been hitherto directed, no attempt having ever been made to bring the several branches into union and harmony with each other. But it would be a profitless task,—for we trust, ere we close this brief article, to show that no amendment of separate institutions which zeal, judgment, energy, and sincerity united could effect, would answer the end which the well-being of the collective profession, and the interests of the community imperatively demand. The old arrangement of the profession, derived from influences foreign to either its natural tendencies or its beneficial applications, comprised physicians to prescribe for internal diseases,—surgeons to deal with maladies calling for manual dexterity,—and apothecaries to dispense the prescriptions of both. Analogously to what may be observed in various other social constructions, the form of the edifice remained long after the appropriation of the several constituent tenements had varied immeasurably from their original destination. At present, we suppose, it is hardly within the memory of any one to recall the time when the foregoing adjustment of professional duties was strictly adhered to. Surgeons have long found their most considerable, and most profitable occupation to consist of the practice of physic rather than of surgery; and apothecaries have as successfully combined the practice of both physic and surgery with their more immediate art. Is this a ground of reproach? Far from it; both surgeons and apothecaries, in yielding to a public necessity which they could not, if they would, control, have but discharged their highest duty, that of ministering to the public welfare; and their triumphant encroachments on the department of the physician only prove how powerless are legal provisions and

collegiate restrictions, when not based on the only sure foundations of sound principle and the public good. Sound principles give no sanction to the older division of labour in the medical profession, at least none to the extent to which it was proposed to be carried; nor was the public benefited by it. The public could not recognize any sufficient grounds for the distinctions made between physic and surgery, the human body, with all its properties and attributes, physiological and pathological, being the subject on which both were to be exercised. They could see no propriety in having a surgeon to take charge of a fractured limb, and a physician to minister to the fever which the accident occasioned; and they acted as common sense directed,—for, as the physician was not prepared to treat the fracture, they obliged the surgeon to grapple with the fever. Nay, they have done more; for, not content with requiring the surgeon to act as physician, they have, in a wide range of practice, forced him to become apothecary also, so far as to dispense the necessary medicines to his own patients. They who charge surgeons and apothecaries with having culpably and fraudulently trenched on the physicians' province are guilty of a great mistake, and of actual injustice. By no wilfulness, or fraudulent encroachment, could the change have been effected, if the public will and the public necessities had not enforced and rendered it inevitable. Speculations, however, on how the general practitioner of the present day has been called into existence, are out of date; the fact being, not only that this class actually exists to a wide extent, but that it is now legally recognized, and protected by legislative enactment far more perfectly than any other department of the medical profession.

Such being the indisputable facts, it is strange that any can be found to regard a renewal of the old system practicable, or even desirable. Yet are there several, chiefly among the class of physicians, who, yearning for a return of the good old times, really imagine that the old system could be revived, and whose beau ideal of medical reform would be the re-establishment of the physician, surgeon, and apothecary (*i. e.* druggist), each restricted to his own special functions, as was presumed to be the case in the earlier period. Far different is the reform of the profession now indicated. No reform, indeed, can have a chance of permanency, unless, while it provides for the welfare, efficiency, and respectability of the profession, it also adapts itself to those wants of the public which have been so unequivocally demonstrated: namely, by supplying an adequately qualified class of general practitioners.

Before discussing the special measures of reform which now seem most expedient, it is necessary to advert to one more circumstance, which, above all others, renders a legislative reorganization of the profession a matter of indispensable necessity, namely, the various and incongruous modes by which medical practitioners now procure admission into the profession. So numerous are these, that testimonials of qualification are now attainable from sixteen different sources, if not more. Where the object is the simple one of providing for the public a practitioner qualified to treat the several derangements of health to which the human body is subject, such a diversity in the primary qualification is not only unnecessary but absurd. But, unfortunately, it is mischievous also; for, of each qualifying body, the interests depend on the number of candidates qualified: and, though on speculative grounds it might seem that, in the competition so excited, superiority of system and discipline would be sure to attract towards it the greater number of candidates for medical honours, decisive experience has proved that the facility of obtaining these honours has far greater weight in determining the choice; and that, in the main, that institution which grants its testimonials on the easiest terms will be sure to supply the greatest number of practitioners. On the effect of such competition in lowering the tone of the profession by deteriorating the quality of the members composing it, it is needless to descant. Such rivalship, although it may commence with high-toned feelings, and the laudable ambition to merit a preference by the superiority of the advantages afforded, yields almost of

necessity to the force of more sordid considerations, and ends in bartering its wares for pecuniary gain, instead of honorable and elevating fame. This is no imaginary surmise, but a real fact; it having been explicitly acknowledged at the great medical congress held in Dublin in May, 1839, that the Dublin College of Surgeons, which had long prided itself on the superiority of its discipline, and the strictness of its examinations, had been actually compelled to abate in both, in order to prevent the Dublin schools being deserted for others where testimonials were of easier attainment. Should a system, not only leading to such evil, but actually enforcing it, be suffered to continue, it needs no prophetic inspiration to predict that the final issue must be the degradation of the profession, retrogression in professional competency and acquirement, and consequent injury to the commonweal.

By no means can this progress of evil be arrested, save by establishing a minimum of qualification, without which no member can be suffered to undertake the responsibilities of medical practice; and, in order to ensure this minimum being acquired, by ordaining that qualification for practice shall be conferred by one examining and licensing body only, in each division of the kingdom, uniformity of qualification and equality of rights and privileges being established throughout the whole. All entering the profession through the same course of instruction and examination, all would, of course, have their general competency adequately proved. In order to afford due encouragement for still higher cultivation, and for the exercise of superior talents, there should be a higher grade, to which those ambitious of it might ascend. Such an arrangement would fulfil all that has ever been accomplished by the old system, while the higher class, generated by the proposed plan, would have the signal excellence not only of high literary and scientific attainments, but the still more valuable requisite of maturity of practical knowledge.

These were the considerations which led the committee of the Provincial Medical and Surgical Association to recommend, and the Association itself to adopt, the late petition presented on their behalf to both Houses of Parliament, in which they pray for the establishment of one examining and licensing body for each division of the kingdom. Without such consolidation, it was the clear conviction of the framers of this petition, that no reform of the profession through mere modification of the existing institutions could be of the slightest avail; and believing this to be the grand requisite which could furnish the only sure foundation of any system of effective reform, they limited, and wisely as we conceive, the prayer of their petition to this one point.

Such a measure would not alone supply every want of the profession, but it is essential as a groundwork for every other amendment, and, with it gained, the profession could afterwards, and, with little aid from Parliament, would be equal to work out its own regeneration. The examining and licensing bodies, having no direct interest in medical schools, would recognize those only which could furnish proof of their capacity; and the profession educated under such a system, could be at no loss afterwards in providing for efficient self-government. For this purpose it would be only required to create one general Medical Institution or College, or whatever might be its name, for each division of the kingdom. This might in the first instance be formed by unions of the present Colleges of Physicians and Surgeons; and as all legally qualified practitioners should be admissible as a matter of right, and enrolled on verifying their testimonials, no unjust or capricious exclusion could take place. The self-elective system, too, in the appointment to offices, which obtains in all our present Colleges, to their own detriment and the scandal of the profession, could be readily superseded by a more popular mode of election, in which the elective franchise might be limited to practitioners of three, five, seven, or ten years' standing, or in any other way that mature reflection should most approve. It may be imagined that by depriving the existing institutions of the right to confer the primary qualifications, they would be rendered inert and useless. But so far from this being the case, release from the unsuitable office of examining

and licensing is absolutely necessary for allowing them to attend to manifold duties which ought to engage their attention. It is apprehended, we know, that by withdrawing from existing institutions the right of examining and licensing, these institutions would, in the loss of their present fees, become ruinously crippled in their finances. But all such fear must be groundless; for if a general medical institution or college were to discharge the many important duties which would of necessity fall to its share, adequate support must be furnished to it from some source or other, as certain, at least, as that from which the present incomes of the several colleges are derived. As all medical practitioners would necessarily become enrolled in such colleges, a moderate introductory fee would raise a large fund in the first instance; successive enrolments would add to this; and should deficiency of means be, nevertheless, experienced, the services rendered to the state would amply warrant a claim on the public revenue, which no statesman would reject.

There is one more view of the subject yet to take, which may possibly reconcile one class of objectors to the kind of reform now contemplated. The old system of physician, surgeon, and apothecary would in fact be revived, having only superadded to it the all-important class of general practitioners. Many physicians and surgeons of course would not engage in pharmacy: therefore apothecaries would still be needed; and these apothecaries are, in fact, already numerous, being the chemists and druggists of the present day, who are precisely what the apothecaries originally were in days of yore. Why were the apothecaries withdrawn from their proper functions, and elevated into medical practitioners? Simply because the want of the public for a general practitioner, though clearly manifested, was not supplied by those institutions which ought to have discerned the want, and provided for it. Having overlooked the want, having even doggedly refused to acknowledge it when pointed out to them with express solicitation for their aid in supplying it, the apothecaries had no alternative but to fill the chasm thus wilfully left void,—and from 1815 to the present day, they have done so in a manner which reflects on the Apothecaries' Company the highest credit.

But ought it to be inferred from this that the present Apothecaries' Company is the body from which the general practitioners of the kingdom ought to emanate? We hesitate not to answer most determinedly in the negative. The Apothecaries' Company ought not to have the powers which they now possess; and the legislature will greatly fail in its duty, if the qualification of the general practitioner be not transferred to a less exceptionable source. In this transfer, no injustice will be done to the Apothecaries' Company. Antecedently to 1815, the associated Apothecaries had no province, save some superintendence over the drug trade. Accident, or rather the apathy and supineness of the Medical and Surgical Colleges, required them at that time to undertake a higher function,—and meritoriously have they discharged it. But the legislature which assigned to them this novel office is not only at full liberty to transfer it to other hands, but bound by every sense of public duty so to do. In resuming their old functions this company would find ample occupation, for greatly does the drug trade of the kingdom need enlightened superintendence; and in correcting its errors and abuses the Company of Apothecaries would find enough to do. By the members of this company—new modelled, perhaps, and with new powers, and under a new name, as a College of Pharmacy,—should every dispensing chemist and druggist be examined and licensed; and to them, as the most competent judges, should all inspection of the drugs kept by dispensers be assigned. For such duties their capabilities would be acknowledged by all; and the faithful discharge of these duties would be a national blessing. Should any one, endued with peculiar foresight, surmise that a change similar to what took place among the older apothecaries, namely, that of conversion into medical and surgical practitioners, would still go forward among the chemists and druggists, our reply is, that such would be the difference of circumstances that no such tendency on the part of the latter need be apprehended. The apothecaries

became general practitioners, because the pressure of public necessity forced them into an unoccupied void. An adequate class of general practitioners being provided, there would be no void into which the chemists and druggists could pass. Prescribe they would, and ever will, behind their counters; nor could any legislation, however stringent, or penal, prevent this. But they would not, to any extent, engage in actual practice: the competition of a numerous, well-qualified, and active body of general practitioners leaving them no prospect of the slightest success in any such attempt.

We have thus presented to our readers briefly, and, so far as our own knowledge extends, faithfully, the elements of a question which, regarded in its bearings on the well-being of the community, is second in importance to none. Earnestly do we recommend to all classes of the profession to give to it that dispassionate and mature consideration which the magnitude of the interests involved demands.

REFORM OF THE LONDON COLLEGE OF SURGEONS.

Since the preceding observations were in type, an excellent letter has appeared from Mr. Key (in the *Medical Gazette* of Dec. 6), on the necessity of reform in the constitution of the London College of Surgeons. Did our space permit, we should willingly transcribe the whole of this communication, which, we doubt not, will produce an impression commensurate with the high character, distinguished abilities, and elevated professional station of the author. We can only find room, however, for a small portion of it; but the extracts will prove, by the highest possible evidence, the justness of our preceding remark, that "this department, also, stands in need of some reform." Mr. Key's views, it will be seen, are much less comprehensive than ours; but we gladly hail the accession to our cause of so able an advocate, although he may be, as yet, prepared to go only a part of the way with us. We cannot, however, but hope, from the liberal and clear views displayed in Mr. Key's letter, that a further consideration of the subject will lead him to regard the reform of the College of Surgeons as only a part of the general reform, which we conceive to be indispensable for the honour and welfare of the profession. If the College of Surgeons and College of Physicians would only condescend to read the signs of the times, and deign to be wise, even now in this their day; instead of opposing, they would cooperate with all honest reformers in bringing about the contemplated and inevitable changes: otherwise, they will probably lose even the scientific and departmental influence which would naturally attach to them, and the possession of which, as sections of one body representing the whole profession, would not be incompatible with the most thorough reform.

.... "Can it be said that the College is popular with the profession? The members, it is true, seek to obtain its diploma, because without it some public appointments cannot be held; and, till lately, no other chartered body has had the power of conducting surgical examination. The degree of popularity that it does enjoy is owing to the individual weight of one or two examiners, and not to the whole as a body. Is it popular with the country? The answer is, that a petition to parliament for such powers as had been granted to the Apothecaries' Company was made, and refused; and the profession saw in silence the failure of the application. On occasions when the proceedings of the College have been assailed without regard to truth, and misrepresented and distorted to serve party purposes, not a voice has been raised in its defence. The Royal College of Surgeons is sometimes pledged as a toast at a public dinner, but more as a matter of form than of feeling; and then something cold is said by way of compliment, and something as cold replied by a member of council, as the expression of thanks for the honour conferred; its prosperity excites in its members no feeling of pride, as its degradation would none of sympathy.

"Why is the College, with its talent, respectability, and great resources, regarded by the profession with so cold, so jealous an eye? It is because the

members have no community of feeling with the College. The student, when he becomes a member on receiving his diploma, and leaves the door of the building, feels that he has no closer connexion with it than before his name was enrolled. He asks himself, and asks in vain, what advantage he derives from the fee that he has paid, and from the severe self-imposed examination that he has undergone. His hopes and fears, once centered in the College, end with his examination. In truth, he can hardly be said to have any connexion with the College. If he chance to live in London, he may hear, if he please, an occasional lecture, and may have access to the library—advantages that his former school of medicine still continue to afford him. It is no *Alma Mater* to him. He may long for its honours, but they are beyond his grasp; he feels that no industry, no exertion of talent, can place them within his reach. As he advances in life, he sees his former fellow-student, possessing half his industry, talent, and knowledge, called to the council of the College, from which he is excluded, and raised to the rank of examiner, which he never can attain. He has no voice in the election of the council, much less in its measures; he feels himself a cypher, if not an alien, and that he is so regarded by the College. Surely there is nothing in this system to attract, but everything to alienate from the College, the affection of its members....

...."By what rule a member is elected to the council I do not exactly know. To be a London surgeon, and not to be a general practitioner, seem to be two of the necessary qualifications. He need not be a surgeon to an hospital, for persons are selected who are known only as private practitioners. He need not be a surgeon of high professional reputation, for some are of the council whose scientific attainments will not bear a close scrutiny. There is a conventional rule, I conclude, not well defined, but sufficiently understood, to guide the election of the members of the council. Thus entrenched, the governing body of the College knows that it is not popular, and endeavours to atone by legislation for the defects of its system. The exclusiveness that prevails mars their best intentions, leading to all kinds of evils, both to their own body and to the profession; among others, to measures of doubtful policy, in order to conciliate popular feeling. The two last alterations in the laws of education will illustrate what I mean. One is, the abolition of apprenticeships, and the limitation of the period of study to four years. Those who brought forward such a law must surely have forgotten how much, or rather how little, they themselves knew at the end of four years; that the increased severity of their examination requires rather a prolonged than a curtailed period of study: they might also bear in mind, that it takes as much time to make a good surgeon as it does to make a good carpenter. But for this error there is an excuse, as it would seem to be only a show of liberality; for the law is wholly inoperative, while the hall retains its period of apprenticeship. The other is, the committing education wholly to provincial schools. It cannot be said that either of these measures is calculated to raise the standard of education; but they are both popular, and so far answer their purpose. One is probably intended to conciliate the good will of students; the other, that of provincial surgeons; and the end is thought to be gained. The policy is short-sighted. The council cannot suppose that the obvious inference from their own law is overlooked by provincial surgeons; namely, that if their schools are qualified to conduct the whole of education, and to be placed on an equality with the London schools, the leading provincial surgeons ought to be placed on a footing with the London surgeons, in respect to eligibility to the council: and why should they not? Besides, all these small acts of legislation, as expediency may drive, cannot have a lasting good effect: they have the semblance of liberality, but not the substance. The surgeon and the student still remember that they are excluded from the honours of the College, and, forgetting the boon, think only of what is withheld from them. To meet the coming difficulty, the College has three modes of proceeding....

..."They may apply at once to parliament to strengthen their powers, and to make the examination, which at present is voluntary on the part of the student,

compulsory. ..... Another line of policy is, to wait the tide of events, and the demolition of the frail fabric of the university [of London.] . . . . . A third course, by which alone its strength can be permanently increased, is to rest its claim for support on the attachment and esteem of its own members. This can only be done by allowing to each member a vote in election of the members of council. Such a measure would make the College what it has not yet been—a representation of the whole body of surgeons, instead of being, as it now is, a small self-elected section, taking into its own hands the management of its internal concerns, and the legislation for the whole of the profession. The council and the members would form one powerful body, acting in unison, for the common good; the feelings of the members, now diverted from the college, would be concentrated towards it. They would join in measures for its advancement, and in defending it against attacks, which, if made, would then be few and feeble; honorable ambition would be encouraged; and those who work for the profession would replace those who do nothing for science. The libeller would be silenced, and the discontented would be satisfied. Thus strengthened, the College might apply with confidence to parliament for any reasonable extension of powers. The council would then embody the surgical talent of the country in a most popular form. . . . . .

. . . . "Objections that will be urged against such a measure are such as are made against popular elections in general. An election of members of council by a learned body cannot be termed popular, in the objectionable sense of the term. A popular election is defective, from the passions or interests of the electors giving them an improper bias, or from ignorance of the qualification necessary for a member of the council. The interest and feelings of the member would be identified with the College, and no improper motive would be likely to interfere with a sound exercise of their privilege. . . . . .

. . . . "It may be said, that the leading members of the profession would not submit to the annoyance of a canvass and a popular election, and would not come forward as candidates; while men of doubtful pretension, by force of intrigue and noise, might be preferred. It might be so ordered, that the names of candidates should be laid before the profession for the period subsequent to the last election, in order to give time for the relative qualification of each to be ascertained. I am far from intending to lower the standard of qualification for a member of council; on the contrary, my wish is to raise it; nor am I at all desirous of depreciating the character of the hospital surgeon, as I cannot be insensible to the opportunities that surgeons long connected with hospitals possess, of cultivating the science and practical part of the profession, and of qualifying themselves in a superior manner, for the discharge of the duties of an examiner. If surgeons of hospitals do their duty towards the profession, by improving its science and raising its character, the profession in return will not fail to elect them, as the fittest persons, to the council of the College. But this fitness, I say, let the members determine.

"Other advantages to be expected from such a change, are not few nor unimportant. The present want of exhibitions will probably be supplied; a motive for enriching the funds of the College, for such an excellent purpose, will then exist, and generous individuals will be prevailed upon so to apply their redundant means. It is probable that it will also have a good effect on the literature as well as the tone of the profession. Crude works, hasty opinions, ill-digested theories, and imperfectly tested modes of practice, will more rarely issue from the press, as they will fail to produce that lasting good impression, which will then become more the object of active and intelligent surgeons.

"A change of measures in any society usually inflicts injury on some; and a change in the mode of election of the council will not be without its consequences to those who, like myself, look forward, in the present course of events, to become members of that body. The council itself will not suffer by the change, and the members generally will benefit by it. The expectants, under the present system of election, will be the only sufferers."

NORTH OF ENGLAND MEDICAL ASSOCIATION.

This is another of those Associations, now happily numerous in the land,* forced into existence by the profound conviction, almost universal among medical men, of the necessity of an important change in the general economy and government of our profession. We heartily congratulate our brethren on the accession of so numerous and influential a body to the great cause of medical reform, which, we doubt not, will, ere long, have many other similar aids to boast of. The North of England Medical Association was established at Newcastle on the 14th of November, at a meeting convened by a hundred of the most eminent medical men of the northern counties, and presided over by Dr. Headlam, of Newcastle The following resolutions show distinctly the nature and objects of the society:

"1. That, in the opinion of this meeting, the medical profession throughout Great Britain and Ireland has long been in a most unsatisfactory and anomalous condition; and that, owing to the defective nature of existing laws and institutions, consequences have ensued which imperatively demand the intervention of the legislature, being alike injurious to the profession, and incompatible with the welfare of society.

"2. That, with a view to submit to the notice of parliament the sentiments entertained by members of the profession at large, on subjects relating to their interests, and for the purpose of more effectually impressing upon the legislature the urgent necessity of reform in the medical institutions of this country, it is especially desirable that there should be union amongst medical practitioners.

"3. That, in order to adopt such measures as may facilitate the attainment of an improved system of medical government, as well as for the advancement of medical and surgical knowledge, and the promotion of friendly intercourse between practitioners residing in the northern counties, an association be now formed, under the title of the 'North of England Medical Association.'

"4. That a prominent feature in this Association shall be an active and zealous co-operation with other societies of a similar character throughout the United Kingdom."

M. MAGENDIE'S EXPERIMENT ON ENDOSMOSIS OF THE EGG.

In the review of M. Magendie's Lectures on the Physical Phenomena of Life, in our last Number (p. 318), we referred to an experiment made by that gentleman, as proving what he terms *imbibition by a double current.* This experiment, which appears to us to be novel and ingenious, consisted in placing an egg, with part of its shell removed, in alcohol, so that there should be only the layer of membrane lining the egg-shell between the albumen within and the alcohol without. Supposing at the time, that M. Magendie had used due caution in the performance of this experiment, we gave the result as he states he found it; namely, that while, by *endosmosis*, the alcohol passed through the membrane and coagulated the albumen—by *exosmosis* the albumen itself transuded through the membrane, and mixed with the alcohol in the vessel, rendering it turbid, and forming albuminous flakes. We have lately repeated this experiment more than once; and here, as in other cases, we have not found the result to coincide with the statement of M. Magendie. Having performed it precisely in the manner mentioned by him, we found that while alcohol readily passed in, and coagulated the albumen, in a degree proportioned to the length of contact, not a particle of albumen passed out through the membrane. The alcohol was as clear, after the lapse of a fortnight, as when first placed in the glass. It was allowed to remain until it had sunk, partly by imbibition and partly by evaporation, below the level of the egg; but still no turbidness appeared, nor were any albuminous flakes visible in it. On breaking the egg, a large portion of

* Two have been for some time established in Scotland—one at Glasgow, and one at Dundee. They are both highly respectable.

albumen was found coagulated at the lower part, and firmly adherent to the membrane. The layer of coagulum was dense, and extremely tough.

The experiment was now modified, so as to render the effects visible to the eye. The albumen of an egg was placed in a glass tube, the mouth of which was firmly tied over with a layer of fine skin, so that no liquid could transude, except by imbibition. The tube was then placed with the mouth downwards, in alcohol. In the course of two days the albumen became coagulated within the tube, forming a thick investing layer over the inside of the fine skin, and the action then seemed to proceed but slowly. Still, even after a fortnight's immersion, when the coagulating action seemed to have ceased, the alcohol was perfectly clear and transparent,—not a particle of albumen had passed out. The inside of the skin, on examination, was found to be covered with a dense white layer of coagulated albumen, resembling that found in the egg in the previous experiment.

We can explain the difference in M. Magendie's results only in two ways: 1, We may suppose that he inadvertently punctured the membrane of the egg, in removing the shell; 2, the entrance of the alcohol into the egg forces out, through the small hole made at the opposite end, a quantity of albumen, which drains down, and mixes with the alcohol beneath: if, then, in the experiment, he did not rupture the membrane, we can only refer the difference of his results to this fact, which may have escaped his observation. We have thought it proper to expose this fallacy, since M. Magendie bases many of his speculations on the physical powers of life, on experiments like this, conducted without even ordinary care. It is certain, either that he can have performed this experiment only once; or if more than once, he must have perforated the membrane of the egg each time. How is such an experimentalist to be trusted?

MR. WHARTON JONES'S RESEARCHES IN EMBRYOLOGY.

We owe it to our readers, and especially to Mr. Wharton Jones, to correct an error in the first Article of our present Number, which was not discovered until too late to be altered in the proper place. It is at page 6, line 7 from the bottom; and we request the reader to insert in that line, "man," or "the human ovum," in place of "the rabbit." The paragraph was intended to present, in a compressed form, the successive steps of discovery by which it has been established, that the germ-vesicle prevails generally throughout the animal series as an element of the ovarian ovum. And Mr. Jones's discovery of that vesicle in the human ovum (presumed by him at the time to be, with his other observations of the same in the pig, rabbit, &c., the earliest evidence of its existence in the class mammalia,) is one of those important steps. With respect to the germ-spot, Mr. Jones, placing greater reliance on his own observations, made with the simple microscope, than on those of others where the compressor was used, adheres, as we understand, to his first description of it, and maintains it to be an elevation on the side of the vesicle. We beg to add, that, in Mr. Jones's valuable paper, the same resemblance between the mature ovarian ovum of a mammal and the immature ovarian ovum of a bird is insisted on, which we have presented at greater length from Valentin, at page 9. In his second paper, besides those just views, as we conceive them, respecting the origin of the chorion, which we have noticed in the body of our article, Mr. Jones also details his observations and inferences from them relative to the genesis of the blastoderma: a subject of which the interest is equalled by the delicacy and the difficulty of the research. This subject must soon engage the attention of other observers more directly than it has hitherto done, when these original observations will demand a special discussion. We allude to them now for the purpose of recording, that here is detailed the dissection of a human ovum, probably, as Dr. A. Thomson remarks, "the very earliest that has been accurately described." "The whole cavity of the chorion," says Mr. Jones, "was filled with a fine gelatinous cellular tissue, imbedded in which, towards one

extremity of the ovum, was a small round body. It was evidently the spherical blastoderma; on being taken out, and examined under the microscope, it presented the same friable globular structure found in the spherical blastoderma of the rabbit. . . . . There was no vitellary membrane to be seen"

REVACCINATIONS IN THE DANISH ARMY IN 1837-38.

Year.	No. Revaccinated.	Cicatrices of former Vaccination.		Result of Revaccination.		
		Distinct.	Not Distinct.	Genuine Pustules.	Not Genuine Pustules.	No Result.
1837	3741	...	...	1601	1015	1125
1838	3721	3317	404	1464	1000	1257

Of the number who in 1838 resisted revaccination, 129 had had the natural smallpox, and 111 had been twice revaccinated. In the year 1838, there were, in the whole army, twenty-four cases of smallpox, partly children. Of the individuals who have been successfully revaccinated, not one has been attacked with smallpox."—*Bibliothek for Læger*, 1839.

OBITUARY.

JAMES HAMILTON, M.D., F.R.S.E., PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF EDINBURGH.

Died at his house, in Edinburgh, on the 14th Nov., Dr. Hamilton, the distinguished and popular professor of midwifery, in the 72d year of his age. This melancholy event was the consequence of a febrile attack, induced by a violent degree of exertion and consequent fatigue in the course of his attendance on a case of laborious labour. At first, little danger was apprehended; and although, as time advanced, the symptoms of weakness became more alarming, yet so often did he rally, that, till within a day or two of his demise, strong hopes were entertained that his naturally good constitution would carry him through the attack. The event, however, was otherwise; and he terminated his industrious and useful life on the morning of the day above mentioned, having been confined to the house little more than three weeks.

Dr. Hamilton was born in Edinburgh, in April, 1767. His father, who was his predecessor in that chair which he so long and so ably filled, carefully directed and superintended his son's education, with a view to his succeeding him. His classical and literary studies he prosecuted with equal zeal and success, being remarkable even at that early period for the same quickness of perception and soundness of judgment which characterized him in after life.

In the pursuit of medical knowledge he followed the same course, and the result was the same success. For many years, from the age of eighteen, he rose at five, and till ten in the evening devoted to study all the time he could spare from practice and teaching. And even up to the hour when seized with his last illness, he was an early riser, scrupulous and punctual in the division of his time, and exact in all his engagements.

As a practitioner, few individuals have enjoyed a greater share of public con-

fidence and esteem. From his earliest years he made it a chief object to study the phenomena of disease—thus storing up a vast body of facts, from which he afterwards deduced the great leading principles which guided his practice. To a surprising readiness and tact in the discrimination of disease, he added equal skill in the application of the necessary remedies; at the same time securing the confidence of his patients by his kindly manner and soothing address. It was, however, in his own particular department, when at the bedside of the suffering female, that he shone most conspicuous,—indeed we may say unrivalled; whether we consider the patient perseverance and soothing kindness and sympathy which he displayed in cases of lingering or tedious labour, or the boldness and decision with which he acted in cases of difficulty and danger, when once he had determined on the necessity of interference. It was these qualifications, combined with an intimate knowledge of his profession, which raised him so high, as a safe, successful, and favorite practitioner.

Dr. Hamilton's accomplishments as a teacher were of an equally high order, and were called forth at a very early period of life. In 1788, when only 21, he commenced lecturing as assistant to his father; and in 1800, the whole charge was conferred upon himself; thus for upwards of half a century was he engaged in teaching midwifery and the diseases of women and children. To an intimate knowledge of his subject was added an animated, agreeable, and interesting mode of imparting that knowledge to others, qualities which rarely co-exist—at least in an equal degree—in the same individual, and which combined to render him perhaps the most attractive and successful professor of his day.

At an early period in his professional life he was led to adopt certain innovations upon the usual and established practice. These he had been in the habit of inculcating for many years in his class, but did not fully submit to the public till 1836, when he published his "Practical Observations"—a work embodying the result of an experience extending to upwards of fifty years, and comprehending a practice the most extensive, perhaps, that was ever enjoyed by any single practitioner in this department of medicine. Many of his precepts are now generally adopted by the profession, but the importance and value of others are not as yet admitted by our most distinguished practitioners and teachers. A second edition of this work has just issued from the press, and may be considered, under the melancholy circumstances in which it appears, as his parting legacy to the profession; its final revision and enlargement being the latest object which engaged his attention, previously to his fatal illness.

As to the private character of Dr. Hamilton, we have every reason to believe that the delineation of it given in the public prints of his native city is strictly correct. He is there described as a man of liberal and unostentatious charity to the poor and distressed; as one deeply interested in the success of every beneficent and useful institution; of high moral feeling; of warm attachment and steady friendship. The intense interest displayed by the public, in Edinburgh, during the progress of his illness, and the profound and universal sorrow expressed on the tidings of his death, significantly testify the estimation in which he was so deservedly held.

FRIEDRICH LUDWIG KREYSIG, M.D., FIRST PHYSICIAN TO THE KING OF SAXONY, &c. &c.

F. L. KREYSIG was born on the 7th July, 1770, at Eilenburg, where his father was a practising physician. In his twelfth year he was sent to the public school of Grimma, where he laid the foundation of his scholarship, and where he remained until 1785, when he went to the university of Leipsig, to study medicine. After four years' residence here, having obtained what we would term a travelling fellowship (Das Kregel-Sternbachsche Stipendium), he went to Pavia, in 1792, and had the benefit of the instruction of Peter Frank, Scarpa, Paletta, and Spallanzani. Returned to Leipsig, he took the degrees of Master of Philosophy, and Doctor of Medicine; and, for some time, exercised himself as a private teacher of medicine. In 1796 he was appointed to the chair of

Pathology and Surgery, as substitute of Dr. Leonhardi, which he retained until 1801, when he was received into the faculty of the university as Professor of Anatomy and Botany. Previously to this appointment, however, he had published his important work, in two volumes, 8vo, entitled "Neue Darstellung der Physiologischen und Pathologischen Grundlehren für angehende Aerzte und Praektiker." Leipsig, 1798-1800. Kreysig's reputation as a man of science and a skilful practitioner grew so rapidly, that in 1803, while still only in his thirty-third year, he was called to Dresden, as body-physician to the elector, afterwards king of Saxony. From this time, his fortunes were intimately united with those of his royal patron; and, during the eventful years from 1806 to 1815, these often carried him beyond the limits of his native country. In the king's journies to Poland, &c., Kreysig was his constant companion, and, on these occasions, his abilities procured him a degree of reputation throughout these northern regions, which never left him. The leisure he enjoyed in the years 1813-15, during the king's compulsory stay at Berlin and Friedrichsfelde, he devoted to the composition of his great work on Diseases of the Heart, which was published at Berlin, in four volumes, 8vo (1814-18), under the title "Die Krankheiten des Herzens systematisch bearbeitet und durch eigene Beobachtungen erläutert." On his return to Dresden, he published, in 1818-19, his "System der praktischen Heilkunde," in two parts; a work which was never completed. He also took upon himself the professorship of Pathology and Therapeutics, and the direction of the Medical Clinic in the Medico-Chirurgical Academy, which he himself had almost created; besides other high official duties relating to the medical institutions of the kingdom. From this period, his private practice became most extensive; his reputation not being confined to Dresden or Saxony, but extending throughout Europe. Patients, affected with chronic diseases, came to him from all countries, and his consultations, by letter, were probably unexampled in extent. And this reputation was most legitimately acquired, through the superior science and skill, and the most cordial attention of the physician. His success as a practitioner was worthy of his renown; and it is much to be regretted that we possess no permanent record of his views and practice at this period of his life, beyond what is contained in his work on mineral waters, first published in English in 1824, and in the following year at Leipsig, under the title "Ueber den Gebrauch der natürlichen und künstlichen Mineralwasser von Carlsbad, Ems, Marienbad, Eger, Pyrmont, und Spa."

In the year 1822, Kreysig suffered a severe illness, and the consequences of this, and the death of the king, in 1827, led him to resign some of his official appointments, and also to contract the sphere of his practice; and henceforward he gave himself more up to the common amusements of life. His occupations were, however, still of a scientific kind; and he carried to a considerable extent the indulgence of his taste for horticulture and exotic botany. He also never lost sight of the improvement of his two principal works, on Diseases of the Heart, and on Practical Medicine, both of which he was desirous of placing on a level with the actual state of science, and of enriching with the treasures of his own practical experience. These objects, however, were far from being attained; he being only able to leave ready for the press the first volume of the first of these two works. In the year 1838, Dr. K. paid a visit to England, in company with Dr. Chaufepié, of Hamburg, with whom he also visited Ireland, and greatly enjoyed the reception he met with from his brethren in this country. Probably this journey was too much for his strength, or the excitement too great for his nervous system. At any rate, he fell into bad health afterwards, and, on the 27th of the following May (1839), he was seized with erysipelas of the face and head, which terminated fatally, on the 6th of June. On the 7th of the same month, he was buried with great pomp, being followed to the grave by all the most distinguished persons, professional and others, in Dresden: and it may be truly said of him, as it has been said only of the best of men, that, as he lived beloved, he died lamented.

CARL JOSEPH BECK, M D., PROFESSOR OF SURGERY, &c., IN FREIBURG.

C. J. Beck was the son of a country practitioner (Physicus), and was born at Gengersberg in the Kinzigthal, on the Rhine, on the 27th June, 1794. His father died before his birth, and his mother in 1799 entered into a second marriage with M. Schwörer, of Freiburg, to which place she removed with her two children. Young Beck was sent to the chief school at Freiburg, where he showed from the first great promise of excellence; he attended the university in the same place from the year 1808; here he remained until 1812, when he went to Tübingen, and spent a year pursuing his medical studies under Autenrieth, Kielmayer, &c. Immediately afterwards, while still only in his 19th year, on the march of the allied armies towards France, he was appointed to do the duty of a regimental surgeon in the field-hospital established for the benefit of the troops of Baden then blockading Strassburg. Here he had the great advantage of the counsel and assistance of the staff-surgeon-general, Meyer, and greatly distinguished himself in his treatment both of the sick and wounded. In consequence of his meritorious exertions, and after the proper examinations, he was promoted to the rank of regimental surgeon, and in this capacity made the campaign of Alsace, in 1815. The two following years, with the sanction of the government, and still retaining his military appointments, Beck spent in what may be called professional travelling, in company with his friend, Professor Chelius: they visited among other places Vienna, Berlin, Göttingen, Würtzburg, and Paris.

On his return in 1818, he was appointed professor-extraordinary and assistant-surgeon in the school of Freiburg, and at the same time took charge of the operative and ophthalmological branch of the surgical clinic. In 1819, he was named ordinary professor, taking for his share operative surgery, chirurgical nosology, diseases of the eye and ear, bandaging, &c. He subsequently taught medical jurisprudence, and occasionally assisted in the other departments. In 1828, he was named Counseller (Hofrath); in the following year, Privy Counseller (Geheimer Hofrath); in 1834, Höchstihr Geheimer Hofrath (whatever dignity that may be); and finally, in 1837, he received the crowning honour of a knight's-cross of the order of the Lion. Previously to this, however, in 1835, Professor Beck had begun to lose his health, and although he struggled manfully against his disease (an affection of the heart), returning, after intervals, to his professional duties, he finally sunk under it, on the 15th of June, 1838, leaving behind him a wife and four children. Dr. Beck was distinguished both as a practical surgeon and scientific teacher. The following are his principal works:—

Ueber die angeborene Verwachsung der Finger. Freiburg, 1819.—Ueber die Vorzüge der Lappenbildung bei der Amputation in der Continuität der Gliedmassen. Freiburg, 1819. Handbuch der Augenheilkunde. 2 Auflage, 1824-32.—Sacra semisæcularia, etc. Drs. Prof. Menzinger: Insunt: im Animadversiones de capitis vulneribus practicæ, etc. Freiburg, 1833.—Die Krankheiten des Gehörorgans. Heidelbg. u. Leipzig, 1827.—Ueber den Kropf. Freiburg, 1833.—Abbildungen von Krankheitsformen aus dem Gebiete der Augenheilkunde. Heidelb. 1835.—Ueber die Anwendung der Ligatur, etc. ein Beitrag zur Therapie des traumatischen Blutungen. Leipzig, 1835.

JOHN RUTTER, M.D., OF LIVERPOOL.

(From Dr. Jeffrey's Address at the Meeting of the Provincial Medical Association.)

Since our last anniversary meeting we have to lament the death of our highly-respected and venerable senior brother in Liverpool, Dr. John Rutter. He was an able, scientific, and honorable practitioner amongst us for about half a century, and no less conspicuous for the urbanity of his manners than by his anxiety to promote every scientific object in the profession, or indirectly connected with it; nor has he left any one more competent to supply

his place, as our leader and guide, whether we look at his character as a consulting physician or as one always selected to preside over our medical meetings. He was by disparity in age removed far beyond all competition amongst us for the last ten years of his life; and previous to that period, his seniority was always modestly assumed and cheerfully conceded. Most of our public institutions bear testimony to his intellectual labours, amongst which may be mentioned his activity in conjunction with the late Mr. Richard Heber, Mr. Roscoe, Mr. Clarke, and others, (I believe the late Rev. Dr. Frodsham Hodgson, Principal of Brazenose College, Oxford, who was a native of Liverpool,) in the formation of the Athenæum Library, and his exertions in forming our first Botanic Garden, the catalogue of which he arranged and wrote. He was for thirteen years one of five physicians attached to the Dispensary, to which were then appended the sick wards or infirmary of the workhouse, the fever hospital, a pauper lunatic asylum, the gaol, blue-coat hospital, and other minor charities, now all under separate and distinct medical officers. The Liverpool Medical Library had only imperfectly existed for seventy years, and never, until his untiring efforts, did it meet with a suitable resting-place; it is now permanent within the walls of this the Medical Institution; to the arrangement, improvement, and stability of which establishment he devoted much of his time, and nobly contributed to the extent of one thousand pounds. He also has enriched the building by a bequest of minerals and books, valued at more than that amount, and he would, had he been spared, have done much more. Although neither his age nor his habits allowed him to attend and enjoy our anniversary meetings, he always took a very lively interest in the proceedings of the association; and I frequently conversed with him upon the subject of the honour to be conferred upon us by the present reunion, from which he anticipated much satisfaction; and expressed an anxious wish that the medical meetings should be held within this building, the erection of which must be ascribed to his patronage. Dr. Rutter was a native of this town: he was a faithful member of that intellectual body, the Society of Friends; and he died at the age of 76, in affluence, the just reward of a well-spent life. The numerous attendance of his medical friends at his funeral amply evinced their respect and esteem; and I rejoice in having such an occasion to record my individual and personal experience of his abilities and uniform benevolence.

DR. SWEATMAN.

At his house, in Berners-street, on the 18th September, Dr. Sweatman, Lecturer on Midwifery at the Middlesex Hospital, aged thirty-nine. Dr. Sweatman was deservedly respected for his abilities, and beloved for his moral worth.

SIR ANDREW HALLIDAY, M.D., F.R.S. E.

Since our last publication, the profession has to regret the loss of a very estimable member, in the person of Sir A. Halliday, deputy-inspector-general of army hospitals. He died at his seat near Dumfries. Sir A. Halliday was the author of several works well known to the profession, the most recent of which we reviewed in a recent number. We hope to be soon able to give a fuller notice of Sir A. Halliday's active and useful life.

JOHN M'DIVIT, M.D., OF CANTERBURY.

Died on the 14th instant, at Bishopsbourne, Kent, at an early age, Dr. M'Divit, one of the physicians of the Kent and Canterbury hospital. He was the author of several valuable communications in our journals, which gave promise of more important contributions to medical science, had his valuable life been spared.

BOOKS RECEIVED FOR REVIEW.

ENGLISH.

1. First Annual Report of the Registrar-General of Births, Deaths, and Marriages. Presented to both Houses of Parliament by command of her Majesty.—London, 1839. Folio, pp. 16. Abstracts and Appendix, pp. 77.

2. Observations on Malaria, with suggestions for ascertaining its nature. By Thomas Hopkins.—Manchester, 1839. 8vo, pp. 26.

3. Changes produced in the Nervous System by Civilization, &c. By R. Verity, M.D. Second Edition.—Paris, 1839. 8vo, pp. 143. 6s.

4. The Anatomist's Manual; or, a Treatise on the manner of preparing all the parts of Anatomy, followed by a complete description of these parts. By J. P. Maygrier, M.D.P. Translated from the latest French Edition.—London, 1839. 8vo, pp. 564.

5. Text-book of Anatomy for Junior Students. By Alex. Jardine Lizars, M.D., &c.—Edinb. (no date). 12mo, pp. 272. 6s.

6. Cases of Chronic Hydrocephalus, or Water in the Head; with observations, and a detail of a new and successful plan of Cure. By J. F. Barnard, M.R.C.S.—Bath, 1839. 8vo, pp. 30.

7. Clinical Remarks on some cases of Liver Abscess presenting externally. By J. G. Malcolmson, M.D. (from the Edinb. Journ. No. CXLI.)—Edin. 1839. 8vo, pp. 44.

8. Medical Portrait Gallery. Part XX. XXI. XXII. Containing William Harvey, M.D.; Marshall Hall, M.D.; William Cullen, M.D.; Sir James M'Grigor, Bart.; John Brown, M.D.; Richard Powell, M.D.

9. Practical Observations, showing that Mercury is the sole cause of what are termed secondary symptoms. By P. J. Murphy, M.D.—Lond. 1839. 8vo, pp. 106.

10. The Modern Treatment of Syphilitic Diseases, both primary and secondary; comprising an account of the new remedies, with numerous formulæ. By Langston Parker, Lecturer on Anatomy, &c., in the Birmingham School.—London, 1839.—Sm. 8vo, pp. 158. 5s.

11. Observations on Malaria, with suggestions for ascertaining its nature. By Thomas Hopkins.—Manchester, 1839. 8vo, pp. 26.

12. Insecta. By George Newport, Esq. (From the Cyclopædia of Anatomy and Physiology.)—London, 1839. 8vo, pp. 128.

13. Elements of Physiology. By J. Müller, M.D., &c. Translated by W. Baly, M.D. Second Edition. Part I.—London, 1839. 8vo, pp. 471. 10s.

14. Valedictory Address to the Students of Medicine of the University of New York. Delivered Feb. 28, 1839. By J. B. Beck, M.D.—New York, 1839. 8vo, pp. 24.

15. A Practical Treatise on the Diseases of the Eye. By William Mackenzie, M.D., Surgeon-Oculist to Her Majesty, &c.—London, 8vo, pp. 923. 25s.

16. A Treatise on the Medical Jurisprudence of Insanity. By I. Ray; with an Introductory Essay by Dr. Spillan, M.D.—London, 1839. 8vo, pp. 436.

17. Treatise on Obstetric Auscultation. By Dr. H. F. Naegele. Translated from the German, by Charles West, M.D.—London, 1839. 12mo, pp. 120.

18. Observations on Yaws, and its influence in originating Leprosy; also Observations on Acute Traumatic Tetanus and Tetanus Infantum. By James Maxwell, M.D.—Edin. 1839. 8vo, pp. 134.

19. Illustrations of Osteology. By T. S. G. Boisragon, M.D. Folio. Two Plates. 1839,

20. A Treatise on the Diseases of Infants. By C. M. Billard, M.D., with Notes by Dr. Ollivier, of Angers. Translated from the Third French Edition, with an Appendix. By James Stewart, M.D.—London, 1839. 8vo, pp. 620.

21. Elements of Natural Philosophy; being an experimental introduction to the study of the Physical Sciences. By Golding Bird, M.D., F.L.S., F.G.S., &c. &c.—London, 1839. 8vo, pp. 407.

22. Gatherings from Grave-Yards, particularly those of London; with a concise history of the modes of interment among different nations from the earliest periods, &c. &c. By G. A. Walker, Surgeon.—London, 1839. 8vo, pp. 258.

23. The Fifty-first Report of the Visiting Justices of the County Lunatic Asylum at Hanwell, with the Report of the Resident Physician, John Conolly, M.D.—London, 1839. 8vo, pp. 70.

24. The Eye: A Treatise on the art of preserving this organ in a healthy condition, and of improving the sight, &c. By J. C. A. Franz, Doctor of Medicine, Surgeon of the University of Leipzic, &c. —London, 1839. 8vo, pp. 296.

25. Medical Report of the House of Recovery and Fever Hospital, Cork Street, Dublin, for two years. from 1st January, 1837, to 31st December, 1838. By G. A. Kennedy, M.D., Physician to the House of Recovery, &c.—Dublin, 1839. 8vo, pp. 120.

26. Elements of Surgery. By Robert Liston, Surgeon to the North London Hospital, &c. Second Edition. Illustrated with Engravings.—London, 1840. 8vo, pp. 795.

27. The Retrospective Address delivered at the Seventh Anniversary Meeting of the Provincial Medical and Surgical Association, held at Liverpool, July 24th-5th, 1839. By J. A. Symonds, M.D., Senior Physician to the Bristol Hospital.—Worcester, 1839. 8vo, pp. 100.

28. An Introductory Lecture on the Anatomy, Physiology, and Diseases of the Eye, delivered at the Birmingham School of Medicine, October 4, 1839. By Richard Middlemore, Surgeon to the Eye Infirmary.—London, 1839. 8vo, pp.

29. Observations on Medical Education, with a view to legislative interference. By Richard Jones, M.R.C.S.—London, 1839. 8vo, pp. 52.

30. A Lecture introductory to the Course on the Principles and Practice of Medicine, delivered at the Aldersgate School, Oct. 1839. By Robert Willis, M.D., &c.—London, 1839. 8vo, pp. 21.

FOREIGN.

1. Beiträge zur operativen Orthopädie. oder Erfahrungen über die subcutane Durchschneidung verkürzen Muskeln und derer Sehnen. Von Dr. L. Stromeyer. Mit 8 lith. Tafeln.—Hannover, 1838. 8vo, pp. 154.

2. Beobachtungen auf dem Gebiete der Pathologie und Pathologischen Anatomie. Von Dr. J. F. H. Albers.—Bonn, 1838. 8vo, pp. 218.

3. Beobachtungen und Erfahrungen aus dem Gebiete der praktischen Arznei- und Wundarzneikunst. Von Dr. Löwenhardt.—Prenzlau, 1838. 8vo, pp. 425.

4. Memorie della Società Medico-chirurgica di Bologna. Vol. ii. Fasc. ii —Bologna, 1839. 8vo.

5. Das Blut in seiner heilthäthigen Beziehung zum Schmerz im Allgemeinen und zu den (wahren und unwahren) Neuralgien inbesondere. Zur vorläufigen Erörterung empfohlen von C. J. Heidler, M.D. &c.—Prag. 1839. 8vo, pp. 57.

6. Osservazioni sul sangue umano, e Considerazioni sui metodi di piu' conveniente investigazione intorno ai fenomeni dei corpi organici Lettera Di Maurizio Bufalini.—Venezia, 1838. 8vo, pp. 123.

7. Six Mois de Séjour en Angleterre pendant l'année 1836. Par Sirus Pirondi, M.D.—Marseille, 1839. 8vo, pp. 435.

8. Avvisi agli Stranieri che amano di viaggiare in Italia o dimorarvi per conservare o recuperare la salute. Del Professore Giacomo Barzellotti, dell' I. e R. Università di Pisa.—Firenze, 1838. 8vo, pp. 290.

9. Théorie de la Phlogose, de J. Rasori. Traduite de l'Italien, par Sirus Pirondi, M.D.—Paris, 1839. 2 Tom. 8vo, pp. 334-406.

10. De Strabismo Dissertatio, quam scripsit N. G. Melchior, M.D.—Havniæ, 1839. 8vo, pp. 75.

11. De Crimine Raptûs, dissertatio quam scripsit F. C. Bornemann, Juris Candidatus.—Havniæ, 1839. 8vo, pp. 136.

12. Conspectus Aegrotorum qui in 1837-8, in Nosocomio Kiliensi tractati sunt. Auctore O. H. With, M.D.—Kiliæ, 1839. 4to, pp. 38.

13. Commentationis de Arsenico Specimen. Ad summos in philosophiæ honores obtinendos, scripsit B. S. Lewy.—Berolini, 1839. 8vo, pp. 45.

14. Dissertatio de Radesyge, Lepra et Elephantiasi Septentrionali. Auctore J. J. Hialtalino.—Kiliæ, 1839. 8vo, pp. 36.

15. De Chemicis Calculorum Vesicariorium rationibus. Scripsit E. A. Scharling, Chemiæ Lector.—Havniæ, 1839. 4to, pp. 52. Cum Tab. vi.

16. Londres Ancien et Moderne, ou Recherches sur l'état physique et social de cette metropole. Par A. M. Bureaud-Rioffrey, M.D., &c.—Paris et Londres, 1839. 8vo, pp. 138

17. Histoire de la Lithotritie précedée de reflexions sur la dissolution des calculs urinaires. Par Leroy-D'Etiolles, M.D.—Paris, 1839, 8vo, pp. 168.

18. Dissertazioni Hahnemanniane con Annotazioni critiche.—Cremona, 1839. 8vo, pp. 458.

19. Tarantismo o Malattia prodotta dalle Tarantole velenose. Memoria di Achille Vergari, M.D.—Napoli, 1839. 8vo, pp. 61.

20. Etudes statistiques sur les Fractures et les Luxations. Par M. Malgaigne, Professeur de la Faculté, &c.—Paris, 1839. 8vo, pp. 31.

THE

BRITISH AND FOREIGN MEDICAL REVIEW,

FOR APRIL, 1840.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

Recherches Cliniques sur l'Auscultation des Organes Respiratoires, et sur la Première Période de la Phthisie Pulmonaire. Par Jules Fournet.—*Paris*, 1839. One Vol. In Two Parts. 8vo, pp. 1029.

Clinical Researches on Auscultation of the Respiratory Organs, and on the First Stage of Pulmonary Phthisis. By Jules Fournet.

Remarkable as the history of Laennec's immortal discovery is in many points of view, the length of time which elapsed between the first promulgation and establishment of any considerable improvement in his system, forms, perhaps, its most striking peculiarity. Such were the multiplicity, the variety, and the delicacy of the phenomena he described, that, unless in the case of persons singularly gifted by nature, the very task of familiarizing themselves with the physical characters and diagnostic bearing of these was sufficient to absorb, for a length of time, all the keenness of the senses, and exercise all the vigour of the intellect. It would doubtless, too, have seemed a sort of heresy, once the incredulity of ignorance and inaptitude had been overcome, to question the accuracy of what the great discoverer had pronounced to be true, to look beyond the secrets he had chosen to reveal. But the cause which most powerfully contributed to the stability of Laennec's doctrines was of a kind still more honorable to him; for it was none other than the exceeding accuracy of his particular observations: by this was ensured a degree of perfection in his general results, which has rarely attended the first labours of a philosopher in a new path of science. Frequently the power of execution seems, as it were, blunted by splendour of conception; the mind that conceives shrinks from the minute details of practical application. But it was far otherwise with Laennec: each petty detail was followed out with a precision and felicity of observation usually belonging only to the humbler order of minds well fitted to apply the discoveries of others, but themselves deficient in the genius to create.

In proportion, however, as men became familiarized with the details of the new science, and especially after the death of its founder, the

spell was dissolved; and the senses sharpened under his tutoring were directed to the detection of flaws in his system. That the result has been of a most valuable kind, that improvements have been made, that some crude observations and hasty generalizations of Laennec have been exposed, and new facts established, is matter of notoriety. The observers, both at home and abroad, to whom we are indebted for these changes, have been contented to follow in the track of Laennec; and without swerving from the method in which he observed the audible phenomena of respiration, to announce such scattered novelties as presented themselves in the course of clinical study. But the case is to a certain degree different with M. Fournet, to whose volume we now propose to draw the reader's attention. Had this observer not perceived the possibility, as he informs us, of pursuing the study of auscultation differently from his predecessors, and of examining its phenomena on new principles, he would not have conceived the hope of enriching the science with important novelties, nor would the present volume have seen the light. What these "principles" are we shall at once inform the reader. M. Fournet, then, has invariably distinguished, both in health and disease, the two murmurs, the inspiratory and the expiratory, accompanying each act of respiration; whereas Laennec habitually spoke of these murmurs as of a single sound. He has, also, minutely analyzed the properties of each of these sounds, and so ascertained the existence and diagnostic importance of several abnormal conditions, which had either escaped, or been imperfectly put to profit by others; and by methodically enquiring, in the instance of every thoracic affection, with which murmur the morbid phenomena of respiration coexist, he has succeeded in recognizing many new signs, and obtained a clearer insight than previous observers into the relations connecting a variety of others, their successive conversion into each other, their natural course, and its accidental interruptions. The general practical result of all this is, it is alleged, a new degree of certainty and precision in the diagnosis, and, consequently, in the prognosis and treatment of pulmonary complaints. But the particular result, that on which M. Fournet's energies have been chiefly expended, is the power of detecting tuberculous disease at an earlier period of its existence than had previously been deemed practicable.

In this statement we have pretty closely followed the author's own enunciation of his achievements. Though his estimate of these may not be distinguished for modesty, the reader will allow that, unless it be most powerfully exaggerated, there must here be materials worthy of the gravest examination—materials of a kind to silence the sophistry of those interested persons, in whose hands, either from prejudice, laziness, or incapability, the stethoscope is destined for ever to remain an *inutile lignum*.*

* Auscultation has many avowed adversaries to contend with; these it fears not. The encomiums of its putative friends are, in some cases at least, much more likely to shake its credit. The worst of objections of the class referred to, is that they prove, not the inefficacy of auscultation, but the incapacity of Dr. A. or Mr. B. There are persons who would not be conscious of any more disagreeable auditive impression from hearing two barrel organs play different airs together at the same door, than from hearing "A te O Cara," from the boards of Her Majesty's Theatre, with Rubini for the tenor.

M. Fournet's work is divided into two parts: the first devoted to auscultation generally; the second to the history of the earliest stage of phthisis, its diagnosis, etiology, curability, and treatment. We shall pursue this order in our examination of the volume.

The fundamental distinction between these researches and others on the same subject, consists in the accuracy with which the condition of the expiratory murmur is examined in its normal and perverted states; it is, therefore, not without interest in this place, to trace the progress of knowledge concerning this sound, as well as our author's connexion therewith. The reader need hardly be told, that Laennec in his prolegomena, described in set terms the existence of an inspiratory and expiratory murmur, as accompanying the exit and entrance of air from and into the lungs. Yet, by a strange oversight, the most serious of which he was ever guilty, he speaks, throughout his work, of the respiratory murmur as of a single sound. The authority of his name, as well as some circumstances connected with the phenomenon itself, contributed to keep observers in ignorance of the existence of an expiratory murmur; until the well-known discovery made by Dr. Jackson, in 1832, respecting its distinct occurrence opposite tuberculated lung, directed the attention of numbers to the subject. M. Louis, in particular, recognizing the importance of the new sign, invariably enquired into its condition in all cases of suspected tuberculization; and was gradually led, from extending his enquiries, to the conclusion, that expiration always produces an appreciable, but exceedingly slight murmur in the state of health. The latter fact was by some considered, though, as we shall see, erroneously, to invalidate the importance of Jackson's observation in respect of phthisis. Other French observers came to similar conclusions; and in this country, the existence of an expiratory murmur was formally recognized, and formed the theme of an *ex professo* paper by Dr. Cowan.* Except in the instance of phthisis, the sound does not appear to have been habitually examined in its diseased aberrations; but we may be permitted to remind our readers, that we nearly two years since dwelt on the important information to be obtained from their study in pulmonary emphysema.†

With the knowledge he possessed of these facts, as far as they refer to Parisian practice, it is not a little curious to find M. Fournet recording the "astonishment" he felt, when he commenced his enquiries in 1835, at discovering that two murmurs instead of one accompanied the act of respiration. On Monday, this gentlemen, as yet a student, hears M. Louis and others speak of "prolonged, hard, or bronchial" expiration

No doubt if these persons favoured the world with their lucubrations on music, they would set out, by the aphorism, that the distinction of harmony and discord is mere fudge. But whom would they persuade, unless individuals sharing in their unfortunate style of organization? Neither will they, who are incapable of appreciating the delicate physical signs of disease, persuade those gifted with acuter senses to sacrifice their superiority. It must not be imagined from this that we regard auscultation as only to be compassed by a select few; on the contrary, we believe that there is nothing wanting but the early and proper study of its phenomena, to enable every one, possessed of the average degree of perfectness of the auditory sense, to attain a knowledge of it sufficient for all ordinary practical purposes. Here, as in everything else, there will and must be degrees of excellence.

* Med. Gazette, vol. xviii., p. 332. † Br. and For. Med. Rev., Vol. VI., p. 37.

in the common course of things, and marvels not; on Tuesday, he assumes the character of an original observer, and lo! he is forthwith astounded at hearing two respiratory sounds, because *Laennec* constantly refers to but one. This weak attempt to arrogate to himself what his countrymen would call the *idée mère* of his researches, gives painful evidence of M. Fournet's vanity.

In a somewhat prolix introduction, we are made acquainted with the properties of the respiratory sounds, which it has been the business of this observer to investigate, together with the method followed and precautions used by him in the practice of auscultation. The points which he conceives should be examined in each sound are: 1, *its proper or distinctive character;* 2, *its hardness or softness;* 3, *its dryness or humidity;* 4, *its quality* (timbre*); 5, *its note or tone;* 6, *its intensity;* 7, *its duration;* 8, *its rhythm.* To follow M. Fournet in his remarks on each of these properties is unnecessary, as their nature and importance as signs will appear in our examination of the subsequent sections of the work; but we shall here briefly notice some experiments made with sponge, for the purpose of ascertaining the mode of production of the dry, humid, and bubbling characters.

If a perfectly dried sponge be alternately compressed, and allowed to dilate close to the ear, a sound, varying in delicacy with the fineness of that substance, and distinctly conveying an impression of dryness, is perceived. If the sponge be then wetted, at first slightly, and then to a gradually increasing degree, a sensation of humidity is produced: "the humid character is at first homogeneous, then divides into a number of unequal consecutive sounds; the latter separate from each other, become isolated, and finally produce the sensation of bubbles originating, being developed and bursting." The transition from the slightest to the greatest possible degree of humidity is marked by distinct phases in the audible phenomena produced. But the result may be varied by changing the liquid with which the sponge is imbibed; when water is the fluid employed, the sensation of humidity is simple, uncombined with a character of viscidity, and resembles the respiration of an œdematous lung. If water, slightly charged with blood or gummy matter, be employed, a sensation of viscousness, such as accompanies the sound audible in a lung affected with active sanguineous congestion, is perceived. If the quantity of such fluid be increased, the bubbling character is developed; but the complete evolution of the bubbles seems obstructed by the clamminess of the tissue of the sponge.—Again, if a very fine sponge, moderately

* Much difficulty has been experienced in rendering into English the French term *timbre;* Professor Wheatstone objects to the translation *quality,* as not sufficiently definite; but we confess we cannot see why this word may not be conventionally allowed to answer to *timbre,* although it may commonly bear a wider signification. We shall ourselves so employ it, convinced that much less inconvenience will arise from this course than from joining in the habit, so mischievously prevalent at the present day, of interlarding our language with foreign words and phrases. To explain, in a few words, the meaning of *timbre* or *quality,* let us suppose two different instruments—a violin and a flute for example—to sound the *same note* with *the same intensity,* and for *the same length of time;* there will still be a certain peculiarity in each of the notes produced, distinguishing it from the other—that peculiarity is its *timbre.* The physical cause of the special *timbre* of each musical instrument and human voice is still unknown; some attempts at an explanation may be consulted in Müller's Physiology.

soaked with pure water, be compressed at any particular point with the finger, and then allowed to expand, a crepitating sound follows, possessing the precise characters of those puffs of crepitant ronchus, coexisting more particularly with the inspiratory movement, in pneumonia. The perfection of the bubbling character, the duration of the sound, &c., are modified by the manner in which the finger is removed after effecting the pressure. If it be raised in a jerking manner, so as to obstruct the free ingress of air into the tisssue of the sponge, it appears to effect its entry by successive efforts, the bubbling character is incompletely developed, or a few scattered bubbles only are distinguished. Now here we have exactly the characters of the crepitant ronchus of pneumonia, when the entire mass of a lobe is, with the exception of a few vesicles, impermeable to the air; the practical inference from this is, according to M. Fournet, perfectly clear—abundant puffs of crepitant ronchus indicate a more favorable physiological condition of the lung, than a ronchus consisting of a few scattered and ill-formed bubbles. To us it appears that the experiment merely illustrates the practical fact ascertainable by clinical observation. When, of the kind now referred to, the crepitation commonly occurs only at every second or third inspiration, and at the close of the movement, a condition which is known to accompany the period of transition from the first to the second stage of the disease.

M. Fournet further avails himself of his experiments with sponge in illustrating the mode of production of the inspiratory and expiratory murmurs, and their relation to each other. The expansion after compression is accompanied, as we have seen, with a distinct sound; unless a coarse sponge be employed, no appreciable noise is, on the contrary, produced by its compression. The sound elicited in the former instance is analogous to the inspiratory murmur, because resulting from the rush of air into the material experimented on; its *duration* and *intensity* are in the direct ratio of the thickness of the part compressed, in other words, of the quantity of substance acted on. In the same way, according to M. Fournet, in the normal state of the lungs and of the moving powers of those organs—that is, the number of permeable vesicles, and the force of respiration continuing the same—the duration and intensity of the respiratory murmurs will be constant quantities, and may hence be expressed numerically; to this proposition we shall presently have occasion to return. But we may not any longer delay with these experimental enquiries, which are carried much further by the author, and may be varied by any one inclined for such investigation. They certainly illustrate usefully the phenomena of healthy and abnormal respiration; for that the application of these results to the pulmonary tissue is neither strained nor inadmissible seems proved by the fact, that experiments by M. Piorry, on pieces of lung imbibed with various fluids, gave rise to precisely similar sounds. There is excellent exercise for the young auscultator, whose clinical opportunities are limited, with a piece of sponge, a basin of water, and a little gum.

These preliminary notions conduct us to the body of the work, which is divided into eight chapters, embracing enquiries into the physiological sonorous phenomena of respiration; their morbid varieties; the course, relative importance, and combinations of the latter; their classification;

the laws regulating their coexistence with inspiration or expiration; the characters distinguishing both murmurs in the different diseases of the respiratory system; the primitive seat or place of origin of those sounds; and, finally, the mechanism of their production. To introduce the reader successively to the various details of this comprehensive plan is plainly incompatible with the objects of this Journal: in the selection of topics, we shall principally have in view novelty and practical utility of isolated facts; though the author would probably object to such a course, as his generalizations and co-ordinations of phenomena exhibit his intellect in the most favorable point of view.

Let us commence then with the normal respiratory sounds in the vesicular section of the apparatus. Nothing is more common than to hear the inspiratory murmur of individuals characterized as "pure and vesicular," when it agrees with the adult type of health; but this expression, if meant to denote the character of the sound, to convey the notion of a successive dilatation of separate vesicles, is seriously erroneous. M. Fournet is perfectly correct in affirming that, if we look for such characters in healthy respiration, we shall search in vain for sound lungs. Far from this, the vesicular character, thus understood, is an abnormal phenomenon; whilst the true healthy sound is a mellow, continuous, gradually developed, breezy murmur, unattended with a sensation either of dryness or humidity. The use of the term vesicular is only justifiable with reference to the presumed seat of the sound. If we add, with our author, that the healthy expiratory murmur possesses precisely the same acoustic constitution as the inspiratory—that it is as soft and breezy, as free from hardness and dryness, and differing therefrom solely in duration and intensity, we shall probably startle no few practised auscultators. At least the testimony of writers of repute is to a different effect. Thus, in the very useful lectures of Dr. Williams, we find it stated that "in expiration there are at most only two kinds of sound, *bronchial* and *tracheal;*" and on enquiring into the grounds of this affirmation, we learn that it rests, as was to be apprehended, not on direct observation, but on deduction from some ingenious though, we think, fanciful theories respecting the reciprocal influences of the air and pulmonary tissue. "In respiration, the motion begins with the lungs, and the air, passively yielding to it, there is not motion or resistance enough to produce sound, until, by the converging together of the small tubes, the impelled air is gathered into a current in the larger tubes, where, impinging against their sides with its now acquired velocity, it at length produces sound." (p. 33.) Hence, according to Dr. Williams, the expiratory murmur is necessarily, from its seat, bronchial in character. The appreciation of this point is ascribed to MM. Andral, Louis, and Cowan; now we can affirm that M. Louis neither professes nor ever did profess any such doctrine; and we are persuaded that Dr. Cowan, whose otherwise excellent paper well deserves perusal, has in some instances mistaken the bucco-pharyngeal for the parenchymatous expiratory movement. But the question is neither to be decided by authority, nor by an appeal to *à priori* notions on the physical play of an organ, of the intimate structure of which nothing is in reality known;—it is a question of pure observation. Let the observer place his ear to the chest of healthy subjects, think neither of physics, nor of chemistry, nor

of written dogmas, but apply his mind to perceive his own sensations, and he will invariably find that (unless when, as is not unfrequently the case, it is inappreciable by the senses) the expiratory murmur possesses all the softness, gentle breeziness and freedom from bronchial character, which belong to its predecessor in the rhythm of respiration. Above all, he will discover that the period elapsing between the termination of the inspiratory and the commencement of the expiratory sound is an almost indivisible moment; that, consequently, where the one ends the other begins—that if the former be seated in the vesicles, the latter originates there also.

But, as we have already said, these murmurs do differ in two essential points, intensity and duration; the inspiratory exceeds the expiratory in both. M. Fournet has ingeniously endeavoured to give a precise estimate of the degree of difference subsisting between them, by assigning a numerical value to each; and fixes on 10 : 2 as the ratio of their comparative intensity and duration in the healthy state.* Yet, while we applaud the attempt, we must not disguise from our readers the sources of fallacy which interfere with the general truth and applicability of this estimate. In the first place, it is a point of extremest delicacy to determine in any given case how many times longer the first is than the second sound; and skilful auscultators may very readily form different opinions on the point: observers are not agreed as to the proportional duration of the two sounds and period of repose of the heart, yet here the problem is far less delicate. Secondly, does the ratio remain unchanged under all physiological conditions of increase or decrease of the respiratory murmurs, such as those of anhelation, &c.? M. Fournet infers that it does; but whether he has actually observed the fact does not clearly appear: we are disposed, however, judging from a limited number of experiments, to accede to the justness of this proposition, that when the inspiratory sound augments in health, the expiratory undergoes a closely similar proportional increase. Thirdly, is this ratio constant in all healthy subjects, in both sexes and at all ages? Here is the point respecting which we have the strongest misgivings; we might go further, and affirm that it is not—at least as far as regards the murmurs appreciable by the ear. It is in truth utterly impossible, even by causing them to vary their mode of respiring in every possible way, to detect a shadow of expiratory sound in certain subjects; and though the intensity of inspiration is generally low in such individuals, it is clear that the ratio of something to nothing is not as 5 : 1. For the present, however, M. Fournet's estimate may be accepted as a fair approximation to the general truth. Dr. Phillipp writes that no regular proportion prevails, that the expiratory sound may be totally absent, very weak, approaching to, equal to, or exceeding the inspiratory in point of intensity.† This is a most exaggerated statement of the irregularity observable, and evidently proceeds from this writer's having confounded certain diseased states with the healthy condition. The absence of expiratory sound alone cannot lead

* As 5 : 1 would be a more natural way of notating this ratio, that in the text is preferred by the author, as allowing a greater range of figures to express the modifications of increase and decrease occurring in the state of disease.

† Die Lehre von der Erkentniss und Behandlung der Lungen- und Hertzkrankheiten, p. 47.—A useful manual for the author's countrymen.

to any diagnostic error, if the important position laid down by M. Fournet, that such simple absence never depends on an anatomical lesion of the respiratory apparatus, be accurately true; for, under these circumstances, it is a variation not only compatible with but peculiar to the physiological state. This observer, indeed, affirms, that the expiratory sound is never absent alone, unless when patients, imagining that some extraordinary effort is required on their parts to facilitate the examination of their chests, breathe in that convulsive and jerking manner, with which the clinical observer is familiar, and which obstructs materially the circulation of the air in the lungs. To the correctness of this statement we cannot, as may be inferred from the remarks we have just made, by any means accede.

Passing to the section on normal bronchial respiration, we learn that M. Fournet has vainly sought in some individuals for this character, even in the murmurs produced in the interscapular space corresponding to the roots of the large bronchi. A vesicular murmur masks the bronchial in these cases; and the peculiarity is presumed to be due to the investiture of the portion of the tubes in question by a tolerably thick stratum of pulmonary tissue. The author has at least, as he avers, occasionally met with this anatomical disposition, but does not inform us if he has done so in individuals in whom the unusual absence of bronchial sound had previously been detected. We very fully agree with him in considering the region referred to the only one in which bronchial respiration is normally heard; authors have, as we believe, rather speculated than recorded the results of observation in assigning a bronchial character to the murmur in the axillæ and opposite the spine of the scapulæ; and consequently we can by no means assent to the statement made by Dr. Williams, that natural bronchial respiration is heard " over the space of two or three inches on each side of the top of the sternum."

In describing the pharyngeal, buccal, and nasal murmurs, M. Fournet dwells on the error, occasionally committed, of mistaking those sounds for bronchial respiration produced in the region to which the ear or stethoscope is applied. Independently of the difference of *quality* and of the seat of these sounds, which is easily recognized by an attentive and practised ear, there are two modes of correcting the illusion noticed by our author: the doubtful sound, if developed in the pharynx or mouth, may be altered in character by causing the patient to change the form of the openings of those parts, and vary the degree of rapidity with which the air penetrates; and again, as in some instances the pharyngeal sounds are perceived not by the applied but by the free ear, closure of the meatus of the latter will correct the error, as true bronchial or vesicular respiration are never perceived by the distal ear.

The laryngeal inspiratory and expiratory murmurs are, according to this observer, equal in intensity and duration, and may be represented by the number 20. The tendency to equalization of the two sounds increases, the higher the section of the respiratory passages in which they are examined: an observation of very nearly the same kind was made four years since by Dr. Cowan.

The diagnostic value of certain modifications of the respiratory murmurs is found to depend in a great measure on the existence of such modified character in one lung only or in corresponding parts of both

organs: in other terms, certain states of respiration derive their sole, others their chief value from comparison, a fact judiciously inculcated by Dr. Stokes. Now, for the employment of this principle with precision, the respective conditions of both lungs in health require to be pre-ascertained; for a very trifling natural difference in the murmurs in each will obviously produce the same effect, as a certain amount of disease would on the supposition of their being naturally identical. Let us suppose, for example, that a practised auscultator detects a delicate shade of difference in the *intensity* of the *inspiratory* murmurs under the clavicles; he will have an important sign of commencing tuberculization on the stronger side, provided, in the healthy state, the intensity is equal on both. Auscultators have carefully examined, therefore, whether the murmurs are similar or dissimilar in the corresponding points of the two sides of the chest; but we regret that the delicacy of the question has already interfered, and is likely to continue to interfere very seriously with uniformity of opinion respecting it. Dr. Stokes dwells on the importance of his discovery, that "in many individuals there is a natural difference between the intensity of the murmur in either lung, and in such cases, with scarcely an exception, the murmur of the *left* is distinctly louder than that of the right lung." * M. Fournet has, on the contrary, satisfied himself that, in persons presenting all the characteristics of healthy lungs, the sounds of inspiration and expiration are precisely identical in all corresponding points: in the few individuals in whom he detected a slightly greater development of the *expiration* under the *right* than the left clavicle, there were some motives for a dubitative opinion respecting the state of the lungs. Not contented with this result of observation, the author indulges himself in detailing some anatomical comparisons between the two lungs, whence it would follow that their physical constitution does not furnish any efficient cause for a difference in their respiratory sounds. This method of investigating the question—a tame imitation of the old system of writers, who busied themselves with ascertaining not what *is*, but what *ought to be*—is obviously not entitled to a particle of confidence. But, admitting the principle of enquiry to be a correct one, the inference to which it leads can hardly be that announced by M. Fournet, for he reasons from an erroneously applied datum respecting the relative size of the two sides of the chest. He states that the transverse diameter of both sides† and the capacity of the summits are equal; but neither these transverse diameters nor the capacity of the summits can really have the influence bestowed on them by M. Fournet; the question of size, as far as it bears on the point at issue, centres in the *general* comparative capacity of the two sides. Now, as has been proved by the researches of M. Woillez, the normal capacity of the right side of the thorax exceeds that of the left by a mean quantity 1·4 centimetres.‡

According to our author, the normal resonance of the voice is somewhat more marked in front than behind, at the summit than at the

* On Diseases of the Chest, p. 394.

† We presume this means when measured internally on the dead subject, though this is not distinctly stated; if the assertion were applied to the external measurement, it would be at direct variance with the statement elsewhere made, that the right transverse diameter is greater than the left.

‡ Vide British and Foreign Medical Review, Vol. VII.

base of the chest, but equal in the corresponding points of both sides. This account in its last particular clashes with the experience of the most eminent men. Often have we heard M. Louis expatiate on the existence of slight natural bronchophony—not as a constant, but as a very frequent condition—opposite the origin of the right bronchus; and we have been too closely familiarized with the exquisite perfection of M. Louis's acoustic faculty to question his accuracy, more especially as his opinion harmonizes with the experience of Sir James Clark and Dr. Stokes. The former observer recognizes greater resonance under the right than the left clavicle as a condition of health; the latter incidentally mentions his belief, that a similar superiority prevails over the entire of the right side of the thorax. (Op. cit., p. 497.)

M. Fournet considers the natural resonance of the voice much more marked in persons with a grave than an acute voice. Dr. Williams maintains on the contrary that it is most distinct when the voice is high or treble. The French writer's opinion is contradictory of an admission elsewhere made, that bronchophony and pectoriloquy have little symptomatic value in children, precisely because the natural resonance is in them very generally so intense as to simulate those morbid conditions.

In his division of the *morbid* phenomena of respiration, our author, like some of his predecessors, distinguishes transformations of the normal murmurs from another order of sounds, which are wholly new formations. The diseased changes to which the natural murmurs are liable are all referred to the heads of *augmentation, diminution, cessation* and *perversion.* Those belonging to the last class form a sort of transition between the modification and the new production: thus the metallic tinkling of hydropneumothorax belongs to the latter class, though it also follows in the train of alterations of *quality* belonging to the former. The author's experiments on sponge mark the error of Laennec in supposing that the various ronchi are not modified conditions of the normal respiratory murmurs; they exemplify the gradual transition from the natural to the bubbling character of ronchi. M. Fournet displays, at the same time, his usual ignorance of the literature of this and indeed of his own country, in fancying himself the first to proclaim the falsity of Laennec's belief.

The normal intensity and duration of the inspiratory sound being represented by 10, the extreme degrees of increase and decrease mark 20 and 0; between the maximum point of elevation and that of total cessation, all intermediate grades are observed. A remarkable difference in the mode of production of increase and diminution is, according to M. Fournet, that the former change never springs directly from any physical alteration in the pulmonary structure, and is produced, not in diseased parts, but in circumjacent healthy tissue; in a word, it announces the general fact, that a part of the lung supplies, by increased action, the functional incapacity of another, and characterizes *supplementary* respiration. On the contrary, the diminution of the murmur is the direct effect of some physical obstruction to the entry of the air, and represents the intensity of that obstruction. The importance of this modification, which, in the great majority of cases, affects both the intensity and duration of the sound, is apparent from the fact, that there is scarcely an organic disease of the larynx, trachea, bronchi, pulmonary tissue, and

pleura, which, as well as certain spasmodic affections, is not productive of it to a greater or less amount.

In speaking of the total cessation of the respiratory murmurs, occurring in well-marked pneumothorax, in cases of abundant liquid effusion into the pleura, &c., M. Fournet states, that towards the end of the inspiratory movement a gentle sound may occasionally be perceived, which appears to result from the lateral pressure of the condensed pulmonary tissue, by the air which is prevented from entering the bronchial ramifications. He conceives that the phenomenon thus arising may be rendered tolerably intelligible by the term *sound of pulmonary compression;* but, we confess, it conveys to us no very distinct idea. Dr. Williams, who long since noticed the peculiarity in question, described it more happily, as an abrupt stoppage of inspiration, accompanied with a kind of *hic.*

In health, the inspiratory sound is uniform and continuous; this condition constitutes, according to M. Fournet, its normal *rhythm.* In cases of sharp pleurodynia, he states, this rhythm changes; the murmur becomes abrupt, jerking, and divides into several successive and unequal parts. In incipient pleurisy, in the dry stage, a similar state is however observed; so that this observation throws no new light on the diagnosis of these two complaints. During the alteration of inspiratory rhythm, the expiratory remains unchanged; a fact easily intelligible.

A few points in the remarks on alterations of *quality* of the inspiratory murmur deserve notice. These changes are arranged according to their rate of intensity, in the following ascending scale: 1, the clear character; 2, the ringing; 3, the blowing; 4, the bronchial; 5, the cavernous; 6, the amphoric; 7, metallic tinkling. M. Fournet takes much credit to himself for seizing the intimate relation of these successive gradations, instead of regarding them, with Laennec, as distinct and isolated phenomena:—with what real claims to originality he plumes himself on this specimen of his acuteness, we need not remind the reader. He generalizes the anatomical conditions giving rise to alterations of *quality*, referring them all to solidification of the pulmonary tissue, increased size of the cavities in which the air circulates, or both lesions combined. We are scarcely satisfied that M. Fournet has not been over minute in attempting to establish those intermediate degrees of change between the natural and bronchial *qualities;* and it is certain, from his own admission, that these tranformations cannot be detected in acute solidification, whatever may be the case when that process is chronic. In pneumonia and pleurisy, for example, the *clear quality* and the *ringing*, which is a slightly increased degree of the former, are avowedly not detected in the transition to bronchial respiration; M. Fournet attributes this to the sudden and rapid increase of density undergone by the pulmonary tissue,—with what justness, further experience must decide.

Change of *quality* invariably, according to our author, commences with the expiratory murmur, and, after a period of variable duration, becomes manifest in inspiration. In its disappearance, it follows the opposite order; expiration is the first affected and the last released. The morbid *quality* may pass to the fourth grade in expiration, without having exhibited itself, even in the lowest degree, in the inspiratory murmur. This is a generalization of the point established by Jackson,

in respect of the expiration in incipient tuberculous cases. Hence the coexistence of even the slightest degree of altered *quality* with the inspiratory sound, announces a more advanced state of disease than even the fourth degree of change in expiration: this and allied facts, should they be found of uniform occurrence, may prove of considerable service to the practitioner in ascertaining the active or quiescent state, locally considered, of tuberculous disease. In their slightest degrees, the morbid changes under consideration appear only at the close of the inspiratory and expiratory murmurs; as the modification rises in the scale of intensity, it affects an increasing share of each sound.

The expiratory murmur is subject to much greater increase in point of intensity and duration than the inspiratory: if we credit M. Fournet, the maximum increase in these respects may be represented by the number 20, that already employed to designate the corresponding condition of the inspiratory sound. Now, as in the normal state, the former and the latter murmurs are made respectively equal to 2 and 10, it follows that while inspiration is only capable of acquiring double its healthy duration, expiration may attain ten times the natural proportion. And again, as it is elsewhere stated, that while the expiration undergoes this enormous rise, the inspiratory sound may fall to 1, it follows that instead of the expiration being only one fifth as intense as the inspiration, it may be twenty times as intense as the latter; and hence that it may actually bear one hundred times a higher proportion to the inspiratory murmur than natural. We are almost persuaded there is exaggeration in this expiratory estimate; at least we have never, ourselves, observed a degree of prolongation in cases of vesicular emphysema (wherein the abnormal extension has to us appeared to reach its utmost limit) which could be rated at more than five or six times the natural amount.

Augmented expiration may either coexist with a proportional increase in the inspiratory murmur, or the healthy ratio of the two phenomena may be destroyed by an accompanying fall in the inspiration. The former condition occurs in puerile or supplementary respiration; the latter in the early stage of phthisis, and in emphysema: these are indeed the only affections in which the disproportion exists to a very large amount, and hence its special value in their diagnosis.

There are two circumstances likely to induce a false estimate of the degree of abnormal prolongation. With one of these sources of fallacy, the bucco-pharyngeal murmur, we have already made the reader acquainted, and stated M. Fournet's instructions for correcting the illusion. The other originates in the fact, that the inspiratory murmur sometimes diminishes in length, while the expiratory remains stationary: if the ear take cognizance simply of their *proportional* condition, the error of supposing the duration of the expiration increased, will be very readily committed; to correct it, the attention must be directed to the *absolute* length of each murmur. The former circumstance is by far the most practically important, and we have had sufficient personal acquaintance with its deceptive influence, to recommend the young auscultator to familiarize himself thoroughly with the characters of the buccal, nasal, and pharyngeal sounds, and the means recommended by M. Fournet for correcting the erroneous impressions to which they are prone to give rise.

Diminution of the expiratory sound is much more rarely noticed than

of the inspiratory; the latter often falls to 4 or 5, while the former retains its normal value, 2. But if the cause of the depression of the inspiratory murmur continue in action beyond this point—in cases of pleuritic effusion, for instance,—the expiratory sound gradually falls to 0. In respect of the relation subsisting between the two murmurs in the morbid state, our author asserts the accuracy of the following general propositions. All affections which produce a simultaneous and proportional decrease in the intensity and duration of inspiration, are attended with diminished expiration; for example, pneumonia, pleurisy, pulmonary œdema, and apoplexy. On the other hand, when the intensity and duration of inspiration undergo opposite modifications, the one of increase, the other of diminution, the expiratory murmur is augmented; of this fact, pulmonary emphysema and tuberculization supply illustrations.

A parallel instituted between the morbid variations of the two murmurs, discloses a curious opposition in the laws regulating them; a few of these may be stated, as indicating the general character of the whole. The intensity and duration of the expiratory sound almost always rise or fall coevally; these characters frequently undergo opposite changes in inspiration. Modifications of *quality* belong more especially to expiration; those affecting the properties of dryness and softness, to inspiration. Diminution, affecting both murmurs, appears always first, and advances to its extreme degree more rapidly in inspiration than in expiration; in the return to the healthy standard the expiratory, on the contrary, precedes the inspiratory sound. In the particular facts related regarding each murmur, we have furnished the reader with materials for extending the parallel.

The exaggerated importance attached by Laennec to the distinction of morbid vocal resonance, to pectoriloquy and ægophony, for instance, as signs of excavation in the substance of the lung, and of the presence of a thin stratum of liquid in the pleura, is matter of daily demonstration. M. Fournet, who with even greater boldness than his predecessors affirms that it is in some cases impossible to determine whether a given ringing of the voice be ægophony, bronchophony, or pectoriloquy, has judiciously arranged the arguments proving the comparative futility of their distinction. We recommend this chapter to that rather numerous class of practitioners, who somewhat perversely cling to the correctness of Laennec's doctrine,—to persons who fancy themselves justified in invariably distinguishing hepatization from effusion, by the absence or presence of an ægophonic twang in the vocal resonance, or predicate the existence or non-existence of caverns from that of pectoriloquy. But while we express ourselves thus strongly against the habit of trusting mainly to these signs, more especially to pectoriloquy, we by no means deny their occasional value, and dispute not their general importance as allied signs. The accurate information derived from certain forms of pectoriloquy, has been satisfactorily pointed out by Dr. Williams; and, as M. Fournet observes, the strange, mysterious, ventriloquous sound produced in caverns, when the laryngeal voice is reduced by the progress of the tuberculous disease to a mere whisper, can hardly be confounded with any other known phenomenon.

The augmentation of natural adult respiration, termed *puerile* by Laennec, because resembling that of children; *supplementary* by Andral,

from its making up for diminished energy in some other part of the lungs; undergoes, upon inadequate motives, a second change of title in M. Fournet's volume: the idea of the new epithet, *exaggerated*, is contained in Andral's term, which, besides, marks the most important signification, practically speaking, of such respiration. The chief character of this form of respiration is an increase of intensity and duration of both murmurs, proportionally greater in expiration, and accompanied with a trace of *clear* or *blowing quality*. The comparatively greater increase of the second murmur, and the *quality* of both sounds in supplementary respiration, make it difficult to distinguish this form from cases in which the expiratory murmur has singly undergone a *morbid* increase. This difficulty, as we shall hereafter show, arises in the first stage of phthisis. In emphysema it may also be encountered, and the distinction is hardly to be made without the aid of other signs, e. g. hardness and dryness, in the murmurs. In truth it seems highly probable that, as M. Fournet hints, supplementary respiration, and the state characterized by prolonged expiration, are one and the same, fundamentally, and acquire their distinctive peculiarities from the combination of other changes with the latter.

Supplementary respiration is sometimes of no mean importance as a diagnostic sign; of this fact, strikingly remarkable in certain cases of pneumonia, M. Fournet gives a useful illustration. In three individuals expectorating the rusty viscid sputa, and exhibiting the general symptoms of pneumonia, the ordinary physical signs could not be detected. The only unusual phenomenon was marked supplementary respiration on one side of the chest; in two of these cases the ordinary signs subsequently appeared in the situation where the augmented respiration had existed; in the third, the latter phenomenon disappeared with the rusty sputa and general symptoms. Here were indubitable examples of central inflammation; in the latter instance, persisting to the end in its original situation; in the two former, spreading to the periphery of the lung. In cases where the characteristic sputa are wanting, as well as the usual physical signs,—an exceedingly rare coincidence however,—M. Fournet's observation may, we doubt not, be found of utility. Allied with this subject is the important question of the earliest signs of pneumonic inflammation, upon which a few words here will not be misplaced. Dr. Stokes, as our readers are aware, teaches the existence of a distinct stage of pneumonia prior to that of engorgement, and is led to believe that "an intense puerility of respiration in the affected part" will be found to accompany the earliest seizure of the pulmonary tissue, and hence point out the precise spot in cases of spreading inflammation about to become the seat of the crepitant ronchus. Dr. Williams, in imitation of Dr. Stokes, is of opinion that "the natural respiratory murmur will be rendered rough, and perhaps sharper, before the crepitation begins." But on the other hand, M. Grisolle, an observer of respectable Parisian reputation, declares "he has been enabled to follow the gradual extension of the inflammation, by the *loss of force and purity* on the part of the respiratory murmur, in a circumscribed point adjoining the manifestly inflamed part."* Now the particulars above related, from M. Fournet's work, testify to the accuracy of Dr. Stokes's *obser-*

* Mémoire sur la Pneumonie, p. 11. Paris, 1836.

vation respecting the sign of imminent attack of a particular part by the inflammatory action: but there is this fundamental difference between the two observers, that the one considers the puerile character produced in a still healthy part, the other in tissue in an incipient state of inflammation; in the one case it announces the actual existence of adjacent engorgement, in the other it implies that engorgement is ready to occur. That Dr. Stokes's *rationale* is probably (judging from close analogy, as well as from the third case alluded to by M. Fournet,) erroneous may, we think, be assumed; and, as we shall presently see, M. Fournet's experience of the signs produced in actually congested tissue, tallies more closely with that of M. Grisolle than of our countryman.

Under the name of *pulmonary crumpling sound* (bruit de froissement pulmonaire), the author describes a phenomenon apparently occurring, as he alleges, in the substance of the lung, and differing from all auscultatory signs hitherto noticed. In its most intense form, this sound very closely resembles the *new-leather creak of pericarditis*, differing therefrom solely in its greater acuteness of *quality;* in a less marked degree it constitutes a sort of plaintive noise, varying in tone with the rapidity of respiration; and, in the weakest and most common form, bears a close similitude to the gentle, quick, dry sound, elicited by blowing on fine paper. This phenomenon is generally produced in a very confined space, ordinarily coexists with inspiration only, and has been noticed by M. Fournet in the first stage of phthisis, in one case of encephaloid cancer of the mediastinum, and in another of non-tuberculous cavity of the summit of the lung. Some phthisical subjects experience a feeling of discomfort in the situation where the sound is audible; but this is rare. The anatomical conditions under which it arises are stated to be, a mechanical obstacle to the expansion of the lung, lobular induration of the pulmonary texture, and alternate flapping backwards and forwards of a fibrous lamina forming the wall of a cavern. As a sign of intra-thoracic tumour it possesses no value; it is of greater utility in cases of phthisis, as it existed in one eighth of tuberculous subjects attentively examined.

We shall recur to the subject of this sound, when engaged with the early diagnosis of phthisis. Meanwhile we may venture to express our doubts, founded very much on his own description, of M. Fournet's correctness regarding the location, mode of production, and close relationship of these modifications of sound. We can discover no proof of their being connected in the manner alleged; there is no attempt even made to point out the stage of transition from one to another, and that a sound like the pericarditic leather creak should be produced by the mere compression of indurated pulmonary tissue by the inspiratory rush of air into the neighbouring vesicles is, primâ facie, most questionable. It is very probable that the most delicate form of *crumpling sound* may be thus developed; but the intense variety referred to appears to us an uncommon and peculiar species of friction sound, produced in pleural false membrane, or similar structure. In the case of cavern adverted to, such was confessedly its mode of production. We have before us notes of a case of chronic pleurisy in a tuberculous subject, observed in August, 1835, in which we find these words: "Under the left clavicle the inspiration is masked by a well-marked *friction* sound, which does not exist in expiration, though when the patient breathes expansively, there is a

well-marked expiratory murmur. In the supra-spinata fossa a sound resembling the creaking of new leather coexists with inspiration and expiration; opposite the spine of the scapula this changes into a slight crackling sound, still masking the respiratory murmurs; these are heard about and within the angle of the scapula, the inspiratory sound is feeble, unaccompanied with ronchus, the expiratory prolonged and blowing." Now let us contrast the characters assigned to the creaking and friction sounds:

Creaking variety of crumpling sound,	*Friction sound,*
Coexists with inspiration only.	Almost always coexists with both movements.
Is continuous, and unaccompanied with any sensation of displacement.	Is jerking, and attended with a sensation of elevation and depression.
Is most frequently heard at the summit of the lungs.	Is most commonly met with at the central part of the posterior surface.
Does not produce any sensation appreciable by the patient himself.	Is not unusually felt by the patients themselves.
Is accompanied with no thoracic fremitus.	Gives rise to distinct fremitus.

The reader will observe, that the phenomenon under the clavicle possessed the majority of the characters of the crumpling sound, while the sound in the back was that of friction. Now the similarity in their characters appears presumptive evidence of the similarity of the tissue in which they are produced, and all the differences noted between them may be accounted for on the supposition, that the one arises from the unfolding of indurated pleural tissue, while the other originates in the collision of granulations, or laminar false membrane. It must be remembered, too, that although the distinctive peculiarities of the friction and crumpling sounds generally exist as they are stated above, yet each character may be absent, or those represented as peculiar to the one coexist with the other. The subject requires further examination.

Under the title of *humid ronchus with continuous bubbles,* M. Fournet describes a morbid sound which, he states, existed in twenty-three subjects, the only ones carefully examined, affected with active sanguineous congestion of the lungs. This is a vesicular ronchus, composed of bubbles with a peculiar character of viscidity and humidity, and which, in the language of our author, instead of attaining a completely spherical shape before bursting, form a third or a half only of a sphere,—an imperfection of development very distinctly traceable to the viscous quality of the liquid forming them. They hold a mean rank, in point of size, between those of the crepitant ronchus of pneumonia and of mucous ronchus. They are few in number, three or four only occurring during each inspiration (with which movement they coexist exclusively), and are slowly formed. Each new bubble is produced before the full development and explosion of its predecessor; hence the sensation of continuousness with which this ronchus is accompanied.

We have closely reproduced the more striking characteristics of this ronchus, as detailed by M. Fournet; the minuteness of analysis gives it an apparently novel character, but we have no doubt it has been observed by many, and confounded with the subcrepitant ronchus of acute capillary bronchitis. The latter is however, according to the author, distinguishable from it by the larger size, greater number, more perfect

sphericity, and comparatively rapid production of its bubbles, from its existing over a wider space, and at the posterior bases of *both* lungs, while the ronchus of active sanguineous congestion is usually confined to one side.

In the normal state, the movements of expansion and retraction of the lungs do not give rise to any appreciable sound, having its seat in the pleuræ. Of this fact, ascertainable by auscultating healthy individuals, and demonstrated on horses by M. Andral, the author adduces new proof from the examination of the thoracic and pulmonary movements in *two* rabbits. These experiments would not have been worth notice, were they not cited as proving—for M. Fournet affirms that M. Piorry and himself *saw* this through the flayed walls of the animals' chests—that friction of the pleural surfaces against each other regularly occurs in the healthy state, during inspiration and expiration. The observation clashes with some physical principles, and the nice deductions of M. Woillez on the reciprocal influence of the thorax and its contents; but as we have not space to enter on a consideration of the subject, we must content ourselves with referring the reader to our review of that gentleman's recent work. Were M. Fournet's experiments more numerous, and of unimpeachable accuracy, no à priori consideration should interfere with the inference they furnish; but they are few in number, and (as all who have examined the curious question of the effects of penetrating wounds on the motions of the lungs will at once recognize on perusal of their description) performed by a person most imperfectly acquainted with the complex nature of the phenomena he was about to analyze.

The morbid phenomena originating in collision of the pleural surfaces are examined with characteristic minuteness. Three varieties of friction sound are here admitted: 1, *The grazing sound* (bruit de frôlement); 2, *Friction sound properly so called* (bruit de frottement); 3, *Grating sound* (bruit de râclement). They are always audible in inspiration, not appearing in expiration unless strongly marked; thus the *grazing* variety is not perceived in the latter movement, while the others manifest themselves in both; under all circumstances they appear first in, and disappear last from, inspiration. Their occurrence over an extensive surface is of favorable augury, as such state usually announces rapid and uniform absorption of pleuritic effusion.

M. Fournet's experience confirms the conclusion already arrived at by Andral, Louis, Dr. Stokes, and others, respecting the non-existence of friction signs in interlobular emphysema. Indeed, the error of Laennec, in assigning this causation to the sounds in question, is now generally recognized.

The rarity of friction signs, compared with the frequency of pleuritis, has not failed to strike our author. Upon the more familiar causes of this disparity it is needless to dwell; we may however notice that M. Fournet considers diminished facility of locomotion in the lung, either from general pleuritic adhesion or extensive engorgement of its tissue, as capable of preventing their development. That they do cease, under the circumstances, had already been noticed by Dr. Stokes, who tendered, as an hypothesis, the explanation now announced as matter of certainty. Indeed, the fact that the supervention of friction sounds, in some cases of pleuro-pneumonia is coeval with the period of resolution, not of the

pleuritic effusion, but of the coexisting pneumonia, seems otherwise inexplicable.

According to M. Fournet, some trace of respiratory murmur always coexists with friction signs, because the expansion of the lung, and the rubbing which follows, cannot be effected without the penetration of some air into the vesicles. We are not unwilling to admit the accuracy of the latter clause, but question the truth of the inference from it, and deny the propriety of the mode in which that inference is drawn. Dr. Stokes was, we believe, the first to point out the interesting fact that friction phenomena may coexist with "great and universal dulness," and consequently with extensive liquid effusion; now, though Dr. Stokes does not mention precisely the condition of the respiratory murmur under these circumstances, yet if the dulness were at once great and universal, that these were absent seems the only reasonable conclusion. Nevertheless, it may be urged in favour of M. Fournet's opinion that, as in a case of abundant effusion attended with friction sound, he traced their production to some membranous adhesions retaining one part of the lung in rather close connexion with the costal pleura, the respiratory murmurs must have been audible opposite those adhesions, and are likely to have been similarly perceptible in Dr. Stokes's cases.

We can hardly assent to the correctness of another of our author's "laws,"—that the duration and intensity of the respiratory murmur generally decrease in proportion as the friction sounds reach a higher type. The occasional coexistence of the most intense form of friction sound (*grating variety*) with minute granular elevations, either on the surface of a false membrane or of the pleura itself, a circumstance adverted to by himself, is clearly at variance with the supposition of proportional diminution of the respiratory murmurs.

The grazing variety differs, in some important particulars, from allied phenomena. Though remarkable for mobility, it occurs more frequently at the upper part of the pleura than elsewhere; the changeableness of its situation depends on its being produced by local and transitory attacks of pleurisy, themselves determined by eccentric tuberculization of the subjacent lung. It is ordinarily of short duration; a single day suffices for its production, development, and termination, and this series of phases may, as M. Fournet has observed, be accomplished several times successively, in the course of a few days. Should these statements be verified, they must give a new character of precision to the diagnosis of local pleurisy, and perhaps help to determine the still unsettled question of the cause of the wandering thoracic pains of phthisis.

On the causes of the variable duration of friction signs, nothing novel is related. M. Fournet however states, and the statement is to us new, that a blister, applied in the situation where the rubbing sound is audible, hastens its disappearance. We are unfortunately left in the dark as to the number of times the fact was observed; but, admitting the occurrence to be a common one, how is the effect produced? "In one case, two blisters were applied successively; on both occasions the friction sound disappeared during the period of suppuration, and reappeared when they were suffered to dry up." We confess we cannot comprehend these changes, unless by supposing that the blisters produced slight effusion in the subjacent portion of pleura, and so deprived the false membrane of

the property of dryness essential to the development of the sounds in question. Dr. Macartney was in the habit of remarking, in his Lectures, that the cutaneous irritation produced by *flogging*, often brings on pleurisy; now we should not wish to draw any decisive inference from the analogy here presented, even were the alleged fact clearly proved, but, coupled with the phenomenon detected by auscultation, it rather warrants doubts, which are justified upon other scores too, of the real *therapeutical* power of blisters *applied over the affected part*, in pleurisy. We are the more anxious to draw attention to this point, as young practitioners, deceived by the marked relief of pain afforded by the application of blisters, imagine that in prescribing these epispastics they are really curing the disease, and are hence not unlikely to neglect means of less contestable efficacy. By *therapeutic* power, we mean that of diminishing the mortality, or shortening the mean duration of a disease.

The sudden disappearance of friction signs, originally developed on the absorption of an effusion, announces the return of the latter, or as a corollary from the proposition already stated, the supervention of pneumonia; M. Fournet affirms that in one instance this change in the pleural signs first awakened his suspicions, confirmed by the event, of the latter disease having appeared. There are several other particulars in this section well worthy of notice; but we must allow the reader to discover them for himself.

In his examination of *vesicular* ronchi, M. Fournet confirms the general observation, particularly dwelt on by Dr. Stokes, respecting the occasional absence of crepitant ronchus, both previous to pneumonic solidification and after its resolution. While upon this point, we may take the opportunity of stating our belief that Dr. Stokes has rather undervalued this ronchus as a sign of pneumonia. The coarse auscultator, to whose ill-educated sense all crepitating murmurs appear *the* crepitant ronchus, will no doubt sin frequently in his judgment respecting the presence of pneumonia, when no such affection really exists, if he presume to found his diagnosis on his acoustic perceptions alone. But of the *pathognomonic* character of the crepitant ronchus, formed of minutely delicate, dry, uniform, closely-pressed, regularly-rounded bubbles, bursting as with a sudden puff under the ear, we, with M. Fournet, entertain not a shadow of doubt.

The distinction of the second and third stages of pneumonia has long, confessedly, been one of the most puzzling points of semeiology. M. Bouillaud has, it is true, lately written, apparently with the view of enhancing the excellence of his marvellous *saignées coup sur coup*, as if the distinction were to him matter of facility; but he has convinced none, even of his countrymen, of his prowess, except those whose natural vocation it is to bow the head when "an authority" opes his lips. Dr. Stokes, however, some time since, announced his persuasion that the supervention of the third stage (that of interstitial suppuration) might be detected, with tolerable certainty, by the coexistence of "a sharp and peculiar muco-crepitant ronchus with bronchial respiration;" and M. Fournet, believing himself, as usual, original, confirms the accuracy of this statement. He adds the important observation, that the ronchus coexisted with inspiration only: we call this important, because it shows that the phenomenon was not produced in the bronchi, and is therefore not the

same as that which Laennec, à priori, as it is suspected, assigned as characteristic of pulmonary suppuration.

In his fourth chapter the author conveys, in a tabular form, the substance of his remarks on the healthy and morbid characters of respiration, and classifies the various ronchi. The presumed seat of the phenomenon forms the main groundwork of his classification; the character of dryness or humidity gives rise to the formation of sub-classes. The idea of this arrangement is far from new, but is here carried out more completely than elsewhere. In respect of their seat, they are divided into six classes,—intra-vesicular, extra-vesicular, bronchial, tracheal, laryngeal, and bucco-pharyngeal. The intra-vesicular class contains five species,—the humid crepitant, with continuous bubbles, already described at length; the sub-crepitant of pulmonary œdema, the sub-crepitant of acute capillary bronchitis, the ronchus crepitans redux of pneumonia, and the primary crepitation of the same disease. Under the head extra-vesicular, are included those morbid sounds, presumed to originate in the actual texture or parenchyma of the organ. The term is evidently ill-judged, for it is plain that tracheal ronchi have quite as fair a claim to such title as those on which our author confers it. Be this as it may, the class contains two subclasses, founded on the fact of the organ having undergone excavation, or being free from solution of continuity. To the latter belong the pulmonary crumpling sound, and the dry crackling ronchus (of which we shall presently give a full description), produced, as M. Fournet imagines, by the crumpling of the pulmonary tissue against tuberculous deposits. With the former, rank the humid crackling ronchus, the clear mucous, or cavernulous of Hirtz, the dry cavernous, and the humid cavernous, or gurgling.

With the remaining classes we find no reason to detain the reader; and pass on to the chapter developing the laws of coexistence of the morbid phenomena of respiration with the inspiratory and expiratory sounds respectively. The separate appreciation of the two sounds, under all conditions of disease, has led M. Fournet to the conclusion, that such laws do prevail, and that intimate acquaintance with them frequently furnishes a valuable guide in practice. Of this we have, indeed, already given one rather striking illustration. We have shown too, that, according to our author, the progress of certain affections may be traced by the manifestation of a morbid *quality* in both sounds, which had previously accompanied one only. Of the constancy of these "laws," we are of course, for the present, obliged to accept M. Fournet's assurance. The greater number of morbid characters belong to inspiration; but those of highest diagnostic importance appear in expiration; none are peculiar to the latter murmur only. The laws of coexistence may be traced to a certain number of general principles, of which the following are the most striking and important. 1. The closer the proximity of the seat of any ronchus to the pulmonary vesicles, the more marked is the tendency to exclusive coexistence with inspiration. 2. On the contrary, the further the seat of any ronchus is from the vesicles, the more uniformly does its production coincide with both movements. As a corollary from these two propositions it follows, that the proneness to exclusive coexistence with inspiration increases in the inverse ratio of the caliber of the tubes in which the ronchus originates; and this holds good in respect of the

ronchi of cavities of new formation and of increasing size,—witness the successive series of the humid crackling, the cavernulous and cavernous ronchi. 3. The larger the bubbles of humid ronchi, the more frequently do these coexist with both movements, and vice versâ. 4. The greater the intensity of dry ronchi, the more commonly do they affect both murmurs; the weaker they are, the more exclusively do these appear in inspiration only: as an instance of this, dry bronchial ronchi may be compared with the crumpling and dry crackling sounds of phthisis. 5. Ronchi of grave tone coexist more especially with expiration; those of acute tone, with inspiration. 6. Ronchi which are exclusively inspiratory appertain, in general, to more serious diseases than those audible with both movements; the humid crackling and humid cavernous ronchi form the only exceptions to this rule.

In the following table of the author's we present our readers with the means of increasing the list of general principles commenced above, and of more accurately estimating the originality and character of M. Fournet's researches on this section of his subject, than we could even by the fullest commentary.

Table, *showing the mode of coexistence of the Morbid Phenomena of Respiration with Inspiration and Expiration.*

[The order in which the different phenomena are set down in each division, exhibits the degree to which they relatively acknowledge the law regulating them all.]

A. *Morbid Characters coexisting exclusively, or almost exclusively, with inspiration.*

1. Humid ronchus with continuous bubbles.
2. Primary crepitant ronchus of pneumonia.
3. Mucous ronchus of third stage of pneumonia.
4. Grazing pleuritic sound.
5. Pulmonary crumpling sound.
6. Dry crackling ronchus.
7. Subcrepitant ronchus of œdema.
8. Subcrepitant do. of capillary bronchitis.
9. Ronchus crepitans redux of pneumonia.

N.B. The first three sounds coexist exclusively with inspiration; the others sometimes occur in expiration also, but exceptionally only. The frequency of these exceptions increases from No. 4, downwards.

B. *Morbid Characters coexisting with both movements.*

1. Humid cavernous ronchus.
2. Dry cavernous ronchus.
3. Dry { Bronchial, Tracheal, &c. } ronchi.
4. Cavernulous ronchus.
5. Grating pleuritic sound.
6. Friction pleuritic sound.
7. Augmentations of intensity.
8. Diminutions of intensity.
9. Augmentations of duration.
10. Diminutions of duration.
11. Amphoric character.
12. { Cavernous character. Veiled puff. }
13. Bronchial character.
14. Metallic tinkling and echo.*
15. Blowing character.

* In some cases of hydropneumothorax with perforation, observes M. Fournet, "the *metallic tinkling* of Laennec is not to be discovered; but, instead, the amphoric character of the respiration seems to reverberate in a sort of vague diffused echo, which rings like the voice under an archway; this phenomenon may be called *résonnance métallique;* it often accompanies the voice and cough." The English reader will here recognize the precise description, almost the very words of Dr. Williams, in reference to the phenomenon of *tinkling echo.*

16. Ringing character.
17. Clear ditto.
18. Mucous ronchus.
19. Humid crackling ditto.
20. Dry, hard, rough, laborious character.
21. Humid character.

C. *Morbid Characters coexisting chiefly with Inspiration.*

1. Diminution of duration and intensity.
2. Complete cessation.
3. Humid character.
4. Dry character.
5. All varieties of friction sound.
6. Pulmonary crumpling sound.
7. Dry crackling ronchus.
8. Subcrepitant ronchus of œdema.
9. Subcrepitant ditto of capillary bronchitis.
10. Ronchus crepitans redux.
11. Humid crackling ronchus.
12. Buccopharyngeal ronchi.
13. Cavernulous ronchus.
14. Gurgling ditto.
15. Humid, bronchial, tracheal, laryngeal ronchi.
16. Dry acute-toned bronchial, cavernous, tracheal, laryngeal ronchi.

D. *Morbid Characters coexisting chiefly with Expiration.*

1. Augmentation of intensity and duration.
2. Metallic tinkling and echo.
3. Clear character.
4. Ringing ditto.
5. Blowing ditto.
6. Bronchial character.
7. Cavernous ditto.
8. Amphoric ditto.
9. Dry grave-toned bronchial, cavernous, tracheal, laryngeal ronchi.

E. *Morbid Characters coexisting first with Inspiration, and then extending to Expiration.*

1. Pleuritic friction sounds.
2. Dry and humid crackling ronchi.
3. Hard, rough, dry, laborious character.
4. Humid character.
5. Crepitant ronchi, primary and redux.

F. *Morbid Characters coexisting at first with Expiration, and then extending to Inspiration.*

1. Clear character.
2. Ringing ditto.
3. Blowing ditto.
4. Bronchial character.
5. Cavernous ditto.

In confirmation of the utility of the distinction of ronchi, &c., into inspiratory and expiratory, and those common to both movements, our author collects together some of the most striking examples of differential diagnosis obtainable thereby. We have already, however, touched on the majority of these, and shall only notice two more points. There is no small difficulty, we have said, in distinguishing supplementary respiration from respiration with the expiratory murmur morbidly prolonged; now, if change of *quality* of the bronchial type exist in expiration only, all hesitation is at an end—the case is certainly not one of supplementary respiration. But important as this distinction is, scientifically, in all cases, and practically in many, there are circumstances in which it may have little value in the latter point of view. Let us suppose a respiration of the doubtful character alluded to, audible under the clavicle: to whichever species it really belongs, it may announce the presence of tubercles; if it be supplementary, we conclude that the foreign bodies are rather deeply seated in the lung; if of the other species, that they are close to the periphery: the practical man will hardly thank us for the delicate distinction.

If M. Fournet be correct, the primary crepitant ronchus coexists with inspiration only; the ronchus redux with inspiration chiefly, but slightly with expiration: the inference is, that the period of the disease may thus be ascertained in a patient observed for the first time, and under unfavorable circumstances for learning the previous course of the complaint. But the observations of two distinguished home auscultators do not coincide with those of M. Fournet. Thus Dr. Williams, speaking of the primary ronchus, states that "it is first heard at the commencement of inspiration and the end of expiration; but it soon accompanies the whole respiratory act, and, in advanced degrees of the first stage, is heard only at the end of inspiration and the beginning of expiration."* And Dr. Stokes, describing the ronchus redux, affirms that "it is to be heard during the whole inspiration, and, in a diminished degree, during expiration;" and, so far agreeing with the French observer, adds, that "in other cases the first part of the inspiration is pure, and the râle only appears at the termination," and has once observed "râle first, and then pure vesicular murmur." The impetus which the promulgation of M. Fournet's doctrines will, we are convinced, give to the study of the two murmurs, must soon settle questions of this character; and it is on this account we abstain from noticing numerous points in the table which our own experience would lead us to regard as of doubtful accuracy. We would, however, distinctly state that in all probability the characters or phenomena placed lowest in each division very frequently infringe the law to which they are referred.

A brief examination of the physical signs of the different affections of the lungs follows. Here there is little original matter, except the careful statement of the respective conditions of the two murmurs; to these we have already, as far as is practicable, referred. The exposition of the signs of emphysema is rendered curious by the author's fancying himself the first to study the movements of the chest in that affection with precision.† He has, however, fairer claims to the character of an original observer, in respect of a disease termed active congestion of the lungs; and as both symptomatically and anatomically this affection probably possesses some importance, we shall condense the more essential particulars related about it. If, as M. Fournet conceives, it be really the preparatory stage of pneumonia, and by early recognition of it the development of the latter severe disease may occasionally at least be prevented, it needs no stronger claim to the notice of the English practitioner.

This affection, which is described from the observation of twenty-three cases, chiefly occurred in subjects between the ages of seventeen and twenty-five; was apparently little influenced by sex; most commonly attacked muscular subjects of sanguineous temperament; acknowledged in many instances the influence of hereditary predisposition; [how this was ascertained, we are at some loss to understand;] or of previous

* See, with reference to the last particular, M. Fournet's experiments with sponge alluded to at p. 303.

† It may not be useless to remark that this observer agrees with MM. Louis, Woillez, &c., in recognizing bulging of the intercostal spaces as a character of emphysematous heteromorphism. Dr. Stokes has the *élite* of observers against him on this point. Vide Br. and For. Med. Rev., Vol. VI., p. 33; and Vol. VII., p. 367.

congestive affections; appeared most frequently in the hottest months of the year, and was in four instances immediately caused by "carbonic acid asphyxia," prolonged muscular effort, or insolation. The local signs were humid viscous respiration (see experiments with sponge); *fall of the inspiratory murmur to* 4 *or* 5, *of the expiratory to* 1; the ronchus described as humid with continuous bubbles; slight dry cough; slight bronchophonic echo, and dulness on percussion, in extreme cases; sputa rare, white, aerated, slightly viscous; (18 times in 23 cases) all coexisting with a peculiar sensation of discomfort and oppression. Though most commonly developed at the central and posterior part of the lungs, it may occur in any situation, and is neither in its primitive seat nor mode of extension subservient to the laws of gravitation.

Several series of characters, putatively distinguishing this affection from the first stage of pneumonia, capillary bronchitis, passive œdema, and passive congestion, are very industriously laid down; but we cannot help suspecting that M. Fournet's imagination has not been passive in the construction of these well-ordered categories. They are, at all events, such as *à priori* reasoning would easily supply; and *man's* imaginings and *nature's* doings so rarely agree, that we cannot avoid regarding this nice harmony with singular mistrust.

The terminations of active congestion are in resolution, hemorrhage, or pneumonia. The second, it is said, may be either pulmonary or bronchial. The disease merged into pneumonia in eleven of the twenty-three cases, and it is presumable that pneumonic inflammation is always preceded by active congestion: this at least is M. Fournet's opinion; but even analogy does not support it. He conceives the proposition proved, however, by the following result: Having, while intrusted with the care of M. Andral's patients, received two subjects of similar age, constitution, and strength, and both presenting the signs of active pulmonary congestion, he bled one freely, and prescribed some simple ptisan for the other: on the morrow, the signs of congestion had almost completely disappeared in the former, and were converted into those of pneumonia in the latter. This proceeding has been since repeated in a similar manner with a like result. M. Fournet calls it an "innocent experiment;" but, unless this ingenious gentleman has the power of saying to a pneumonia, "Thus far shalt thou go, and no farther," the innocence of the pastime seems somewhat of an assumption.*

M. Fournet's observations on the anatomical characters of active congestion are founded on a single case; they are manifestly in great part those of engorgement, and are completely different from those attributed by Dr. Stokes to the preliminary changes in pneumonia. Dr. Stokes's description has not yet received the sanction of general experience; but that gentleman cannot at least be charged with the absurdity of describing as novel, appearances already familiarly known.

We cannot find room for an account of three chapters occupied with the relation of experiments and hypotheses respecting the connexion of

* M. Bouillaud some time since, as we took care to inform our readers at the period, made the important disclosure that Broussais was the "*Messiah of Medicine.*" Broussais has since departed this life, and, for aught we know, the mantle has fallen on M. Fournet; if so, we must confess it would be heresy to persist in our doubts of the safety of his therapeutical gambols.

the pulmonary murmurs and thoracic movements, the seat and mechanism of those murmurs, and the reason of the coexistence of morbid variations with inspiration or expiration. We confess we regret this but little, as, on the whole, these chapters possess an anticipatory character: they are busy in imagining causes for phenomena of which the very existence is still in many instances problematical.

We shall here take leave of our author's researches on auscultation generally, and introduce the reader to the second part of the work, containing his contributions to the diagnosis of the earliest stage of phthisis. And here, before we enter into a critical examination of these, let us award M. Fournet his due meed of praise for the apparent zeal with which he has entered on his task. If, as Mr. Farr's tables show, 19·6 per cent. of the specified mortality of England is from phthisis, neither the philanthropist nor the medical philosopher can devote their energies better than in searching, by well-ordered observation and close induction, for some method of arresting its ravages. He who shall one day cry "ευρηκα"—and is it Utopian to believe that day will come?—will appear in the records of fame not second even to Jenner.

The incurability of phthisis by art in the second and third periods of the disease is unfortunately a demonstrated fact; but, as M. Fournet enquires, where are the proofs, and who has supplied them, that in its earliest anatomical condition the seal of incurability is stamped upon it? The truth is, they cannot exist; for we have been to the present hour without the means of detecting the presence of tubercles with surety and precision, while yet in their actually incipient state; the opportune time for action slips through our fingers before we are aware of it, and we commence the attack when the enemy is assured of victory. The first point, then, in an enquiry into the possibility of curing tubercle must be an attempt to establish its early diagnosis; and with this view the author has investigated minutely 192 cases of phthisis in the different aspects which the progress of clinical observation and his own special acquaintance with auscultation permitted him to do.

"The existence of the cause is a reason to suspect that of the effect," says M. Fournet; and, building upon the accuracy of this postulate, he examines those agencies which are, for the most part conventionally, fixed on as causes of phthisis, in order to enrol them among the signs of its presence. The justness of this proceeding hinges altogether upon the inseparableness of cause and effect in the evolution of phthisical disease. Now, as with reference to the previous state of knowledge, the idea of such inseparableness is absurd; as the fact of hereditary transmission is almost the only point in its etiology thoroughly established from direct observation on the *human species*, it follows that, unless M. Fournet's enquiries have completely altered the character of that knowledge, his first step in its diagnosis is not remarkable for soundness. This author has 192 cases on which to build his doctrine—192 cases, when thousands have failed to elicit truth! But perchance the defective use of these thousands has been the cause of the failure, and that 192 facts accurately observed and well arranged may succeed where multitudes have only started doubts without settling conviction. Be it so; and let us see how this author has proceeded to avail himself of his

limited resources—wherein he exhibits unwonted correctness of plan and execution. He sets out with the principle—of which we grant the truth—that phthisis is either inherited or acquired; and where hereditary transmission cannot be made out, diligently seeks out some anti-hygiological condition to which the patient may have been previously exposed: if successful in his search, he straight concludes that what comes last is the consequence of what went before. Glorious system, this *post hoc propter hoc* mode of argumentation, which tells us, for instance, that night is the consequence of day: so long as we are wise enough to appreciate its excellence, we shall be in no want of etiological instruction. A consumptive patient consults us; we obtain an admission that he has been immoderately given to sexual indulgence, and can discover nothing else to our fancy to account for the disease, therefore that indulgence is its cause; and as it is here a *cause*, in the next individual we meet, whose appetites have similarly sinned, the frailty constitutes a *sign* of phthisis.

But this fundamental vice is not all we have to expose in this chapter; M. Fournet displays marked ignorance of the principles of statistical medicine. He informs us, for example, that a particular form of lymphatic temperament was the most common among his phthisical patients, and hence infers its efficacy in inducing the disease. But this statistician forgets that, in order to justify any such conclusion, lymphatic subjects should not only superabound absolutely, but proportionally to the quota of individuals belonging to each temperament, forming the floating population of the Parisian hospitals in which he observed. If (we of course put the case hypothetically, though the probabilities are in favour of its reality,) lymphatic individuals are more numerous in those hospitals than those of other temperaments, will not the necessary effect be an excess of such patients among the consumptive, unless that form of constitution be actually prophylactic against the disease?

M. Fournet teaches his countrymen that, according to Sir James Clark, the period of life during which the greatest number of phthisical patients are *observed*, is comprised between the twentieth and thirtieth years: now Sir James Clark has not made this assertion, for the table referred to in that pathologist's volume relates altogether to the period of *death*. Though a misquotation, this and numerous other passages show that M. Fournet has not only perused but studied the work in question; yet, with singular hardihood, he produces as original the important opinion that diseases of the digestive and secretory systems of parents are those most prone to influence the health of children. But he inferred the fact from his own researches—possibly he did; but who taught him to search for it? who proved the search would not be in vain? He has no more claim here to the character of a discoverer than the traveller who, provided with the chart of the first explorer of a previously unknown region, follows confortably in the track of his enterprising predecessor.

We have been induced to expose the imperfections of this part of M. Fournet's volume, not on account of any assumed importance of his etiological principles in the diagnosis of phthisis—for he admits generally that little is to be learned in this way, but in order to warn the reader, who in the interest of perusal might forget the fallacious manner in

which many of them were obtained, from being imposed upon by the air of scientific exactitude wherewith this author has managed to invest his notions on the causation of the disease. This is the more necessary, as M. Fournet has the *folle vanité* to propose making a book, based on his 192 cases, on the etiology and nature of phthisis. Besides, if, as we conscientiously believe, in minute observation and the numerical method lie the main hopes of improving medicine, it is our first duty to expose such application of numbers to pathology as must tend not only to perpetuate but to give authority to error.

In the author's article on the influence of diseases of the lungs and pleura on pulmonary tuberculization, we find an affirmation that he has several times found tuberculous miliary granulations in the substance of pleuritic false membrane, while the subjacent lung contained not a trace of such product, or simply presented a few granulations or slight tuberculous infiltration of its peripheric substance. He has also seen a large cavern in false membrane similarly situated, while there were only a few traces of commencing excavation in the pulmonary tissue; and known the tuberculous deposition to advance from the false membrane outwards and affect the subcostal cellular membrane only. Under these circumstances the opposite lung, uninvested by false membrane, had occasionally remained perfectly free from tubercle. From these alleged facts the conclusion is drawn, that tuberculization of pleuritic tissue may occur independently of preexisting pulmonary disease; may be primitive and induce consecutively a similar morbid process in the lung. The author's observations tend also, he assures us, to prove that a similar mode of transmission from the serous membranes of the subjacent structure occasionally prevails in the intestines and brain. As we are promised a distinct publication upon this point also, we for the present register it without comment; we shall, on the appearance of the new tome, have occasion to examine the proofs given of the tuberculous nature of the granulations referred to.

M. Fournet's observations lead to the result long since announced by M. Louis, that the existence of *double pleurisy with effusion* involves, in the immense majority of cases, that of tubercles in the lungs. His conclusion on the influence of thoracic inflammations generally on the production and evolution of tubercle is the same as that adopted by Louis, Andral, Dr. Forbes, Sir James Clark, and all those who have observed nature, unbiassed by the dogma of irritation: as to the diagnostic value of intercurrent thoracic inflammations, derived from their peculiar course and character, we perceive nothing novel.

Here is examined at much length the symptomatic bearing of hemoptysis; but the degree of confidence, which the enquiries of preceding observers have taught us to repose in that symptom, is neither increased nor diminished thereby. Its occurrence as the first indication of the disease and in a state of sound health is justly regarded by the author as a circumstance of exceeding rare occurrence. But the fact that in these cases "the aspect of the patient is generally by no means indicative of perfect health," no matter how earnest his own protestations of robustness may be, had already been pointed out to the profession by Sir James Clark.

M. Fournet is of opinion that "active congestion and hemoptysis

tend, under the influence of a special predisposition to tuberculous formation, to attract and to concentrate in the lungs the process of tuberculization; and when this is once established to perpetuate and accelerate its progress." These opinions are not new; but as one or two arguments of a novel kind are adduced in their support, we shall put the reader in possession of these. The period of life at which active hyperemia of the lungs most frequently occurs is comprised, as we have seen, between the seventeenth and twenty-fifth years: comparing this result with those obtained in different quarters respecting the age of maximum frequency of hemoptysis and pulmonary tubercle, M. Fournet observes that the period of greatest frequency of hyperemia comes first, then that of hemoptysis, and lastly of tubercle. The catenation in respect of cause and effect is immediately inferred. Now we do not object to the principle by which this conclusion is obtained, but we pray the reader to forget M. Fournet's general terms and attend to the state of the individual facts. He observed altogether twenty-three cases of hyperemia, and yet talks of his *statistical* inferences therefrom: in *two* subjects only did this hyperemia terminate in hemoptysis, in one of these there were already manifest phthisical symptoms, in the other the disease was suspected; and finally, M. Louis has shown that hemoptysis was most common from forty to sixty-five, and in males of nearly the same frequency at all ages. Besides, has it not been proved by M. Louis, that the existence of tubercle in any organ in subjects past the age of fifteen, involves almost with mathematical certainty their presence in the lungs; in other words, that those organs are essentially the natural primary seat of the morbid formation, and this without the influence of any previous hemoptysis to "attract the process of tuberculization to them?"* But while we deny that these clumsy attempts at numerical induction prove in the least the point on which they are intended to bear, we do not intend to question the probable secondary influence of congestive movement on tuberculization.

The possible production of heterologous substances generally in the interior of fibrinous coagula is a question of high import. M. Andral, on the evidence of a single case, originally maintained the possibility of the direct transformation of an apoplectic clot in the lung into tuberculous matter. Criticised by M. Meriadec Laennec, he acknowledged his inference had been premature; and has never since met with a similar appearance in the lungs, but still affirms that such is the apparent mode of origin of splenic tubercles. Our author considers M. Andral's concession uncalled for, because he has himself observed a fibrinous mass in the centre of a clot in the aorta, studded with and almost wholly formed of small whitish, nonvascularized granulations. It is not a little curious how important a part the fibrinous portion of clots has been made to play by different writers: M. Andral, finding what he took for pus thus situated, in an instant converted the containing coagulum into an absorbing and secreting agent, endowed it with life, and with this *fact* for his text,

* M. Fournet marvels elsewhere why, in a subject addicted to sexual debauchery, the lungs, which had not been locally excited, were the principal seat of the disease; the testes and penis no doubt *ought* to have been the chief sufferers. But he most philosophically masters the difficulty by hinting, that the acceleration of respiration accompanying the act of coition may have been the cause of the pulmonary deposition!

sermonized on the wonders of organization : the microscope has happily dispelled those crude imaginings by showing the presumed pus to be nothing more than some of the substance of the clot itself in a softened state. What ingenuity, again, has been wasted in explaining the origin of the central "pus" of the coagula of phlebitis. And now we are required to credit a doctrine of tuberculization founded, the probabilities are a thousand to one, on some similar illusion. We know that an anatomist of somewhat greater experience than our author, namely M. Cruveilhier, was deceived into the belief that he had produced tubercles by injecting mercury into the bronchi; he mistook pus for that morbid matter. When our advocate of fibrinous tuberculization provides himself with some half dozen cases in which the *microscopical* character of the alleged tubercles are accurately enumerated, we shall give him a patient hearing.

Before entering into M. Fournet's exposition of the physical signs of the first stage of phthisis—a subject on which he is entitled to a greater share of attention than on those just adverted to—it is natural to enquire how these were ascertained; as the affection does not usually cut off its victims at so early a period of its existence as to allow a comparison of the signs of that stage with its anatomical characters. First, this observer carefully noted the signs produced on the confines of manifestly diseased parts, and compared these signs with the incipient disorganization by which they had been induced. Secondly, he traced the gradual advancement of the disease to the rest of the lung in patients presenting the signs of the second stage in the upper portion. Thirdly, he followed the gradual changes undergone by the respiratory murmurs in patients admitted into the hospital for affections unconnected with the chest, but who therein became phthisical, and presented a successive series of signs and symptoms, from the most trifling and obscure to those of excavation. Such being the judicious mode (for the possible fallacies likely to spring from one method are avoided and rectified by the others) in which the signs are stated to have been ascertained, we proceed to lay an abridged account of these before our readers.

Respiration. The *inspiratory* murmur increases in intensity to 12, 15, or 18; though less uniformly, diminishes in duration to 8, 7, 6, 5, and becomes dry, hard, and rough. The latter character is invariable in its existence, until marked by alterations of *quality ;* the former two are not unfrequently absent. The *expiratory* sound undergoes an increase in intensity and duration by successive steps from 2 to 20; both characters in the majority of cases alter *pari passu*, but in rare instances the intensity remains unaltered, though the prolongation is marked: a sensation of dryness and hardness is at the same time conveyed to the ear. The extent of alteration of both murmurs furnishes a tolerably exact estimate of the degree of advancement of the first stage. These changes are accompanied with modifications of *quality*, which, exhibiting themselves first in the expiratory murmur, successively pass through the ascending series already described, the first four grades corresponding to the incipient stage of the disease. While the change of *quality* belongs to one or other of these four grades, it invariably presents itself in a more intense form in expiration than in inspiration; the cavernous and

amphoric qualities, on the contrary, affect both murmurs to nearly a similar amount. With respect to the order of development and respective importance of these changes, it may be stated that modifications of duration and intensity present themselves first, and are therefore the very earliest physical results of tuberculization. Modified duration and intensity of the expiratory murmur, and changes of *quality* in both, have respectively much greater diagnostic value than similar alterations of the inspiratory sound, or than a dry and hard character in either murmur. The morbid variations now passed in review generally coexist with each other; so true is this that were some of them detected without the others, their dependence on tubercles should be called in question. It is important to add that to the description now given M. Fournet has encountered but very rare exceptions.

Ronchi. The ronchi of the first stage of phthisis are stated to be *the pulmonary crumpling sound, and the dry and humid crackling.* The former we have already minutely described; we shall now similarly dispose of the latter. The two crackling ronchi are successive degrees of the same phenomenon, the first accompanied with a character of dryness, the latter of humidity. The dry species is composed of a series of two or three crackling sounds; the humid form is similarly constituted, but its component parts gradually acquire the bubbling character more and more distinctly, until they actually produce a bubbling ronchus. The drier it is, the more completely is it confined to the inspiratory movement, when humid it accompanies both murmurs. The seat of this ronchus is primitively the summit of the lung, but it always originates, wherever there is tubercle in the first stage. The dry form is generally heard over a less extensive surface than the humid. Once developed the crackling ronchus rarely disappears temporarily (it did so in 9 out of 55 cases), but is distinguished by a persistence, which is itself more marked the longer the ronchus has existed. While possessing the dry character it is peculiar to tuberculization, and can be confounded with no other sound even in respect of its audible properties: the humid form may be mistaken for mucous ronchus, if its acoustic characters only be taken into account, but the course and seat of the phenomenon will clear up any doubts as to its nature. The crackling ronchus does not appear until the respiratory murmurs have undergone some slight alteration in intensity and duration, and, while other changes of *quality*, &c., seize those murmurs, passes successively to the humid form, the bubbling, the cavernulous, and the cavernous or gurgling. The time occupied in this series of mutations varies: M. Fournet found, in the majority of cases of acute phthisis, that the passage from the dry to the humid form was effected in from eight to twenty days; in chronic tuberculization in from twenty days to three months. The dry form occurs when a good number of crude tubercles either isolated or collected in groups form the anatomical evidence of the disease; under peculiar circumstances, however, it would appear that a very few minute tubercles scattered through the pulmonary tissue are capable of giving rise to it.

The pulmonary crumpling sound is far from possessing the important diagnostic bearing of the ronchi we have been examining; and really sig-

nifies nothing, unless coexistent with modifications of the respiratory murmurs; but if thus detected, it augments the surety of the diagnosis precisely at the period, the first or second phases of the first stage, when the difficulty of detecting the disease by its sensible signs is greatest. The change to the humid character announces the establishment of softening, and the advancement of this process is measured by the degree of humidity, and rapid or slow assumption of the bubbling character. Sonorous and sibilant ronchi depending upon local bronchitis sometimes coexist in the same region with the phenomena just described.

The regularity of occurrence of the changes traced by M. Fournet is of course a most important element for consideration in their diagnostic utility. He appears to regard those appearing to affect the inspiratory murmur as inseparable from the presence of tubercles, and gives it as his belief that if the dry crackling ronchus be not always observed, this arises from an examination not having been instituted at a sufficiently early period. We have already mentioned his ideas regarding the immediate seat and mechanism of the crackling ronchi; by others the humid form, at least, has been supposed to originate in the minute bronchial tubes, and such seems from the following passage to be the decided opinion of Dr. Hughes: "There is not unfrequently added to the signs already mentioned, either a small thin mucous rattle, resulting from the admixture of air with the glairy fluid existing in the minute tubes, or a single clicking sound, arising apparently from portions of thick viscid mucus, presenting an obstacle to the passage of air through those of larger caliber."* We may here notice that Dr. Hughes speaks of "the bronchial irritation preceding the formation of tubercles," as if such were an usual or even a constant phenomenon. He appears to be ignorant that M. Louis long since proved that the bronchi are as frequently healthy in phthisis, except when communicating with *softened* tubercle, as in individuals dying of all diseases indiscriminately; and that Major Tulloch has shown by the irrefragable testimony of figures, that while 13 per thousand of the whole military force are annually under treatment for consumption in *Jamaica*, and only 5 or 6 per thousand *at home*, catarrhal complaints only affect 65 per thousand of the force in the former place, while they are 122 per thousand in the latter. Pneumonia is still more rare in Jamaica.†—But to return to the signs of phthisis.

Voice. With the second phasis of the first period, in about one half the cases, a peculiar change occurs, according to M. Fournet, in the characters of the voice, rendering its tone grave, its quality less clear, its general character veiled, pectoral, and slightly hoarse: the whole constituting the incipient state of the well-known peculiar condition of the voice of subjects in an advanced stage of the disease. This change appeared to advance with a degree of rapidity proportionate to the frequency and duration of the colds under which the patient had suffered.

We observe nothing of the least novelty in this observer's analysis of the auscultatory phenomena. M. Fournet gives the recommendation to make the patient, instead of answering desultory questions, count a cer-

* Guy's Hospital Reports, No. ix.

† Vide British and Foreign Medical Review. Vol. VIII. p. 220.

tain number of figures, while the comparison is instituted between the resonance on both sides. In this way the changes of intonation produced by different words, and which probably affect the phenomena in question in a slight degree, are avoided. We have long been in the habit of employing this plan, (proposed, if not by Laennec, in his time,) but not, we confess, with M. Fournet's motive, which seems to us important.

On *cough* and *the transmission of the heart's sounds*, as indicative of tuberculization, there is nothing to detain us: the transmission of subclavian murmur by solidified lung, first noticed by Dr. Stokes, has escaped M. Fournet. There seems, however, to be some doubt as to the exact period of the disease at which this phenomenon becomes perceptible. By *percussion* no appreciable change of sonorousness is detected until the second phasis; on the application of this method of diagnosis, the volume before us falls short of the productions of Piorry, Dr. Stokes, and others.

Under the title *acouophonia* or *cophonia*, M. Donné has designated a mode of investigation in which the observer places his ear to the chest and analyzes the sounds produced by percussing different parts of that cavity. This plan would perhaps appear better fitted *à priori* than any other to furnish a correct notion of the different degrees of density of the contained organs. But as M. Fournet has found by experience that it is really deceptive, that the perceived sound bears no precise relation to the condensation or rarefaction of the subjacent parts, we shall not seek to rescue M. Donné's novelty from the oblivion into which it is fast subsiding.

The modifications of locomotive and vibratile movement of the chest ascertainable by *palpation* or manual examination are delicately analyzed by the author; though the result, as respects the former, is rather to be noted from its declared connexion with the first period of phthisis and diminished volume of the lung, than from its newness. "In cases of tuberculization inducing a notable increase of density in the summits of the lungs, the *partial motion* of the corresponding ribs undergoes a diminution proportional to the induration and decreased size of the organ. The *motion of totality* (of general ascent and descent of the thorax) is, if at all modified, rather augmented than lessened in extent. As this sign cannot, generally speaking, be detected, until the disease is sufficiently advanced to be readily discoverable by auscultation and percussion, it might be considered a diagnostic superfluity, did it not seem to possess more negative than positive value. As in cases of disseminated miliary tuberculization the costal movements are not sensibly affected, the absence of morbid change in those movements may become a positive sign of that particular anatomical form of phthisis, when the other physical signs and the symptoms announce an advanced period of the first stage.

To us it has always appeared exceedingly difficult to draw any satisfactory conclusion from what is termed the vibratile motion or fremitus of the thoracic parietes. We shall probably be told that our tactile perception is of imperfect quality; and we should submit to the aspersion without demur, did we not detect a very notable contradiction on the

subject in the statements of observers who, we presume, deem their fingers most happily moulded. If we believe Dr. Williams, liquid in the pleura will abolish the natural vocal fremitus completely, while in cases of hepatization, and under all circumstances of consolidation, the vibration is "*much stronger than on the healthy side.*" According to Dr. Stokes, the *absence* of vibration over the dull portion of surface is an exceedingly useful sign in the diagnosis of pleural effusion, *hepatization*, and enlarged liver, though traces of vibration may be detected in some cases of hepatization. But a very different version is that of M. Fournet. This observer affirms in the most satisfied tone that the maximum of vocal and tussive fremitus obtains in the normal state of the organ; that the phenomenon is always in the inverse ratio of the *density* or *rarefaction* of the pulmonary tissue and is totally abolished alike in hepatization and advanced emphysema. As these latest observations appear to have been made with patience, the following *excerpta* from the general conclusions respecting the healthy fremitus may possess interest for some of our readers. The vocal vibration is stronger in decumbiture than in the erect position; when the individual observed speaks in monosyllables than in polysyllables; in the front than in the back of the chest, on the right than on the left side anteriorly (noticed by Dr. Stokes also), most strongly developed in the lower part of the sternal region; extremely little marked in the clavicular region, hardly better in the sub-clavicular zones; and in many subjects altogether wanting. The difficulty of appreciating slight modifications in a sign thus characterized is sufficiently obvious; but M. Fournet's enthusiasm is nothing daunted, for he affirms that if the hand be laid over a mass of lung partly healthy, partly diseased, "the difference of vibration corresponding to the different parts of the hand may be *easily* distinguished." With such perfect specimens of tactile organization as this gentleman, it is of course vain to argue. It follows, however, from the general position above stated, that, as indeed is elsewhere distinctly affirmed, the vibration is diminished opposite tuberculated lung in the exact ratio of the extent of disease: an idea opposite as night to day to that promulgated by Dr. Williams. We are further told that a very slight difference in density of the summits will induce a manifest difference of vibration; yet this situation is pointed out as one distinguished by the very trifling degree of vibration producible in it when at the maximum, namely, in the normal state of the lungs. Why, if M. Fournet's statements be correct, the laws regulating the decrease of *tactile* should differ so widely from those of *acoustic* vibration we are unable to comprehend.

The author's enquiry into the signs of the first stage of phthisis, furnished by *inspection*, opens with an enumeration of some of the principal facts thus ascertainable in healthy subjects. The only point here stated bearing on this particular subject, which had not been elicited by the researches of M. Woillez,* seems to be that a piece of tape, stretched

* M. Fournet, with his usual happy ignorance of the proceedings of his contemporaries, knew nothing of these researches, of which an account was first published in May, 1835, until "he set about publishing his own work" in 1839! Truly the flood of light, which burst on M. Fournet from all quarters at that particular moment, must have been dazzling. Here, too, he manages to show us how easy it is to descry the

from the nipple to the most prominent part of the corresponding clavicle, will lie in immediate contact with the skin—a fact of which we shall presently learn the application.

The uniform transverse retraction of the upper portion of the chest, which induces the well-known cylindrical form of thorax, may, according to M. Fournet, be observed before or after the deposition of tubercle. This we admit; general experience acknowledges the fact, and M. Woillez has proved it. But we question if such preexisting conformation indicates, as the author affirms that it does, a predisposition to the growth of tubercle; the accurate observer just named has, it is true, shown that such malformation is much more frequent in phthisical subjects than in others, but where is the proof that in the particular individuals furnishing *this* result, the thoracic malformation preceded the tubercles in order of development? M. Woillez, we are aware, classes the defect of form in question with his physiological heteromorphisms; but as he appears to have been ignorant of the atrophy, occurring in tuberculous lungs, which distinctly leads to sinking in of the parietes, we cannot accept his decision in the matter. This species of contraction exists, according to our author, in one fourth, or at the utmost one third, of phthisical subjects; he estimates as follows the diagnostic bearing of different degrees of this deformity. If we discover such contraction in a subject presenting the other signs of the first phasis of the first stage, it must evidently belong to the preexisting species. In the second phasis, if the deformity be much developed, the inference will be the same; but in the third, we may fairly conclude that such abnormal appearance is an *effect* of tuberculization. The preexisting may, it is said, usually be distinguished with ease from the consecutive contraction, in the following way: the former species exists to the same amount on both sides of the chest, and in its entire height, though gradually diminishing from above downwards; the latter is particularly observed on the most diseased side, and at the summit. All this looks amazingly clear on paper, but we strongly doubt that these distinctions are founded on observation, or that the author has ever come to a conclusion in practice from their application. In justice we must add, that he does not regard this heteromorphism as possessing absolute diagnostic force, but merely as warranting suspicions, which it is the business of the other signs to confirm or invalidate: to maintain a different opinion would be absurd, for this precise conformation is observed in persons who descend to the grave without a tubercle in their frame.

M. Fournet again proclaims his literary deficiency, by affirming that

mote in our brother's eye, while we dream not of the beam that obstructs our own. He objects to M. Woillez's conclusion, that individuals following trades, which require particular exercise of the upper extremities, have on an average less development of thorax than others, because it is founded on too limited data (133 cases). Yet he himself preaches the law on every possible point of the subject of phthisis on the foundation of scarcely a greater number of observations, resolves with the most perfect composure problems which the wisest statisticians regard as insoluble in the existing state of national censuses, and deduces *statistical* inferences from *twenty-three facts.* But the true cause of his objection is, that the conclusion in question opposes some of his favorite theories.

depression of the sub-clavicular region has not been noticed in cases of phthisis, except as a consequence of pulmonary excavation, and in supposing himself original in the announcement that it may be induced by crude tuberculization. " Decrease of the antero-posterior diameter of the thorax, with flattening or slight hollowing under the clavicles," says Dr. Stokes, " may be appreciated at a much earlier period than has been supposed..........and is found a most important aid in diagnosis *in the earlier stages of phthisis*." But this modification has been more minutely studied by M. Fournet than his predecessor. In every subject examined after death in whom he had observed it, the summit of the lung was found to be invested by a fibro-cellular false membrane uniting it closely to the adjacent wall of the chest. The sign is not, as he believes, available until the close of the first period, but he has occasionally observed it at the outset of the disease, when preexisting pleurisy had led to the deposition of firmer and more resisting pseudo-membranous tissue at the summit of the lung than elsewhere. This sunken appearance may usually be detected by simple inspection, but we are recommended to measure its degrees, by stretching a piece of tape from the nipple to the clavicle : the distance intervening between it and the thoracic surface gives the measure of the deformity. Dr. Stokes uses a pair of callipers moving on a graduated arc, one branch of which is fixed on the scapula, the other below the clavicle; this method will also give the measurement of any posterior depression which may be present.

M. Fournet adds nothing to Sir James Clark's accurate description of the visible changes in the expansion of the ribs opposite tuberculous accumulations; he states, however, that he has not observed this species of change except when there was sub-clavicular depression, and that both these signs may be detected in about one fourth of phthisical subjects.

Having accomplished his task in respect of the physical signs, M. Fournet turns to the local symptoms of the first stage. Admitting the observation made by Kuhn, that the microscopical *tuberous tissue*, of which tubercle is, according to that writer, composed, may be detected in the sputa of the consumptive, he affirms that the fact cannot be available for the purposes of diagnosis, until the period of commencing softening; as before that change, the passage of tuberculous matter into the expectoration cannot be supposed to take place. But this is an assumption which, though probably well founded, requires the attestation of direct experience; it is irreconcileable with Dr. Carswell's opinions respecting the seat of tuberculous secretion.

In speaking of dyspnœa as a symptom of the incipient stage, M. Fournet avails himself of M. Louis's cases, as countenancing the opinion that that symptom, when originating in early life, proceeds from the fancied imperfection of pulmonary development already spoken of. But he omits to mention, that M. Louis himself has ascribed it to the coexistence of emphysema with the tubercular disease.* The description of this symptom generally adds nothing to the graphic sketch of Sir James

* British and Foreign Medical Review. Vol. VI. p. 38.

Clark; the notice of the dyspnœa of acute phthisis may, however, be usefully consulted.

Regarding the seat of tuberculous accumulations in the lungs, M. Fournet touches upon a point respecting which a curious difference obtains in the statements of observers—we mean the relative frequency with which the two lungs suffer and present the greater share of disorganization. As this is not a point of mere curiosity, but one which has close connexion with the application of the principle of comparison in diagnosis, it deserves investigation. The majority of writers assert that the left organ is most frequently and extensively implicated; Laennec seems to have stood almost alone in maintaining a contrary opinion. M. Louis, among others, has given the authority of his recognition to the former notion, and is cited to this effect by M. Fournet and others; but, curiously enough, the cases of that observer do not, on analysis, warrant his inference, for here is the result in figures of his fifty tuberculous subjects:

Cavities in both lungs in 33 cases	Most extensive in right lung in	13	cases
	left	12	—
	Doubtful in this respect	8	—
Cavity in left lung only		7	—
right		6	—
Tubercles (without excavation) predominating on the	right side	1	—
	left	1	—
	doubtful on which	2	—
		50	

Now both lungs appear to have suffered as closely as possible to the same amount. Of 170 of M. Fournet's patients, 109 presented the greatest share of disease on the right side, forty-six on the left; in fifteen cases both organs were equally affected. M. Hirtz also speaks, in his recent thesis, as if he had obtained a similar result. As a small minority, twenty-nine only, of our author's patients were examined after death (although the ratio was among these as 21 : 8 in favour of predominance of the right lung), the question must be allowed to stand in need of further examination. Whatever be the decision, we, however, see no objection to conceding that it is unusual to find the disease equally developed on both sides—indeed, the tabular view we have given of M. Louis's cases proves this. The author elicits no novelty in his considerations founded on this fact; Dr. Stokes appears to have exhausted the topic.

Before extracting some passages from the author's estimate of the value of the physical signs, we shall exhibit, tabularly, the physical signs of the three phases of the first stage; we shall thus, we conceive, facilitate the reader's comprehension of the subject generally. In drawing up this synopsis, we have noted each phenomenon with the characters and in the position most frequently assigned to it by M. Fournet.

First Stage.

First Phasis. A few small tubercles scattered through the lung.

- Inspiration, dry, rough.
  - intensity increases to 12.
  - duration falls to 9, 8.
  - quality, natural.
- Expiration, dry, rough.
  - intensity, duration, } rise gradually to 8.
  - quality, clear, ringing.
- Commencing bronchophony in rare cases.

Second Phasis. Infiltration of crude tubercles in groups.

- Pulmonary crumpling sound.
- Dry crackling ronchus.
- Sonorous, sibilant, ronchi (symptomatic of bronchitis).
- Inspiration—intensity = 12, 14.
  - duration = 9, 8.
  - quality, clear, ringing.
- Expiration, intensity, duration } = 6, 10.
  - quality, blowing, rarely bronchial.
- Dryness and roughness of respiratory murmurs are now masked by change of quality.
- Slight bronchophony, frequently.
- Slight obscurity of sound on percussion.
- Diminished vocal fremitus.
- Unnaturally distinct transmission of cardiac sounds.

Third Phasis (or of transition from first to second stage). Commencing softening.

- Humid crackling ronchus.
- Sonorous sibilant ronchi, as before.
- Pulmonary crumpling sound disappears.
- Inspiration—intensity = 15, 18.
  - duration = 7, 6, 5.
  - quality, blowing, or slightly bronchial.
- Expiration, intensity, duration } = 12, 15, 18, 20.
  - quality, bronchial.
- Strong bronchophony, or imperfect pectoriloquy.
- Sound more obscure, or even dull.
- Vocal and tussive fremitus much diminished.
- Diminution of partial movements of ribs corresponding to indurated mass.
- Transverse retraction of corresponding part of the chest.
- Subclavicular flattening.

But the facility with which these signs may be ascertained, the constancy of their presence, the regularity of their progress and catenation, form almost as important elements in judging of the confidence to which they are in practice entitled, as the actual reality of their existence. Upon some of these points, in so far as they are capable of being generalized on, M. Fournet speaks with just appreciation of the difficulties of clinical medicine.

"The observer must, while investigating the local signs, carefully guard against being influenced by any preconceived opinions, originating in the external appearance, or in the commemorative or general symptoms of his patient. If dominated by an opinion, in great measure already formed, the very best auscultator is not unlikely to become so careless and inattentive in his examination, that the impressions received by the senses are, without his being aware of it, converted into so many confirmations of his preformed judgment. This is the more likely to happen, because the phenomena appreciated by the senses and the mind are very numerous, and only distinguishable by very delicate differences from others of wholly distinct diagnostic force. When a first

examination has been made, under circumstances like these, we are sometimes astonished at the variation in the result of a second. . . .

"The physical signs are not, as might be believed, pathognomonic of phthisis; for such signs alone merit that title as belong exclusively to a certain anatomical condition. . . . Some observers consider a prolonged expiratory murmur peculiar to phthisis, and almost all that has been written on the sound of expiration bears the impress of this idea; the belief is a groundless one, for the expiratory murmur manifests itself in a multitude of different conditions. . . . But from mutual combination, from coexistence with some signs, and from their appearance, independently of others, the phenomena described may lead to a precise diagnosis of tuberculization; their primitive or absolute value goes no further than pointing out the presence of induration or foreign bodies in the pulmonary tissue. . . .

"At a very early age no inference could, I think, be drawn from the existence of the morbid characters of the respiratory murmur, which I have assigned to the first phasis,—in many instances even to those of the second." (p. 263, et seq.)

Fully as we assent to the justness of these remarks generally, we would, nevertheless, point out a slight contradiction between one of them and the author's previous assertion, that the dry crackling ronchus is peculiar to tuberculization,—if peculiar to, it must, when present, be pathognomonic of the disease.

Nothing can be clearer than that the likelihood of forming a correct diagnosis through the physical signs increases, the more advanced the phasis of the first stage during which the examination is made. We shall not be so traitorous to the principles we have elsewhere professed as to affirm that the diminution of confidence in them, during the first phasis, must often amount to negation, unless they receive strong confirmation from the general and commemorative symptoms; but we should ourselves be disinclined to trust them, unless so supported. We, in truth, in this opinion echo, as we shall presently see, M. Fournet himself. Who shall affirm his capability of surely estimating changes of intensity and duration, amounting only to one or two tenths of the normal sum—who, if persuaded that such delicate variations are estimable by his senses, can declare, that such departure from the normal mean may not be the effect of individual idiosyncracy? And again, is it easily credible that the complicated system of respiratory changes and ronchi, exhibited in the above column, works with the precision and regularity of a mechanical contrivance—that a morbid change once set in action never vacillates, but continues its onward course without interruption, till it reaches its maximum? Assuredly it is not; and however M. Fournet's general tone may be calculated to inspire conviction of the regularity of these changes, he is honest enough to confess, occasionally, that exceptions occur. In fact, the division into precise phases, and the ascending and descending notation of respiratory duration and intensity, are to be taken as approximations to the truth; unless this be admitted, numerous trifling contradictions, which escape the author in different parts of his volume, cannot be accounted for. But whatever be the irregularity to which it is probable the course and progress of these signs are liable, the reader must not forget that there scarcely exists a known combination of symptoms in any disease secure from similar unsteadiness.

Our author has, he alleges, observed, that the pulse of a great number of phthisical patients presents, even during the first stage, a peculiar

softness. To this observation he, however, attaches little importance; and we notice the point merely to introduce to the reader a curious disclosure, recently made by Dr. Guy, respecting the diagnostic information to be derived from the pulse at an early period of the disease.* We have space for the conclusions only, but the striking character of these will, we doubt not, attract our readers to the original paper.

"1. In cases of phthisis pulmonalis the frequency of the pulse varies within wide limits; the difference between the extremes amounting to 90 beats.

"2. In the same individual, the frequency of the pulse undergoes remarkable fluctuations, passing, in a few days, through a range of upwards of 60 beats.

"3. In five out of six cases, the frequency of the pulse in phthisis exceeds the highest frequency observed in health.

"4. The difference between standing and sitting in phthisis is nearly the same for all frequencies of the pulse.

"5. The maximum difference between standing and sitting in all cases of phthisis pulmonalis, falls short of the mean difference in health.

"6. From the average results of a considerable number of cases, it appears that the mean difference in health is six times as great as the mean, and three times as great as the maximum difference in phthisis.

"7. *On the supposition that the slight effect produced by change of posture is peculiar to phthisis pulmonalis, it forms one of the most constant and certain of its symptoms.*

"8. *On the supposition that the slight effect produced by change of posture* is common to more than one disease characterized by increased frequency of pulse, it will distinguish these diseases from others with which they may be confounded; and these diseases themselves are easily distinguished from phthisis pulmonalis, either by the peculiar character of the symptoms or by other physical signs."

We fear Dr. Guy forms too favorable an estimate of the diagnostic utility of the "slight effect produced on the pulse by change of posture in phthisis;" should experience prove us wrong, he will have fairly distanced M. Fournet, for, certainly, there are more persons capable of counting to 100 than of appreciating the delicate phenomena described by the latter observer. It is obvious that Dr. Guy is only entitled to affirm, from observations hitherto made, that the influence of posture is different in health and in phthisis; he cannot at present declare, that in many other diseases the pulse is not similarly affected, as in consumption. In his own practice he evidently sets at nought this difficulty; for he gives an instance of his diagnosticating phthisis on the evidence of this condition of pulse, and such appears, from the narration, to be not very uncommonly his habit. In the seventh and eighth propositions, a sort of provision is made for the objections of those unwilling to go the length of Dr. Guy, upon the imperfect data recorded.

But to return to M. Fournet. This observer next presents us with a close investigation of the course and progress of the disease. The question, whether the local or general symptoms appear first, is one of no mean importance; it is one, also, on which we are willing to hear M. Fournet, because he appears to have comprehended fully, and, as far as possible, eschewed the circumstances likely to involve erroneous inferences on the subject—we mean imperfect analysis of the local signs of the earliest phases of the disease, and careless interrogation of patients

* Observations on the Pulse in Phthisis Pulmonalis. By Dr. Guy, Guy's Hospital Reports, Oct. 1839.

regarding the symptoms they may have experienced at or previous to the supposed outset of the complaint. His conclusions are, that in essentially *chronic* phthisis, the sensible phenomena of tuberculization may be sometimes detected in individuals presenting, in a marked manner, the evidences of predisposition to phthisis, at a period when neither they themselves, nor the medical observer, can discover any general symptoms; that in other cases, the constitutional symptoms lead the van, at a period when, though demonstrative proof cannot be given that microscopical tuberculous matter does not inhabit the lungs, it is yet, if it really does exist, in too minute quantity to affect, in anywise, the respiratory murmurs; that, in a third series of subjects, the local and general manifestations appear to originate simultaneously. In *acute* phthisis the general phenomena appear to set in at least as early as the local changes. These general statements may be considered to leave the matter just as their author found it, and, for the reason we have already given, some confidence may be placed in them. In commenting on the second variety, M. Fournet observes, that the general phenomena cannot be considered a mere effect of the reaction of deposited tubercle on the system generally, but that, in a number of cases, "they appear to be the direct effect of the tuberculous cachexia." This is evidently appropriated, without acknowledgment, from Sir James Clark, and, at the same time, unskilfully borrowed, for under these circumstances they are not the effect, but the actual essence of the *cachexia* itself, as it is termed by our distinguished countryman.

We need not dwell upon the author's description of the course of the symptoms and signs; the order of development of the latter is distinctly shown in the table we have given of them; the account of the former is a periphrasis of Sir James Clark's chapter on the subject, and, indeed, corresponds therewith to a marvellous degree in some of its phraseology.

Two chapters and forty-nine pages are given by the author to the *circumstantial* and *differential* diagnosis of the affection. Under the former of these heads, he presents us with various combinations of local and general symptoms, which may occur in individual cases, and endeavours to estimate the probabilities for and against the existence of phthisis furnished by each. Here is exhibited much clinical sagacity, as, we think, the following specimens, selected chiefly from their reference to the value of the physical signs, will fully show. It is necessary to premise, that the accuracy of the diagnosis, under the circumstances supposed, has, in almost every instance, been ascertained, either by post-mortem examination, or by the gradual passage of the disease to its most advanced stages.

"CASE I. Being given an adult patient of strong constitution, sanguineous temperament, originally of robust health, of healthy parentage, never having been subject to colds or cough, nor having had hemoptysis, but who, *after exposure for a certain time to anti-hygienic influences*, perceived a change in his state of health, we may, with perfect confidence, diagnosticate phthisis, provided we can detect distinctly and persistently the sum of local and general signs attributed to the second and third phases of the first period, and if there exist no other morbid condition capable of accounting for these signs.

"CASE II. Under the same circumstances, the same diagnosis may be made,

but with less certainty of correctness, if the local signs be only those of the first phasis; provided the whole series of signs and symptoms acknowledge precisely, or nearly so, the laws I have traced as ordinarily regulating the development, course, and symptoms of accidental or acquired phthisis.

"Case iii. If, under the circumstances supposed, the general symptoms were wanting, the local signs of the first phasis would only warrant a simple *suspicion* of the existence of phthisis. Again, if the general symptoms were absent or of doubtful character, and the signs of the second phasis existed in a distinct and regular manner, the fact of tuberculization would become *probable*. Finally, if the local signs of the third phasis are observed in a marked degree, and although there be no distinct general signs actually present, but something suspicious in the character of those, either existing or not long past, we may be *pretty certain* of the presence of pulmonary tubercles. . . . .

"Case x. The existence of the general symptoms of hectic fever, which I have said belong to the first, second, or third phases of the first period, if unaccompanied with any of the local signs of that period, could not, even if there were hereditary predisposition to the disease, justify us in concluding, that phthisis was actually present; but would warrant the opinion, that tuberculization will sooner or later take place. If, in addition to labouring under hereditary predisposition, the patient have had hemoptysis, colds, and cough, with the course observed in phthisis, the absence of local signs should be set down as an exceptional occurrence, depending on a particular organic condition of the lung, and the case considered one of phthisis. . . . .

"Case xiv. If a subject born of apparently healthy parents, of only moderately strong constitution, and possessing delicate health, who has had frequent colds and chronic cough, and presents the morbid state of the voice elsewhere described, were attacked with pleurisy with effusion, which effusion becomes chronic, without any apparent cause for such termination, and entails deterioration of the general health, that subject should be considered affected with tubercles."

A similar position had already been established by M. Louis; but its vast practical importance justifies its repetition, whenever an opportunity occurs.

"Case xix. When the ordinary signs of the third phasis of the first period, ascertained by *auscultation and percussion*, exist only *on one side*, while natural respiration and sonorousness are detected on the other, the idea that the case is one of phthisis should be rejected, unless the signs furnished by *inspection* were exceedingly manifest on the diseased side. Experience shows in truth that, unless in the instance of a thick false membrane investing the summit of the lung, tuberculization, of the amount necessary to produce the physical signs supposed, *does not exist on one side only*." (p. 761, et seq.)

With the extensive chapter, next introduced, on the prophylaxis of the disease, we shall not delay; for, though its contents exhibit favorably the author's capabilities for the discussion of such questions, and are remarkable for their excellent arrangement and lucid combination, those acquainted with Sir James Clark's volume (from which, indeed, they seem largely borrowed,) can here discover no new principle started or developed. In the details, some novel notions occasionally break forth; but the youth and necessary inexperience of M. Fournet in the treatment of disease, deprives these of the sterling quality for which the English practitioner would value them.

Our best authorities, M. Andral, Sir James Clark, &c., hesitate in their judgment respecting the possibility of contagious transmission of

phthisis; the following account, bearing on this question, is sufficiently curious to merit extraction. Its accuracy rests on the authority of M. J. Guérin, the orthopædist.

"A female died in the third stage of phthisis, having shared her husband's bed till her last moments. The latter, who was originally of robust constitution, and sprung from a family, none of the members of which had ever been phthisical, married a second time; the subject of this union was of good constitution and healthy parentage. Eighteen months after marriage he died of confirmed phthisis; his wife, who cohabited with him to the last, remarried shortly after, and in two years died of consumption. Her second husband, of strong constitution, and belonging to a family in which phthisis had never been heard of, perished of the same affection soon after the death of his wife."

Upon this tale we shall only remark, *se non è vero, è ben trovato*, for the contagionists.

A disquisition on the curability of phthisis, during its third and first periods, presents some novel and speciously supported opinions. M. Fournet admits the possibility of a natural cure during the third stage, but endeavours to show that Laennec was in error, both in respect of the frequency of its occurrence and in ascribing the fact to the cicatrization of caverns. Curiously enough, this doctrine is put forward at the precise period that another Parisian observer, M. Rogée, publishes an elaborate and needlessly prolix paper, to prove that pathologists have no conception of the vast number of cases thus cured. The substance of M. Rogée's paper is elsewhere given; and we must here limit ourselves to an abstract of M. Fournet's arguments: to institute a critical comparison between the rival doctrines would occupy more space than we can devote to the subject. Our author commences with a verbatim exposition of Laennec's theory of cicatrization, from the works of that observer, and then describes the very different manner in which he views the same phenomena. He remarks, founding his statements upon the dissection of lungs in which no distinct trace of tuberculous disease was discoverable, that when pleurisy with effusion takes place, leading to compression of the lung and exudation of membranous matter, if the false membrane is thicker and firmer in some parts than others, the surface of the lung is always depressed and furrowed in the corresponding spots. The furrows thus produced are sometimes so much as an inch in depth, and always proportional in dimensions to the general or local decrease in size of the lung. The false membrane on the pulmonary surface sends prolongations of different extent into the interior of the sulci. At a late period, the superficial patches of pseudo-membrane, corresponding to the depressed portions of lung, are converted into fibrous or fibro-cartilaginous tissue; which is sometimes incrusted with calcareous matter, and undergoes constant decrease of size from interstitial contraction. At a still more advanced period, this fibrous disc is absorbed, and nothing remains but a few small fibrous bands, connecting together the well-known irregular mammillary prominences, which the affected parts of the lung present under these circumstances. Meanwhile, the prolongations placed in the sulci, and sunk, as it were, in the interior of the organ, similarly undergo the fibrous, fibro-cartilaginous, or even petrous transformations. Sometimes they retain the

character of white fibrinous lamellæ, traversing the adjoining indurated pulmonary tissue in various directions; and occasionally, *all trace of connexion with the external membrane is lost by the perfect closure and adhesion of the outer edges of the sulcus in which they are contained.* The adjoining portion of lung becomes indurated and thickly set with black colouring matter. Now these prolongations of pleural false membrane are, according to M. Fournet, what Laennec took for fibrous cicatrices of tuberculous cavities. Again, these fibrous prolongations sometimes become the seat of a secretion, an excavation forms in their interior, and puriform fluid appears therein; such is the anatomical state which Laennec has, in some instances, taken for the evidence of a cavity in *progress of cicatrization.* Without following M. Fournet seriatim through the very close and plausible train of reasoning, by which these positions are sought to be established, we have given a sufficiently complete outline of his mode of argument to show that his objections to Laennec's theory are not wholly visionary; the reader will, on perusal of the original, find stronger evidence of this fact. The critical scrutiny of the cases published by Laennec and Andral, as affording proofs of the fact of cicatrization, which is appended by our author to the account of his own observations, unquestionably proves that both these writers have ascribed to this process effects otherwise easily explicable, and predicated the tuberculous nature of cavities upon insufficient grounds. Whether M. Fournet's arguments are conclusive of the opinion he desires to substitute for those of his predecessors, further experience must decide. Meanwhile, his general conclusions may be abridged as follows: Pulmonary phthisis is, in extremely rare cases, susceptible of cure during the third stage, but has not been proved to depend upon complete cicatrization of cavities; nor is the mode, in which the cure is effected, yet understood, though it seems more likely to be by conversion of the caverns into fistulæ than by their closure. The tendency of caverns to contraction and obliteration, although a probable circumstance, has not yet received anatomical demonstration; and, finally, the only cases apparently capable of cure are those in which the tuberculous matter occupies a very limited extent of the lung, and gives rise to the formation of a single, or at most of two closely contiguous cavities.

The author shows, in a closing chapter, to what extent legitimate inference from anatomical phenomena supports the notion of the curability of the disease in its crude stage. He commences by examining the truth of Laennec's dogma, that the inevitable tendency of a tubercle is to increase in size and soften,—that the efforts of nature are always pointed in a direction opposed to its cure. This he proves, from Laennec's own lips, to be a perversion of the truth; that author himself actually describes tubercles, bearing all the appearances of having been partially absorbed and replaced by inorganic salts. But to dwell upon what is now a recognized fact—the absorption of tuberculous matter in the lungs —is unnecessary. There is this difference, however, in the opinions of M. Fournet and Dr. Carswell, who has most elaborately traced the anatomical evidences of curability, that the former regards the accidental fibrous tissue, formed occasionally in the substance of the lung, and investing particles or masses of cretaceous matter, as evidences, not of

the preexistence of cavities, but simply of that of crude tubercles transformed into that chalky substance. This is, it must be admitted, a very serious modification of previous doctrines, as it transfers the anatomical proofs of cure from the third to the first stage. Our space is exhausted, and we unfortunately cannot condense sufficiently, without weakening their force, the arguments adduced in support of this new view. In respect of the mode of formation of the accidental tissue round tuberculous matter, its successive transformations, its contractile power, and its occasional total disappearance, whereby the pulmonary tissue is left in what, to a superficial observer, appears a state of perfect health, this author follows exactly in the track of Dr. Carswell. Having thus, as he conceives, shown that all the anatomical probabilities are in favour of cure during the first stage, instead of, as has been believed, during the third, M. Fournet emphatically dwells on the encouragement given thereby to our therapeutic efforts; and, in imitation of Sir James Clark, enforces the necessity of general treatment, in the hope of securing such condition of the system at large, as may promote the absorption of the foreign bodies themselves, and obviate a renewed deposition of them.

Before we lay by our pen, we deem it advisable to take a rapid general survey of the work we have just analyzed, of its character, its pretensions, and its originality. If we ask, in the first place, to what extent the tone of the author, and the evidences he adduces in its favour testify to his accuracy of observation, without which quality, in scientific research, all others are as dross, the answer must be of a mixed kind. The cases scattered through and appended to the volume have been minutely observed, and ordinarily bear upon them the stamp of faithful narration; but, on the other hand, how shall we repose implicit trust in the statements of a person, whose passion for distinction is such as to lead him into the error, alike silly and despicable, of appropriating to himself, as an original discoverer, truths unfolded by the researches of others? Again, although he lauds the numerical method with emphasis, he is vastly chary of employing it; and going further even in dogmatism, than authors who use the vague terms "often," "seldom," "little," "much," &c., as expressive of the amount of their experience of particular facts, simply affirms, without hint of their comparative rarity or frequency, that such and such things *are*. When he does state his experience numerically, the smallness of the number of cases referred to is most striking, and justifies the belief, that when he has not dared to use it, their paucity must be singularly subversive of the importance he would fain have attached to his general announcements. The number of subjects examined after death is not even stated in a straightforward manner; and is, rather accidentally than otherwise, discovered to have been only twenty-nine.

The volume is spun out to a needless length by hyperdivision of topics, occasionally by useless wordiness and by frequent repetitions. The preface and some scattered sentences, in which the author's estimate of his own doings appears, prove him affected with the quintessence of the national failing—vanity. These singular outbreaks are, however, rather amusing than otherwise.

But, on the other hand, M. Fournet has given a new character of pre-

cision to many phenomena already imperfectly known, added some new ones, and taken a more complete and philosophical view of the system of respiratory signs than any previous author. He has pointed out the route, in his diligent investigation of the two murmurs, to, we doubt not, numerous important additions to semeiology, and taught us how much remains unexplored in this, as was hitherto supposed, almost exhausted field: multum adhuc restat operis, multumque restabit, neque ulli nato . . . . . . præcludetur occasio aliquid adjiciendi. The exact character of his improvements, in the early diagnosis of phthisis, requires explanation. He has assuredly himself learned the means of detecting the disease earlier than his predecessors; some cases of the affection diagnosticated in the very incipient stage (and correctly, as the event proved,) drew this admission from some of the members of the Academy of Medicine. But how has he done this, and to what extent has he taught others the secret? When we knew the contents of his volume only by the preface, we imagined by a number of new signs. So sanguine were we in this respect, that we fancied our future difficulty in the diagnosis of phthisis would be of a very novel kind, that the very multiplicity of decisive signs would almost generate confusion, and,

> "Like a rich armour worn in heat of day,
> That scalds with safety,"

betray us into one evil by the superabundance of protection against another. But we were quickly undeceived. His superior power arises not from the discovery of any new sign, but from his accurate appreciation of signs and symptoms already more or less familiarly known; his careful estimate of their mode of progress; his judicious combination of local signs, symptoms, and general phenomena; and the perfection to which he has brought his acoustic faculty by laborious training. And from this it follows, that they who turn to M. Fournet's volume, in expectation of finding therein a sign or two, in itself easily ascertained and decisive of the existence of the disease, of here discovering, without expenditure of time or exercise of thought, an easy road to a diagnostic millennium, will be sorely disappointed.* On the contrary, before his pages will serve them at the bedside, they must devote their energies to the attainment of a just notion of the value of each category of symptoms, of the progress of these, and tutor their ear into the nicest power of perception.

Finally, M. Fournet has the merit, and it is his highest perhaps, of attempting to turn the attention of his countrymen seriously to therapeutics. Should he succeed in diverting thereto, from its usual sole

* Some persons seem to fancy that the mere application of the stethoscope to the chest is quite enough for purposes of diagnosis, that this, by a sort of mesmeric power, discloses all that goes on beneath. And we must in candour admit that these individuals work wonders with the instrument. At least, we lately met a gentleman in consultation, who had hardly placed the stethoscope close to the *upper and anterior edge of the precordial region* when he detected *pleurisy*, though the patient, whose pleura was as sound as his own, really laboured under nothing more than *capillary bronchitis of the posterior bases* of both organs. Unfortunately for persons of this stamp, the stethoscope is not a thinking, speaking, diagnosing being, but simply

> "Ist wie das todte Sprachrohr, das den Schall
> Empfängt und wiedergiebt, und selbst nicht höret."

object, pure pathological anatomy, even a tithe of the energy the French enquirers are wont to exhibit, something may be hoped from his innovation. And while it will afford no small satisfaction to the profession in this country to find that the branch of medicine, to which they have always directed their chiefest attention, begins to assert its due rank among their continental brethren, the distinguished physician, from whose work on Phthisis M. Fournet's scheme of treatment is derived, must behold with honest pride the sterling improvement to which the dissemination of his labours shall eventually give rise.

Art. II.

First Annual Report of the Registrar-General of Births, Deaths, and Marriages in England.—London, 1839. 8vo, pp. 168.

Before presenting our readers with a view of the contents of this very important volume, we think it may be useful to offer—not an apology for statistics, for this science stands in need of no apology—but a vindication of it from certain imputations to which we have observed it subject. The first we shall notice is, that the study is a cold-hearted one—that it leads its votaries to regard their fellow-creatures as mere abstractions, or even as "ciphers," and not as human beings, with the thoughts, feelings, and aspirations of men within them. Now, we feel convinced that it would be impossible for any one to pursue statistics in such a spirit; if he saw the "ciphers" only, they would be speedily abandoned, as the most wearisome of objects. It is the suggested ideas—all which ideas refer to man in the concrete, to man as a being possessing happiness to be augmented and misery to be relieved—which invest this science with all the interest that appertains to it. The figures of a statistical table suggest various things to varying minds: the statesman sees in them one thing, and the physician another; and even in the two classes to which these belong, the ideas will vary according to the benevolence, talent, and previous information of him who contemplates the "ciphers;" but the suggested ideas will ever have a relation to the improvement of the moral and physical condition of man. However strong the objections to statistics in general, those against the application of the science to the improvements of practical medicine have been still more vehemently urged. The impossibility of applying the precision of numbers to phenomena so fleeting and variable as those of disease has been the theme of many a pen. Now it would be no difficult thing to show that numbers lie at the root of almost all our opinions in practical medicine; but these numbers, however convincing to the mind by which they are estimated, are not communicated to others in a form which wins for them confidence, or constitutes them safe grounds of reasoning. An example or two will best illustrate this position. The first we shall take from the excellent letter of Mr. Farr to the Registrar-General, which forms so important a part of the Report before us:

"The prevalence of a disease, for instance, is expressed by the deaths in a given time out of a given number living with as much accuracy as the temperature is indicated by a thermometer; so that when the mean population of the

district is known, the rise and decline of epidemics may be traced exactly, and it will then be possible to solve the problem, whether certain tribes of epidemic disorders constantly follow others in one determined series or cycle Loose phrases are still current, for which numerical formulæ will be substituted.—Sydenham, one of the most accurate of medical writers, in speaking of smallpox, employed such terms as these: (1661) 'It prevailed a little, but disappeared again.' (1667-9) 'The smallpox was more prevalent in town for the first two years of this constitution than I ever remember it to have been.' (1670-2) 'The smallpox arose; yielded to the dysentery; returned,' &c. &c. These terms admit of no strict comparison with each other; for it is difficult to say in which year the smallpox was most fatal, and impossible to compare Sydenham's experience thus expressed with the experience of other writers in other places and other ages; for 'prevailed a little,' 'raged with violence,' and similar terms, may imply either that smallpox destroyed 1, or 2, or 5, or 10 per cent. of the population. The superior precision of numerical expressions is illustrated by a comparison of Sydenham's phrases with the London bills of mortality in the same years." (p. 87.)

Mr. Farr gives a table containing the mortality from smallpox during each year from 1661 to 1680 inclusive, and then remarks:

"The 1987 deaths from smallpox in 1668, and the 951 deaths from that disease in the year following, express the relative intensity of smallpox in distinct terms. The method of the parish clerks, although imperfectly carried out, was the best. Sydenham guessed the quantity, and called it great or small; the parish clerks measured it, and stated the result in figures." (p. 88.)

So with regard to the treatment of disease, an experienced physician pursues one line of practice, and avoids another, because he has found the one he adopts more generally successful. Here numbers are mentally compared, and when he is requested to count, he is merely asked to do in a precise form what he has constantly been doing in a vague one,—to reduce his experience to a state which admits of comparison with that of others, and renders it an augmentation of the sum of general knowledge, instead of one which is conclusive to his own mind only. Admitting that age, sex, temperament, and idiosyncracy modify diseases so much, that there is not often a perfect identity between individual cases, and that something special may be required in the conducting of many of them; yet a general accordance will be found in the treatment of diseases of the same kind by the same practitioner. Let us take rheumatism as an example: one man may prefer for its treatment bloodletting, another calomel and opium, another antimony, colchicum, &c.; but, however individuals may differ, each will be found to pursue a consistent plan in the treatment of this disease. If each, then, would throw an account of his successes and failures into one common stock, conclusions beneficial to therapeutics might be deduced; for, though the frequency of specialities in individual cases might, according to the well-known doctrine of probabilities, render such a group as any medical man could furnish from his own practice, an insecure ground of reasoning; yet as contributions to some extensive fund, in which, from its great amount, incidental irregularities should compensate each other, and which should furnish the means of comparing together very numerous cases of various diseases, grouped according to correct resemblances, the various groups being similarly and dissimilarly treated, all facts faithfully recorded and well arranged, would be of great value. What appears to be wanting is the machinery for applying the numerical method to the-

rapeutics, on a scale sufficiently ample to obviate the errors which might arise from special and exceptional cases abounding in small groups. Such a Society as the *Société Médicale d'Observation*—of which we have already given some account, and of which, whilst we have censured its abuse of the numerical method, we have acknowledged the plan to be admirable*—established in this country, and receiving statistical reports from its various parts, would meet, as it appears to us, the object to be attained, *if it be attainable by means of statistics.* The main argument against such an expectation, that "the observation necessary for the safe treatment of diseases comprehends many circumstances which distinguish *every* individual case from the rest," we think too broadly stated. *Individual* diseases (cases of disease) resemble each other; they admit of arrangement founded on such resemblance; and, according to the degree of it, are classified into species, genera, orders, and classes. Now it does appear somewhat extraordinary, that their safe treatment should never depend upon the circumstances in which they agree, but uniformly upon those by which they are distinguished. This appears contrary to the whole analogy of nature, and, what is still more important, we venture to assert will be contradicted by the practice of almost every physician in the world. He will be found treating his cases of pneumonia, &c., by means the same at least in *kind*, though in *degree* they will be varied according to the age, strength, and sex of the patient. The contrary supposition, that his practice was founded on the distinctions, not on the resemblances, would be imagining every practitioner to be a law to himself, and would amount to the total abrogation of what little therapeutic principles we possess. If diseases themselves admit of classification, which, as is justly observed in the Registrar-General's Report, "is another name for generalization," and if "successive generalizations constitute the laws of the natural sciences;" if, too, it appears, that this kind of statistics is rapidly teaching us that the circumstances which induce diseases, and the correction of which prevents them, admit of classification—it seems incredible that those circumstances which modify their progress beneficially, in one word, their treatment, should be utterly abnormous. The argument which has sometimes been advanced against the application of numbers to therapeutics,—and which we ourselves have used,—that a man must first consult his tables, and make his calculations, before he can act in any given case, is not to be admitted without much qualification; as thus broadly stated it involves a misconception of the mode in which statistics (if it can improve) must improve therapeutics. The long-continued and conjoint labours of many must first furnish an ample store of well-observed and well-arranged facts, and from these general principles must be deduced, and become the common stock of the profession. We are by no means certain that this can be done, but we are certainly of opinion that it ought to be attempted, and we very much misconceive the character of the members of our profession, if they are not found zealous collaborators in such a work, should it once be instituted.

These hopes of benefit to be conferred upon the most important branch of the healing art by statistics, have not been engendered by the Regis-

* See Vol. V., p. 154, of this Journal.

trar-General's Report: for in the early part of our career we expressed such hopes;* but they have certainly derived additional vigour from the mass of precise information on the causes, prevalence, and diffusion of disease which this Report imparts. We shall now proceed to convey to our readers such portions of this information as we deem most material, and our limits will admit, premising that the toil of analyzing tables and arranging the results of such analysis has been much abridged by the valuable letter, including tables and abstracts of the causes of death, by Mr. Farr, which forms so important a part of the Report.

In framing the general abstracts of the number of marriages and births, the Registrar-General has not attempted to specify the localities, but has given one statement for the whole kingdom of England and Wales, only distinguishing the number of marriages or of births registered in each quarter of a year. In the abstract of deaths, however, he has entered into more minute details, and has assigned the following very satisfactory reasons for his proceeding:

"Such details are of acknowledged value, as data for determining the laws of mortality—as bases for calculations affecting the interests of millions. Tables exhibiting the proportion of deaths at every successive year of age are among the most important materials from which are deduced the true principles on which should be founded the systems of life annuities and of life insurances, and the rules of friendly societies established for the use of the poorer classes. The materials hitherto accessible are admitted to have been too limited for framing, satisfactorily, tables to regulate the amount of contribution at various ages, by which members of such societies may become entitled to allowances in old age, or to sums payable at death. The insufficiency of the data hitherto collected, and the contradictory nature of the several tables founded upon them, are strongly set forth in the Report of the Select Committee of the House of Commons, in 1827, on the laws respecting friendly societies. It is there stated that, 'according to the Northampton Tables, out of 1000 persons existing at the age of twenty-five, there survive at the age of sixty-five 343 persons. By the Carlisle tables, no fewer than 513 persons will survive;' whereby it appears, 'that a society which should adopt the Northampton Tables would, if the mortality among its members should correspond with the Carlisle Tables, have *three* annuitants where it calculated upon *two*. Of those annuitants, moreover, a larger proportion would live to enjoy the annuity for a considerable number of years; for instance, of the 343 persons who would be annuitants according to the Northampton tables, ninety-eight would live for fifteen years; according to the Carlisle Tables, 162 persons would survive through that period, and attain the age of eighty years." (pp. 15-16.)

Still further, to show the necessity for better ascertained facts to elucidate these important subjects, the Registrar-General annexes, in a tabular form, the results of seven approved tables of mortality, the discrepancies among which are excessive. The first two columns will suffice to render these manifest. Of 100,000 persons aged twenty-five, there would be alive at the age of sixty-five, according to Dr. Price's table, founded on the births and burials at Northampton, 34,286; the first Swedish tables, as published by Dr. Price, for both sexes, 43,137; M. De Parcieux's tables, founded on the mortality in the French tontines prior to 1745, 51,053; Mr. Milne's table, founded on the mortality observed at Carlisle, 51,335; Mr. Griffith Davies's table, framed from

* See the review of the work of M. Louis, "*Sur la Saignée*," &c., in our Second Number.

the experience of the Equitable Life Insurance Office, 49,330; whilst of Mr. Finlayson's tables, founded on the experience of the Government Life Annuitants, the one, as mentioned in his evidence in 1825, makes the survivors 53,470; and the other, stated in his evidence in 1827, rates them at 53,950. Of 100,000 persons aged sixty-five, there would be alive at the age of eighty, according to the same authorities respectively, 28,738, 23,704, 29,873, 31,577, 37,267, 38,655, 37,355.

These discrepancies are certainly sufficient to justify the recommendation of the Report of the Committee of the House of Commons, that measures be adopted for making "an accurate and extensive collection of facts," whereby may be facilitated "the solution of all questions depending upon the duration of human life." Mr. Lister prudently abstains from making an estimate of the probable duration of life at successive ages from the abstracts of deaths for a single year; but confines himself to recorded facts, accompanied with deductions that are clear and unimpeachable, justly remarking that each year's accumulation will increase the value of the records, by augmenting the number of facts on which calculation may be brought to bear.

Not only were there great discrepancies in the estimates of mortality as regards the general population, but there was an entire want of all data by which districts widely differing as to the occupations of the inhabitants, and other circumstances by which their health and longevity would be especially influenced, could be compared, or rather discriminated. It was justly stated by Mr. Milne to the Committee on laws respecting "Friendly Societies," that "no one scale of contributions could, with propriety, be adopted by all such societies; but that one composed of members living in or near a manufacturing town required a table very different from that which would be required in places where the population was less dense, and where a considerable proportion of the members were chiefly employed in the open air; but that these were differences which he could not presume to estimate, for want of *data*." The actuary of the National Debt Office gave testimony of the same purport to the Committee on Parochial Registration in 1833.

The Registrar-General, mindful of the various circumstances influencing public health and longevity, has not confined himself to an abstract of deaths for the whole kingdom of England and Wales, but has felt how important it was, to use his own language, "to compare town with country—agricultural districts with manufacturing and mining districts—the hilly with the low and level—the maritime with the inland—the eastern and northern with the western and southern parts." In carrying out this plan, he remarks, "I have had regard not so much to the observance of established boundaries as to those circumstances from which diversity may be expected to arise; and I have in some instances included in the same table contiguous counties similar in soil, climate, elevation, and the employments of the people; and have, in other instances, disregarded the boundary of the county, where it was desirable to compare two large portions of its inhabitants pursuing very different occupations. The expediency of causing each division to consist of integral unions, or superintendent-registrar's districts, has also precluded a very strict adherence to the boundaries of the counties, there being many

unions situated on the outward verge which include portions of two or more counties." (p. 18.)

The amount of registration during the first year is—births 399,712, deaths 335,956, marriages 111,481. The Registrar-General thinks that, with respect to the registration of deaths, the system has, during the first year, been eminently successful. The registration of births has been less perfect than that of deaths; and we regret to observe that the Registrar-General ascribes this comparative imperfection, in a considerable degree, to an extensive and stubborn opposition. It is consolatory, however, to observe, from the increase of the entries during the third and fourth quarters, that the success of this branch of the plan is progressive, and that the various impediments opposed to it are not continuing to prevail against it.

The whole of England and Wales is divided into twenty-five districts or divisions, according to the principle formerly mentioned; the statement of the population in each district being taken from the census of 1831, the latest source of information on the subject given on authority. This statement is, of course, considerably below the reality; although a close approximation might be made to the present population of each district, the population of 1831, being common to all, answers perfectly well the purpose, when the object is to compare the relative salubrity of districts. Besides the population, the number of families, according to the same census, in each, and the proportion engaged in agriculture, trade, manufactures, and handicraft, and of families whose occupations are not referrible to either of these heads, and the area of each district in acres, are stated. The whole plan appears extremely well devised for the discovery of certain general sources of health and longevity, and their opposites; for showing how far these conditions are influenced by the concentration of the people in towns, their diffusion over a wide surface, their occupation in agriculture, manufactures, or mining, and by the nature of the surface, whether mountainous or plain; and, by a little further scrutiny into the food and other circumstances of the inhabitants of the respective districts, for the illustration of most subjects of interest to the statistical enquirer.

Examples of the influence of certain of the circumstances mentioned over the duration of life are readily deducible from the tables. In the whole of England and Wales, out of 1000 deaths, 145 have been at the age of seventy and upwards; while in the thinly-peopled districts of the north riding and the northern parts of the west riding of Yorkshire, and in Durham, except the mining districts, the proportion has been as high as 210. In Northumberland (excluding the mining district), Cumberland, Westmoreland, and the north of Lancashire, the proportion has been 198; in Norfolk and Suffolk, 196; in Devonshire, 192; and in Cornwall, 188. Where the population is densely congregated, the results are very different. In the metropolis and its suburbs, the proportion who have died at seventy and upwards has been only 104; in Birmingham, 81; in Leeds, 79; and in Liverpool and Manchester, only 63. In the smallness of the proportion of such deaths in the four large manufacturing towns, as compared even with the metropolis, we trace the influence of employments in addition to that of concentration. The operation of both the causes of mortality just mentioned seem to be

rendered manifest by a comparison of the reports from Division 22, including the north riding of the county of York, with such portion of the west riding as is not comprised in Division 20, and the county of Durham (*except the mining parts*), and that of Division 23, comprising the *mining parts* of Northumberland and Durham, and the populous towns of Newcastle, Sunderland, and South Shields. Division 22 has a population of 319,042 on an area of 2,104,736 acres; Division 23, has one of 318,941, or within a trifle of the same amount, on an area of 688,708 acres, or less than one third of that of Division 22. Of the 66,175 families contained in the 22d Division, 25,082 are engaged chiefly in agriculture; and of the 69,532 families contained in Division 23, but 7,298 are so occupied. In the former of these divisions, we find the mortality to be, within the year, 6,953, or one in forty-five of the population, according to the census of 1831; that of the latter 8,112, or one in 39·3 of the population, according to the same estimate. Of the 6,953 deaths in the former division, 1,363, or nearly one fifth, took place at the age of seventy or upwards; whilst in the latter, 1,101, or about one eighth of 8,112, occurred at the same period of life. The mining parts of Staffordshire and Shropshire, compared with the adjacent rural district, present a similar deficiency of deaths in old age, and an excess of general mortality, so far as that can be deduced from the population of 1831, such as have been pointed out in the case of Durham and Northumberland.

The Registrar-General observes a very marked diversity in the proportion of deaths of infants in different parts of the country. In the mining parts of Staffordshire and Shropshire, in Leeds and its suburbs, and in Cambridgeshire, Huntingdonshire, and the lowland parts of Lincolnshire, the deaths of infants under one year have been more than 270 out of 1000 deaths at all ages; while in the northern counties of England, in Wiltshire, Dorsetshire, and Devonshire, in Herefordshire, Monmouthshire, and Wales, the deaths at that age, out of 1000 at all ages, scarcely exceeded 180.

Some supposable sources of error are pointed out to those who might be disposed to draw conclusions from the tables. In the 1st (metropolitan) Division the deaths of persons between the ages of twenty and fifty are 241 out of 1000 at all ages; whilst in Division 6, comprising the counties adjacent to the metropolis, they are only 192; but it is discovered, from the enumeration made in 1821, that of 10,000 persons living in Middlesex, there were 4,522 between the ages of twenty and fifty, and in Bedfordshire, Buckinghamshire, and Hertfordshire, but 3,581 persons of the same age; a difference attributable to persons born in the counties near London quitting them, as they emerge from childhood, to seek employment in the capital. A similar explanation is found of the great discrepancy in the proportion of deaths under five years of age in Division 19, comprising Lancashire, south of Morecombe Bay, with the exception of Manchester and Liverpool, and the same in Division 25, comprising Herefordshire, Monmouthshire, and Wales. In the former division, according to the enumeration of 1821, there were, out of 10,000 persons, 3,293 at the period of life mentioned; in the latter, 2,754. As warnings and corrections to erroneous calculations, Mr. Lister has appended to the abstract of deaths the table containing the ages of

persons in the several counties of England, on the 28th of May, 1821, with the addition of the same for Wales.

We now turn to those portions of the Report which fall most immediately into our own province—the letter of Mr. Farr, and the tables containing an abstract of the causes of death in the twenty-five divisions. We shall have ample occasion to express our conviction that justice has been done to this very important subject by Mr. Farr; but no pains taken by him can give the Report the full value of which it is susceptible. This must be derived from the correctness of the *items;* and here all depends upon the body of medical practitioners throughout the kingdom: and, with great confidence in their intelligence and disposition to render a public service, we yet would impress upon them the importance of the duty they are now called upon to discharge, without any reward but the conviction of a benefit bestowed upon their fellow-creatures. If their own ideas of the cause of death are accurate in any given case, it will be less trouble to assign the real than a fictitious one to the registrar, and consequently an inaccurate return will be an impeachment of the soundness of their own judgment: so that, besides the well-being of the community, and the advancement of knowledge (terms which are well nigh convertible), motives of powerful influence over a profession preeminent in science and benevolence, regard to individual character should secure the accuracy of the return. The omission or refusal to give information to the registrar, seeing that it is done with little or no trouble to the medical man, is a case we cannot contemplate, or, of course, discuss.

The first object which arrests our attention is the statistical nosology. Diseases are arranged under the heads of, 1st, epidemic, endemic, or contagious diseases, and 2d sporadic diseases, whilst deaths by violence constitute a 3d division in the nosology. Of the first class, the species are simply enumerated; but those of the second are divided according to their seat in the nervous system, in the organs of respiration, of circulation, of digestion, in the urinary organs, in those of generation, of locomotion, in the integumentary system, and of uncertain seat. The diseases thus classified amount to nearly 200, and, as necessarily *only* fatal diseases are comprehended, the arrangement seems sufficiently ample without needless minuteness. The names recommended to be used are given in a first column, and a second column comprehends synonymes and provincial terms. Mr. Farr is evidently anxious that precise terms should be employed. He objects, for instance, to the term inflammation of the bowels, as not distinguishing peritonitis from enteritis; and contends, very properly, with M. Louis and, we believe, all good pathologists, for the limitation of the word *enteritis* to inflammation of the internal membranes of the bowels, whilst *peritonitis* designates inflammation of the peritoneum, whether lining the abdominal muscles or investing the intestines. In these views we perfectly accord.

Mr. Farr is very properly anxious that a uniform statistical nomenclature should be adopted, and indicates the prevailing sources of confusion, such as designating one disease by various terms, applying the same term to different diseases, employing vague terms, and registering complications instead of primary diseases. This latter point he exem-

plifies by pneumonia occurring after hooping-cough, and proving fatal; and he would, in this case, refer the death to the primary disease, and adhere generally to this principle in similar instances. It appears to us that the members of the profession should be universally informed of the nomenclature and plan of registration most suitable to the purposes of those with whom will rest the labour of arranging the innumerable items received from various quarters, and making the proper deduction from them; and this can be accomplished only by furnishing all medical men with a sketch of Mr. Farr's classification, and the pertinent remarks on nomenclature with which it is accompanied; and this might be accomplished, one would suppose, through the superintendent-registrars, at small expense of money and trouble.

In table A, at page 120 of the volume, will be found an abstract of the causes of death registered in England and Wales, from July 1st to December 31st, 1837, both inclusive. The number of deaths registered during this half year is 148,701, of which 75,159 occurred in males, and 73,542 in females. The population of the two countries is estimated according to the population of 1831, with an addition framed according to the decimal increase which has been calculated at four periods, and found to be very uniform. Corrections were required, in the first place, for a deficiency in the enumeration of males; and again, for the deaths which escaped registration during the first two or three days of the first quarter, and could not receive the compensation which would necessarily occur in each succeeding quarter, those failing to be registered in the second being compensated for from the first, those in the third from the second, and so successively. With these corrections, the annual mortality was found to be, males 2·08, females 1·97, and mean of the two sexes 2·02 per cent.; or, in other language, males 1 in 48, females 1 in 51, and mean of the two sexes 1 in 49. This estimate is formed from the latter half of the year 1837, during the early months of which influenza prevailed epidemically. The latter portion of this year was, as is well known to the profession, very healthy; a condition, be it remarked, very generally existing, for a considerable period after epidemics; we should, consequently, conclude, that the estimated mortality, though there is every reason to think very accurate for the period, is below the average mortality of the country for a series of years.

The same table furnishes much valuable information on the relative prevalence of various classes of diseases, and of individual diseases. The first class, comprising those which are epidemic, endemic, and contagious, was fatal to 32,537, of whom 16,190 were males, and 16,347 females, being in the proportion, relatively to the numbers of the two sexes, of 4·7 to 4·6 per 1000 annually, and of 4·651 per 1000 for the mean of the two sexes. Smallpox, croup, thrush, diarrhœa, dysentery, and cholera, proved more fatal to males than females; while influenza and hooping-cough, particularly the latter, cut off a greater number of females; and typhus, erysipelas, scarlatina, and measles affected both sexes equally. The mortality from smallpox, measles, scarlatina, hooping-cough, croup, thrush, and diarrhœa, constituting 3·036 per 1000 of the entire mortality from epidemic, endemic, and contagious diseases, occurred principally among children; although, it is stated, a considerable number of adults were carried off by these

diseases. Cholera, dysentery, *influenza*, ague, typhus, erysipelas, and syphilis, constituting the remaining 1·615 per 1000, attacked adults chiefly, though they did not entirely spare children. The conclusions regarding certain of these latter diseases are deduced from a small number, but they are, nevertheless, consonant with the general experience of the profession. The total number of cases of influenza given in the table is 484; but the report of Mr. Farr comprises, it will be remembered, only the latter half of 1837; and it was during the first quarter of this year that influenza was so extensively epidemic. The results of this registration, however, confirm the recorded opinion of a numerous body of practitioners, formed from observation of the epidemic in its intensity, as may be observed on reference to the "Report on Influenza," contained in the 6th volume of the "Transactions of the Provincial Medical and Surgical Association."

Smallpox destroyed 5,811 in the half-year. This disease was epidemic in several parts of the country, particularly in Liverpool, Bath, and Exeter. Of 1,056 deaths which occurred from it in Bath, Exeter, parts of Shropshire, Worcestershire, and the metropolis, 887, or considerably above four fifths of the whole, took place in infants under five years of age; and Mr. Farr regards the statement of these ages as an approximation to the ages of the whole 5,811. He remarks, with truth, how far the fact mentioned would go to settle the question as to the progressive decrease of the preventive power of vaccination from the period of its performance, provided the medical attendant would ascertain whether the individual dying of smallpox had been vaccinated, and the interval between his being so and his death, which might easily be done. He thinks that vaccination is too long delayed by all classes, and quotes the early deaths at Bath and Liverpool, to prove that it should be performed within the third month. We find that, at the two places mentioned, twenty-two children died of smallpox between birth and the second month, forty-one between the third and fifth month, fifty-four between the sixth and eighth, and fifty-nine between the ninth and eleventh, making a total of 176 deaths within the first year of age. Of those in the second year, there perished 158; and in the third, 110. The numbers then steadily decrease till we reach the tenth year, and in that period of life only three died; and at all ages above ten, but thirty-five. We need scarcely remark how great a value such a statement as this would have possessed, had it been connected with the information relative to vaccination which the reporter desiderates. As it stands, it furnishes an argument, though not a positive one, in favour of vaccination; for we find a progressive decrease of mortality from smallpox in proportion as the probabilities of this operation having been performed increase.

Under sporadic diseases of the nervous system, 21,852 deaths (fifteen per cent. of the total number) were registered. The mean annual rate of mortality from the whole group was 3·1 per 1000; but males suffered more than females, in the proportion of 3·4 to 2·8. Paralysis, chorea, and epilepsy were the only diseases of this class which affected more females than males. Cephalitis, hydrocephalus, and convulsions, the diseases chiefly but not exclusively incidental to young children, cut off 2·4 males and 2·0 females out of 1000 living; apoplexy, ·42 males, ·35

females; tetanus, ·013 males, ·003 females; delirium tremens, ·025 males, ·002 females. Mr. Farr justly remarks, that tetanus generally follows wounds, and is therefore remotely caused by accidental violence, to which males are more exposed than females. He remarks, too, that delirium tremens is also sometimes brought on by wounds in drunkards, and in persons exhausted by passion or misery. Admitting that traumatic delirium is, in its outward manifestations, very much allied to delirium tremens, if not identical with it, in other classes besides those mentioned, we should yet demur to the opinion intended to be conveyed in this sentence, being convinced that females under similar circumstances of intemperance are very much less prone to this disease than males. We would observe, that the whole number of cases of this affection reported as having been fatal during the half year is so small, being only ninety-five, of which eighty-six were in males, nine in females, as to convince us that cases which ought to have been returned under this head have found their way into that of cephalitis or some other disease.

In the class of diseases of the nervous system, ten deaths, nine of females, one of a male, are ascribed to mental emotions of one kind or another; seven to fright, one to grief for the death of a son, and two to a broken heart. In certain of these, the alleged cause and result occurred within so short a period of each other, as to render the causation less equivocal than it frequently is in such cases. For instance, a female, aged sixty-three, died on November 7th, from trouble for the death of her son, which took place October 30th; and, again, the death of a female, aged forty-one, which occurred November 14th, is ascribed to "fright," occasioned by the sudden death of a brother, and her brother died on the 19th of October. Under the same head are noted several curious cases, such as laryngismus stridulus, one male, one female, aged 100, (we copy exactly, but, though the punctuation would lead to the conclusion that the parties were both of the same age, it appears probable that this is a typographical error, and that the female only was 100;) chorea, a female, aged seventy-five; hysteric fits, one male, aged sixty-seven, two females, aged seven and fourteen days, and others which we pass over, that we may notice the diseases of the respiratory system.

From this class there arose 38,522 deaths, or twenty-seven per cent. of the total number; of these, 27,754, or twenty per cent. of the total deaths, were owing to phthisis. The deaths from this dreadful disease in males amounted to 12,968, in females to 14,786, the ratio being 4·155 of the latter to 3·771 of the former. The mortality from all diseases of these organs was equal in males and females; bronchitis, pleurisy, pneumonia, and asthma destroying more males than females. The deaths from the whole class were in the proportion of 5·5, and of phthisis in that of 4 per 1000 of the population annually. These proportions correspond very considerably with the estimates of recent statists regarding the general population; but they are very much lower, indeed, than those deduced by Major Tulloch from the mortality of the troops in England, especially from that which occurs in the foot-guards. Mr. Farr's law of the mortality from this disease deserves to be recorded. He says, "it may be laid down as a general principle, that wherever the proportion of deaths from phthisis, compared with the total deaths, is

high, the absolute mortality is low, and that the absolute mortality from phthisis itself is low. The deaths out of the living express the real tendency to phthisis."(p.116.) This sounds somewhat paradoxical, but converted from the abstract to the concrete, it will be found to be true. In Great Britain, the deaths from phthisis constitute one fifth of the total deaths—a high proportion; but the total deaths are but twenty annually out of 1000 living—a low mortality; and the deaths from phthisis are but four annually out of 1000 living—a small proportion, indicating, of course, little tendency to the disease. In the Windward and Leeward Command in the West Indies, the deaths from consumption constitute but one ninth of the total deaths—a low proportion; but the total deaths are 93½ annually out of 1000 living—a high mortality; and the deaths from phthisis are 10½ per 1000—a high mortality from this disease.

Under the fourth class, or diseases of the organs of circulation, 1,596 deaths are recorded, of which 902 occurred in males, and 694 in females, or in the proportion per 1000 of the living of ·262 of the former, and ·195 of the latter. The proportion of these deaths ascribed to heart-disease, Mr. Farr remarks, is much below that stated by Dr. Clendinning, and much below the truth. Our own observation convinces us of the justice of this remark; and we are convinced that many cases in which the primary disease was seated in the heart have been returned under the heads of hydrothorax and dropsy, affections which, in a vast majority of instances, are consecutive.

Five thousand one hundred and fifteen males, and four thousand seven hundred and thirty-five females, died of disease of the digestive organs, and the annual rate of mortality in the males was 1·5, in the females 1·3 out of 1000 living. There is little in the return of individual diseases under this head to call for comment, besides the very remarkable facts that there are but twenty deaths from peritonitis (of which seven were in males and thirteen in females), and *not one from enteritis,* whilst of *gastro-enteritis* there are 3,396! This is "passing strange," but we are by no means convinced that it is "not more strange than true."

"Diseases of the urinary organs destroy five times as many males as females, the rate of mortality of the two sexes, under this head, having been ·199 and ·037 per 1000. This disparity has been ascribed to mechanical causes; but will a mechanical explanation account for the fact, that sixty-eight males and only twenty-seven females died of diabetes? Dr. Yelloly, in a paper published in the Philosophical Transactions, estimated that 1 in 108,000 persons was cut annually for stone in England and Wales. It appears from the table, that 47 in 1,000,000 males, and 5 in 1,000,000 females, die of stone and gravel. The latter, it must be admitted, is a vague term in popular language; but the mortality from stone is certainly 1 in 100,000 annually. Bright's disease is registered "disease of the kidneys." The coagulability of the urine is often undetected by careless practitioners. A female child, aged two years, is stated to have died *suddenly* of diabetes." (p. 105.)

We shall now notice, in a less regular manner, certain sources of mortality. The deaths in childbed amount to 1,265, of which we learn, from an estimate by Dr. Ferguson, seven eighths arise from puerperal fever. The annual rate of mortality by childbirth in females generally, is 3·55 in 10,000; or, calculating only those at a child-bearing age,

perhaps 0·8 per 1000. Supposing 290,000 births and miscarriages to have occurred in the period, nearly four in 1000 were fatal to the mothers. It is remarked as a subject of great regret, that 2,500 die in childbirth every year in England and Wales. Under the head of diseases of the skin, we are surprised to observe seven fatal cases of leprosy. Hemorrhage, the effusion of blood, is more fatal in males than females, in the proportion of ·107 to ·060; dropsy, the effusion of serum, more fatal to females than males, in the proportion of ·882 to ·711. Fourteen thousand one hundred and five deaths were ascribed to inflammation of one part or another, consequently one tenth of the fatal cases arose from these affections. Carcinoma was, as might be supposed, more fatal to females than males, the deaths of the former being 875, of the latter 355. Sixty-three deaths were ascribed to starvation, or nearly one annually to a population of 111,000. Mr. Farr's remarks on this point are very just:

"The want of food implies the want of everything else, except water; as firing, clothing, every convenience, every necessary of life, is abandoned at the imperious bidding of hunger. Hunger destroys a much higher proportion than is indicated by the registers in this and every other country; but its effects, like the effects of excess, are generally manifested, indirectly, in the production of diseases of various kinds. The privation is rarely ever absolute; the supply of food is inadequate to supply the wants of the organization, which requires daily animal or vegetable matter containing not less than nine ounces of carbon." (p. 106.)

All this is true, but it is likewise true, as Dr. King has stated, that a *low degree* of vitality arising from deficient nutriment, is not incompatible, in many cases, with a considerable degree of longevity. The stature is stunted, the frame emaciated, and the intellectual and moral qualities are low; and yet these step-children of nature, or rather, we should say, of art, often live long. This, we think, is especially the case where the nutriment has been deficient from the cradle. The organization is then, in many cases, rather defective than vitiated; and though, in frequent instances, the same circumstances produce structural change and disease, yet in others the condition is such as we have described.

The subject of sudden deaths produces some remarks from Mr. Farr worthy of record. He asks, what is the nature of the sudden deaths of which, notwithstanding some difficulties attending the registration of inquests, it appears that ·184 in males and ·118 in females per 1000 are recorded annually? It will be observed, he remarks, that the proportion of sudden deaths is fifty-six per cent. higher in males than in females; the mortality in apoplexy is nineteen per cent., in hemorrhage seventy-eight per cent. higher in males than in females. It is probable, therefore, that sudden death is frequently the effect of hemorrhage. Some strictures are made, with too much truth, on the proceedings of coroners' courts, the rude and cursory examinations after death, of which the results are made the grounds of their decisions; the probability that many cases of poisoning escape detection, from the want of a careful examination of the contents of the stomach; and the uniformity of the verdict of "natural death" on all prisoners who die in gaols, instead of a statement of the cause of death in intelligible language. He says, in concluding this branch of the subject, that "the causes of deaths

registered as the result of a solemn, judicial investigation, are the most unintelligible in the register, as it is impossible to attach a specific idea to 'natural death,' to 'visitation of God,' and several other phrases in use in coroners' courts."

His remarks on violent deaths are very important:

"4,845 violent deaths, 3,605 of males, 1,240 of females, were registered; and the annual mortality of males and females under this head was 1,048, and ·348 per 1000, the males having suffered three times as much as females. The excess of males was 2,365, and it more than counterbalanced the mortality of childbirth. If all the violent deaths had been entered in the abstract, the mortality of males under this head would probably have equalled the mortality from typhus. This deserves attention. The individuals carried off by violence, typhus, consumption, and childbirth, are in the meridian of life; and in a political sense their lives are of the highest value. Drowning in rivers and in the sea, burning, injuries in manufactories, explosions in mines, are frequent causes of violent deaths. Suicides are included; the ages and sex of forty-four were distinguished." (p. 107.)

In a note are described several of the causes of violent death. Some of them are sufficiently curious, such as killed by a lion, one male; by the bite of a donkey, one female; by loss of blood from the bite of a ferret, a male aged four months; and by taking *Morrison's pills*, two males. This last is an especially discreditable item. Some of the causes are more unmixedly tragic: thus there were killed by lightning, eleven males, four females (all in the first quarter); by an explosion in a pit in Lamesby district, Chester-le-Street, and the same number in a pit in the Springwell *county* (we believe this to be a misprint for *colliery*), likewise near Chester-le-Street. We regard these two latter items as disgraceful both to the science and humanity of the country. What is the Committee of the House of Commons, which has been sitting for years, on the subject of accidents in coal-mines, doing? We know what it is failing to do.

Mr. Farr's estimates of the comparative mortality in towns and the open country, are among the most important, to the statesman and physician, which the volume contains. The length to which our extracts and remarks have already extended, compel us to dismiss this portion of the subject with more brevity than we wish; but a portion of the more general facts elicited we shall present to the reader.

In table C (p. 148), is an abstract of the deaths from various diseases for the half year in the metropolis, with a population of 1,790,451, on an area of seventy square miles, and in the counties of Cornwall, Dorsetshire, Devonshire, Somersetshire, and Wiltshire, with a population of 1,723,770, on an area of 7,933 square miles. In the metropolis, the total mortality for the period was 24,959; in the counties with the same population, 15,220. Nor is this very great advantage to the country limited to a comparison between it and the metropolis only; for in table D, we have, with results nearly the same, the proportion of deaths from various diseases in the counties of Essex, Gloucester (except Bristol and Clifton), Hereford, Norfolk (except Norwich), Suffolk, Sussex, and Westmoreland; and in the districts of Aston, Bath, Birmingham, Bristol, Cambridge, Carlisle, Clifton, Derby, Dudley, Exeter, Leeds, Leicester, Liverpool, Manchester, Maidstone, Newcastle-on-Tyne, Northampton, Nottingham, Salford, Sheffield, Stoke-on-Trent, Sunderland, Wolver-

hampton, and West Derby. The cities have a population of 1,762,710 on an area of 677 square miles; the counties one of 1,776,980 on an area of 9,312 square miles. In the former, the mortality in the half-year was 22,994; in the latter, 14,473. It will be observed, that the comparison between the metropolis and other considerable towns is less disadvantageous to the former than might have been expected. The comparison between extensive country districts wide asunder shows a very close correspondence indeed.

In table E, which we regard as one of singular value, we are presented with an abstract of the deaths from twelve classes of fatal diseases in city and country districts, the former with a population of 3,553,161, the latter with a population of 3,500,750. In the cities there perished, from epidemic, endemic, and contagious diseases, 12,766; in the country 6,045; from diseases of the nervous system, the numbers stood respectively 7,705, 3,607; from those of the respiratory organs, 12,619, 7,847; of the organs of circulation, 590, 309; of the digestive organs, 3,467, 1,832; of the urinary organs, 219, 161; of the organs of generation, 460, 265; of the organs of locomotion, 262, 154; of the integumentary system, 62, 55; of uncertain seat, 4,396, 1,657; of age, 2,924, 3,102; of violent deaths, 1,370, 929; of causes not specified, 1,104, 1,657; constituting, collectively, a total of deaths in the cities of 47,953, and in the counties of 29,693.

We subjoin the author's comments on these tables:

"The concentration of the population in cities doubles the deaths from the first two classes of diseases; the ratio of deaths having been as 1 to 2·11, and 1 to 2·13; and upon reference to the individual diseases in tables C, D, it will be observed that the augmentation in the latter class occurs principally in convulsions and hydrocephalus: Deaths by convulsions—counties 1,347, cities 3,723, ratio 1 : 2·76; by hydrocephalus—counties 559, cities 1,540, ratio 1 : 2·75. It has already been intimated, that convulsion is a frequent intercurrent symptom in diarrhœa and diseases of the epidemic class in infants; it may exist, however, as an independent affection, and in that case has clearly, as well as hydrocephalus, with which it is allied, an epidemic character. A similar remark will apply to pneumonia and bronchitis, of which 1,209 cases were registered in the counties, 2,865 in the cities; ratio 1 : 2·37. The pulmonary inflammation was, in many cases, developed in the course of measles, influenza, and other diseases of the first class. The three following diseases, which principally affect adults between the ages of fifteen and sixty-five, show that unhealthy places augment the fatality of diseases in different degrees:

	Counties.	Cities.	Increase per cent. in Cities.
"Deaths by consumption	5,857	8,125	39
,, childbirth	217	372	71
,, typhus	1,564	3,456	221

"This gives the classification a peculiar property. Wherever the absolute mortality is low, the number of deaths in the epidemic class is less than the number in the pulmonary class; and, on the contrary, wherever the deaths in the first class exceed or equal those in the third, it may be affirmed that the absolute mortality is high." (pp. 110-11.)

This last sentence is an explanation of the law of phthisis, which we have already quoted. It seems that it is the variation in the mortality from the epidemic class which produces the contrast between the proportion of deaths from phthisis and the total deaths, in which the law consists.

Mr. Farr, after showing that the Poor Law Enquiry and successive Parliamentary Committees have proved that the excess of the mortality in towns, as compared with the country, does not arise from the difference of the food of the inhabitants of the respective localities, thus expresses himself:

"The source of the higher mortality in cities is, therefore, in the insalubrity of the atmosphere. Every human being expires about 666 cubic feet of gas daily, which, if collected in a reservoir, would destroy other animals; and is constantly producing, in a variety of ways, the decomposition of animal and vegetable matter yielding poisonous emanations in houses, workshops, dirty streets, and bad sewers. The smoke of fires and the products of combustion are also poisonous. All gases and effluvia, like odours, are diffusible; they have a certain force of diffusion, which Professor Graham has expressed numerically; and all the emanations from human habitations in the open country mingle, almost as soon as they escape, in the currents of the atmosphere. But locate, instead of one individual to a square mile of land (the supposed density of population in the uncultivated forests of America, and the stappes of Asia), 200,000 individuals upon a square mile, as soldiers in a camp, and the poison will be concentrated 200,000-fold; intersect the space in every direction by 10,000 high walls, which overhang the narrow streets, shut out the sunlight, and intercept the movements of the atmosphere; let the rejected vegetables, the offal of slaughtered animals, the filth produced in every way, decay in the houses and courts, or stagnate in the wet streets; bury the dead in the midst of the living; and the atmosphere will be an active poison, which will destroy, as it did in London formerly, and as it does in Constantinople now, 5·7 per cent. of the inhabitants annually, and generate, when the temperature is high, recurring plagues, in which a fourth of the entire population will perish. But the health will be little more impaired by residence upon one than 100 square miles, if means can be devised for supplying the 200,000 individuals with 200,000,000 cubic feet of pure air daily, and for removing the principal sources of poisonous exhalations. The latter object is partly accomplished by paved, even streets, by the scavenger, by an abundant supply of water, by large, well-constructed, trapped sewers, and by domestic habits of cleanliness; but it is difficult to perceive how volatile impurities can be removed, and how a stream of uncontaminated air can be supplied where the sun cannot heat the earth and air; where there are no open squares, or the streets are narrow, or the houses are only separated by courts, or built in *cul de sac*." (pp. 111-12)

It sounds somewhat paradoxical to say, that *that* civilization which increases the duration of life, should cause the population to be concentrated in large cities—a circumstance which abridges it. Such, however, seems to be the case, and we must therefore conclude, that among many elements of longevity civilization fosters one mighty element of destruction. Still, be it remembered, that Mr. Farr's very valuable facts are deduced from a comparison between rural districts and English towns, the growth of a progressive civilization, and that the older parts of them, now chiefly inhabited by the labouring poor, were formed in the earlier stages of such civilization. We should like much to see a comparison between the mortality of country districts and large towns brought rapidly into existence, under a civilization fully developed—those of the United States for instance.

We are compelled to close here our review of this very important volume. In our last Number we bore testimony, on our own authority, to its very great value, and to the credit justly due to the Registrar-General and Mr. Farr; and we appeal to the extracts in the present one for a confirmation of the truth of this testimony.

ART. III.

De Chemicis Calculorum Vesicariorum Rationibus. Scripsit E. A. SCHARLING, A.A., L.L., M. Chemiæ Lector.—*Hauniæ*, 1839. 4to, pp. 52.

On the Chemical Relations of Urinary Calculi. By E. A. SCHARLING, Lecturer on Chemistry, &c.—*Copenhagen*, 1839.

IT has happened to us, on several occasions, during the last few years, to find ourselves challenged by the merit of Inaugural Dissertations, both British and Foreign, to rank them among our *Books*, and to notice them as such. The work of M. Scharling belongs to this class; and claims our attention both for the importance of its subject and the ability with which it is treated.

In the account which the author gives of the labours of those who have preceded him, it is a matter of congratulation to an Englishman to find, that his own countrymen have taken by far the largest share in the chemical investigation of calculi. Some suspicion of the nature of urinary concretions appears to have been entertained as early as the fifteenth century, but it was Scheele who, in 1776, first examined these bodies in a scientific manner; although, according to him, they all contained the same principles. Bergmann improved upon the experiments of Scheele, and first announced the presence of oxalic acid in calculi, although, according to the author, we are to suppose that he produced it artificially, by acting on lithic acid with nitric acid. Scopoli and Bernoulli, finding also what they considered to be the acid of sugar (oxalic acid) in calculi, recommended that the use of saccharine, and other substances capable of producing that acid, should be avoided.

In 1786, Tychsen, professor of chemistry in Copenhagen, discovered in a calculus, given to him by Winslöw, phosphoric acid combined with lime; and in 1793, Fourcroy, after having confirmed some of the experiments of Scheele and Bergmann, announced the occasional presence of the phosphates of ammonia and soda (p. 3). In 1797, Wollaston proposed a classification of these bodies, founded on a series of his own experiments; and this, with slight modifications, has continued in use until the present time. The history of calculi may be considered to have been rendered very complete by the researches of Marcet, who added to Wollaston's varieties the xanthic oxide and the fibrinous calculi. It is unnecessary here to enumerate the various additions to the subject made by Brande, Henry, and Prout, in England, and Liebig and Wöhler in Germany, for these must be sufficiently known to the profession.

The author, after remarking that so long as the chemical characters of calculi remained unknown, it was hopeless to look for any good result from medical treatment, proceeds to speak of the physical and chemical properties of these concretions. In giving a brief outline of his observations, we shall only select those points which appear to us new, and a knowledge of which is likely to be of practical utility.

1. *Form.* Calculi are generally round or oblong; those which are of large size are often pyriform. Angular calculi are commonly small, and angularities are generally observed where *several* of these concre-

tions are contained in the bladder. Sometimes they have a tetrahedral or cubic form, specimens of which are figured in the author's plates.

2. *Size and specific gravity.* The size of calculi is subject to great variation. Thus they may weigh from a few grains to several pounds. Out of 126 specimens in the Royal Surgical Museum of Copenhagen, examined by the author, more than one half weighed between half an ounce and two ounces. In estimating the weight of calculi, attention is properly directed to a source of fallacy not commonly noticed by writers on this subject. Thus, if a calculus be weighed soon after removal, its weight will be much greater than after the lapse of a few months. This is what we might expect, from the large quantity of water which these bodies contain, and which slowly passes off by evaporation. This loss of weight even goes on, in some instances, for several years. A singular observation of Professor Jacobson's is referred to, as an illustration: A calculus examined on the 9th of July, 1832, weighed two ounces and twenty grains; on the 11th July, it had lost twenty grains; and, going on slowly decreasing, it had lost on the 12th August, 1833, *fifty-six grains.* The weights of calculi cannot, therefore, be compared, except in those cases where the time at which the weighing is performed is accurately recorded.

Fourcroy found the sp. gr. of calculi to vary from 1·213 to 1·976. The author considers this statement to be generally correct, but in one instance a calculus examined by him had a sp. gr. of 2·014. In estimating the sp. gr., it is necessary that the calculus should be deprived of the air which, from its porosity, it always contains, after it has been exposed. Thus the calculus must be placed for some hours under water, or in vacuo, or it may be boiled in water; the last plan is objectionable, since a portion may become thereby dissolved.

3. *Surface.* Our attention to this is so far important, that the actual constituents of the crust of calculus may be sometimes determined by an inspection of the surface. The angular prominences on the so-called mulberry calculus indicate oxalate of lime, but this calculus is sometimes smooth. The compound phosphate of ammonia and magnesia often forms large foliated crystals, and the subphosphate of lime has commonly a smooth chalky surface. The lithic acid variety has either a smooth surface, or, if it be rough and uneven, the prominences are obtuse and rounded, not acute, like those on the mulberry calculus. (p. 9.) Where more than one calculus is contained in the bladder, these superficial characters are modified or lost by friction. Deep fissures are often seen on calculi, sometimes filled up by septa of membranous matter, or by urinary deposits of a different nature. The chemical composition of an entire calculus can rarely be deduced from an examination of its surface, since the body may be made up of very different materials.

4. *Colour.* This is a point to be attended to in judging of the nature of calculi. Calculi are generally met with of three colours: a dull yellow or *brown,* indicative of lithic acid or the lithates; a *dull purple,* the oxalate of lime; and the third and most frequent *white,* which indicates the phosphates and carbonates. The colours are never so well seen as in the recent calculi; the fifty coloured specimens delineated in

the plates have been taken under these circumstances.* Calculi may present different colours on section. This will, of course, be the case where there is a difference of composition.

5. *Odour.* Calculi often retain an unpleasant odour, if they have not been washed after removal. This is due to the presence of decomposing animal matter locked up in the interstices, such as urine, mucus, &c. When rubbed or rasped, they give out the empyreumatic odour of bone, under the same circumstances. Fourcroy remarked that the mulberry calculus had the odour of spermaceti. This may have been accidental.

The structure of calculi is commonly lamellar, the different layers sometimes easily separating from each other; but the ammoniaco-magnesian phosphate variety seldom admits of this, owing to its softness and brittleness. To show the lamellæ, Berzelius recommends that the calculus should be polished by grinding it with water and the powder of calculi; but the author prefers the simple use of a brush with water. The lamellæ are generally arranged with considerable regularity around a central part, which acts as a nucleus. They are often of different composition, and appear to have been performed at various times and under different states of the system.

The nucleus is the most important part of the calculus, since this seems to be often the primary cause of the production of the calculus. ("Quæ sæpe prima causa calculi formati, æstimari potest.") Although the nucleus may have the same chemical composition as the other parts, it is generally denser. That the nucleus derives its origin from the kidneys the author thinks probable, from the fact that small renal calculi are frequently passed by those subject to calculous disease, before any symptoms of stone in the bladder appear; and also because such individuals often suffer from the most severe pains, when the calculi descends from the kidneys into the bladder. Sometimes substances mechanically introduced into the bladder from without are known to act as nuclei for the calculous deposit. Instances of this kind are to be found recorded by most surgical writers.

Occasionally two nuclei are found; two specimens of this variety are figured, and in some very rare cases more than two. An admirable drawing is given (fig. 38), in which there are four nuclei; this calculus was composed entirely of phosphate of lime. Sometimes there is no nucleus, but there is a space in the centre, filled by membrane, or lined by crystals; at other times the calculus is perfectly hollow, of which a specimen, in the possession of Professor Jacobson, is figured (fig. 5). This, probably like most others presenting this singular conformation, was not closed, but had fissures leading down to the centre from various parts of the surface. The nucleus on which the calculous matter had become deposited was perhaps originally soft, and had become, in the progress of time, removed through these fissures.

* We must here do the author the justice to say, that the drawings are the most correct, in regard to colour and form, which we have ever seen. Here and there, we meet with the objection which applies to coloured lithographs generally, namely, that the blackness of the ink tends to drown the tints.

Having thus given the author's account of the *physical* characters of calculi, we shall next proceed to examine his view of the *chemical* relations of these bodies. He enumerates twenty-two different substances which have, up to the present time, been discovered by chemists as constituting the varieties of urinary calculi. It is needless to mention these: many exist already formed in the urine, others are purely extraneous, as clay and the phosphate of iron. He himself has even found mica in one calculus; "but substances of this kind are to be regarded as of accidental occurrence, and not constituents of calculi. Many of the substances detected by chemists are not very soluble in water; it is easy to conceive that these may be readily deposited; but it is difficult to understand how very soluble salts should be separated in a crystalline form, unless by supposing that, like urea, which is occasionally found in small quantity in calculi, they proceed from that portion of urine which is closely locked up in the calculus at the time of extraction." (p. 13.)

The author's classification of calculi does not differ from that in common use:

1. *Lithic acid and the lithates.* More than three parts of all calculi are formed of this acid, either free or united to bases. In the free state, the acid is never pure, being mixed with more or less albumen or mucus and colouring matter. The chemical properties of these calculi are very accurately detailed: the action of nitric acid is properly relied on as the most satisfactory test. Nitric acid forms a colourless solution with lithic acid, leaving on evaporation a rich red residue, the colour of which immediately disappears on the addition of water, according to Berzelius; but the author did not find that this effect took place until after the lapse of several hours. His friend Jacobson adopts the following neat method in the application of the test: He evaporates cautiously in a watch-glass the nitric acid solution of lithic acid, until it has become inspissated; he then inverts this watch-glass over another, containing a few drops of ammonia, when the peculiar pinkish red colour, indicative of lithic acid, immediately appears. This is, no doubt, a red colouring matter generated simultaneously with purpuric acid. Instead of adopting this simple view of the changes, a sect of modern innovators have sought to complicate the subject, by assuming the production of such substances as alloxan, allaxantin, purabonic acid, and ammonia. This mystical trifling in science seems to have made no impression on our author, who, like a sober investigator, prefers plain facts and well-known names to crude hypotheses and a perpetual change of nomenclature.

Lithic acid may play a very important part in the animal economy: thus it has been lately discovered, that from it may be formed allantoin or allantoic acid—a principle detected by Vauquelin in the allantoic fluid of the cow.

The alkaline lithates are successively examined as constituents of calculi, and the means of identifying them are well described. The lithate of ammonia is known from those of potash and soda by the fact that it is entirely decomposed and dissipated by heat, while the two latter substances leave a residue of their respective carbonates. The

author has met with two compounds of lithic acid and ammonia; that containing the larger proportion of acid abounding in the excrement of many varieties of birds. Lithic acid forms a neutral and an acid compound with lime, the latter being separated as a bilithate, on adding muriatic acid to the neutral combination.

According to the author's experience, calculi consisting chiefly of lithate of ammonia are rare. There was only one specimen in the whole collection referred to; this was taken from a young girl, and the ammoniacal lithate, when it occurs, is chiefly found in young subjects. The analysis of this calculus is very simple; but we do not see the necessity of pursuing the plan recommended by the author, namely, to separate the lithic acid by muriatic acid, before testing for ammonia. The mere heating of the powdered calculus with a small quantity of solution of potash is sufficient.

The lithate of lime sometimes accompanies the phosphate or the oxalate of that base. We must refer to the treatise for the means adopted to separate the lithate from these salts; the processes are simple and satisfactory. The lithate of magnesia is rare, but three specimens are referred to, two of which have already been described, as having the singular forms of a cube and a tetrahedron. As the analysis of this calculus is not given in works on chemistry, we here subjoin the author's description, premising that he has found this lithate more soluble in water than that of lime.

"A portion of the calculus is boiled in water, the solution filtered, and muriatic acid added, on cooling, to the filtered liquid. This separates the lithic acid, which may be obtained on a filter; and now, by boiling the clear liquid with potash, a white precipitate falls down, which is proved to be magnesia, by the facility with which it dissolves in sulphuric acid, as also by its dissolving, although more slowly, in muriate of ammonia." (p. 18.)

2. *Xanthic oxide.* We look in vain, in most modern works on calculi, for a satisfactory account of this variety, which, nevertheless, was discovered a long time since by Dr. Marcet. Some writers have even been disposed to doubt the existence of such a calculus, as a distinct species. Wöhler and Liebig have lately confirmed the researches of Marcet and Stromeyer, on its chemical constitution. This body is composed of the same elements, in the same proportions, as lithic acid, with the exception of its containing one equivalent less of oxygen. A better name has, therefore, been proposed for it, viz., *lithic oxide*, instead of that it now bears, and which is derived from its colour. The following are its principal characters:

"Its surface is smooth, and of a brownish colour, but in some parts earthy and white. It has a reddish brown fracture. It is lamellated in a concentric form, the lamellæ being easily separable from each other, without presenting any fibrous or crystalline union. There is no nucleus distinguishable from the rest of the mass. Its hardness is about equal to that of some of the more compact specimens of lithic acid calculi. It acquires by friction a waxy lustre. It is dissolved by warm nitric acid, without the evolution of any gas; and on evaporation, it leaves a lemon-coloured residue. It never produces with nitric acid the pink-coloured matter which is generated under the same circumstances with lithic acid." (p. 19.)

The last statement is opposed to the results of Marcet; but the author

thinks that in the calculi examined by that excellent chemist there may have been some traces of lithic acid present. Again:

"To render the examination more complete, a portion was dissolved in caustic alkali; the solution thus obtained had a greenish yellow colour, resembling that of bile. It was somewhat opaque, and very difficult to filter. Carbonic acid was passed into this solution, until the alkali was converted to bicarbonate, by which means the xanthic oxide was entirely precipitated under the form of a white powder. After washing, the precipitate did not retain the smallest trace of alkali, in which respect it differs from lithic acid; for this body, thus treated, is never precipitated in a pure state, but always as an alkaline lithate, while another portion of acid is retained in the alkaline solution.

"The xanthic oxide is more soluble in caustic ammonia than lithic acid. On evaporation, it leaves a yellowish foliaceous residue, retaining traces of ammonia. The nature of the yellow mass obtained on evaporating the nitric acid solution of this substance is not known. Xanthic oxide is dissolved by strong sulphuric acid, and water does not precipitate it from this acid solution, in which respect it again differs from lithic acid. It is not soluble in muriatic and oxalic acid, in which, among other properties, it differs from *cystine*. When distilled in the dry way, it resembles lithic acid, in yielding a large quantity of hydrocyanic acid. An empyreumatic matter at the same time results, having a peculiar odour, resembling that of burnt horn. When sublimed, carbonate of ammonia but no urea is generated." (p. 20.)

Such is the description given by the author, by which, we think, professional men will in future be easily able to identify this species of calculus. Its rarity may, however, be inferred from the fact that there was not a single example of it among the museum specimens which he examined.

3. *Ammoniaco-magnesian phosphate.* This calculus was first discovered by Wollaston in the human bladder. Its white crystalline structure and spongy character at once indicate it. Potash expels the ammonia, precipitates the magnesia, and forms an alkaline phosphate. By calcination, ammonia is volatilized, and phosphate of magnesia remains. The test recommended for the fixed base is calcining it when moistened with the nitrate of cobalt, which brings out a rose-red colour; but there are other and better ways of proving the presence of magnesia. This calculus is easily soluble in dilute sulphuric acid, by which it is known from the phosphate of lime; the muriatic and acetic acids dissolve it. The muriatic solution, evaporated to dryness and still further heated, yields muriate of ammonia.

4. *Subphosphate of lime mixed with the compound phosphate of ammonia and magnesia.* This is the most frequent variety of calculus, after lithic acid. The author, in admitting that we are indebted to Marcet, Tennant, and Wollaston, for a knowledge of its chemical properties, says that its composition was known to his countryman, Tychsen, in 1786, eleven years before Wollaston's essay appeared. This calculus is commonly called the fusible variety, since, from its very compound nature, it easily fuses when heated. Its characters will be found detailed in most chemical works. The compound phosphate is sometimes mixed with the carbonate of lime; acetic acid affords a ready means of separating the constituents of this calculus, the subphosphate of lime remaining undissolved. (p. 22.)

5. *Phosphate of lime.* This is known from the subphosphate by the

fact that, when heated to a high temperature, it fuses into a pearly mass. The author gives a quantitative analysis of one of these calculi, to show that it was neutral in composition; but we are not satisfied that his method would effectually separate all the lime. He reserves the process for determining the presence of phosphoric acid, until he speaks of the means of separating the mixed phosphates in a subsequent part of the essay. As this process is omitted in many works treating of the analysis of calculi, we here subjoin it, recommending dilute nitric acid to be used as the solvent, in preference to the concentrated acid employed by the author: "Dissolve a portion of the phosphate in dilute nitric acid; add a few drops of nitrate of silver, and then cautiously neutralize the excess of nitric acid by a weak solution of ammonia; phosphoric acid is known to be present by the immediate production of the yellow subphosphate of silver." (p. 35.)

6. *Oxalate of lime.* This salt forms the hardest calculi; their peculiar form serves to distinguish them: they are generally mixed with organic and other substances. In one, quantitatively analyzed by the author, organic matter and water formed thirty per cent. of its weight, and it contained besides about six per cent. of phosphate of lime. The author relates the method of detecting the base, but he omits to mention how the experimentalist may discover the acid. There are several processes; but one, which we have not seen noticed in books, is, to dissolve a portion of the calculus in dilute nitric acid; add nitrate of silver, and then gradually neutralize with ammonia: the white oxalate of silver is thrown down, known by its unchangeableness on boiling, and by its detonation at a moderate heat. If the oxalate be mixed with phosphate, the usual plan may be followed, of boiling the calculus with an alkaline carbonate.

The oxalate of lime calculus is sometimes met with in a perfectly crystalline state. Marcet saw this in three cases, and in another instance Wollaston observed that the crystals had an octohedral form.

7. *Carbonate of lime.* This is rarely found as a calculus in the human subject, although not unfrequently in animals. Its analysis is very simple, but the author adverts to a possible fallacy, which may mislead the experimentalist. In adding a dilute acid to any calculus, bubbles of air are often observed to escape; this must not be mistaken for carbonic acid; it is nothing more than the evolution of air which adheres to the surface of all porous solids. The presence of a sulphuret will lead to effervescence on the addition of an acid, but this is easily known by a salt of lead. The author has observed the escape of sulphuretted hydrogen from the urinary concretions of the ox during analysis. Carbonate of lime forms the principal mass of calculi found in animals.

Traces of carbonate of magnesia have been occasionally found in urinary calculi.

The description of cystic oxide or cystine is borrowed from Wollaston, through Berzelius. The occasional presence of silica, oxide of iron, and oxalate, and benzoate of ammonia in calculi, is next noticed; but no light is thrown upon the manner in which the first two of these substances become mixed with the more common constituents.

In a pathological point of view, it is a matter of great importance to determine which of the varieties of calculi are of the most frequent occurrence. Tables of this kind have been drawn up, and are attached to the collections of most museums; but it is probable, as M. Scharling remarks, that many of these are erroneous, from the circumstance of the outer crust only having been chemically examined. (p. 30.) Sometimes it is wished that the form of the stone should not be impaired by section, in which case it is obviously absurd to place it in any class, from the chemical composition of its crust. We cannot but admit the correctness of this view, when we reflect on what has been already said respecting the structure of calculi. We fully coincide with the author in the opinion that every calculus should be classified according to the chemical composition of its *principal mass*. In a table of 155 calculi examined by him, he has given the composition, in one column, of the nucleus, in a second, of the principal mass, and in a third, of the outer crust or cortex. If such a rule as this were followed by future observers, different collections might allow of instructive comparisons; whereas now it is often a matter of much uncertainty to compare them. The results of M. Scharling's observations are as follows: "In 94, the great bulk of the calculus was made up of lithic acid, or about sixty per cent. In 23, oxalate of lime, or fifteen per cent. In 25, the mass was composed of the phosphates, or about sixteen per cent." (p. 31.) It is worthy of remark, that in 73, the outer crust was formed of the phosphates; so that, had the examination been confined to this part only, the phosphatic calculi would have amounted to forty-seven per cent., instead of sixteen per cent. It is necessary to observe, that many of the lithic acid and oxalate of lime calculi are really invested by a crust of the phosphates: this is especially the case in the larger varieties of all calculi. The proportions contained in other museums, especially those of England, are then referred to; they do not in general materially differ from those obtained by the author.

The next part of the treatise contains matter deeply interesting to those who wish to engage in chemical researches on the properties of these bodies. It is customary, in most treatises, to give the chemical characters of the varieties of calculi individually, by which the student is led to suppose that the analysis is a very simple operation; but a great practical difficulty meets him in his first attempts, in the fact that the varieties are commonly blended together; hence the chemical reactions peculiar to one substance are obscured by the presence of another. When a calculus is cut through, portions of the different lamellæ should be separately examined; and when these do not exist, when we have only a confused mass for analysis, it will be necessary to pursue a certain series of experiments, if we wish to spare ourselves much labour, and obtain a satisfactory result. M. Scharling's is the only treatise which we have yet seen, wherein these rules are clearly and succinctly described. For an account of these, as well as for the best means of separating from each other all the usual mixtures of calculi, a subject so embarrassing to a beginner, we must refer to the work itself. The rules are evidently derived from long experience, and accurate observation.

There are some sound remarks on the deposition and increase of calculous matter in the bladder. Foreign substances accidentally introduced and left in the organ, are often observed to become incrusted with the phosphates. This has been explained by supposing that, in such cases, the urine abounded in those saline matters, and that there must have existed a constitutional tendency to calculous disease. The author, however, considers, and with great probability, that the change in the chemical constitution of the urine may really have been due to the irritation produced by the presence of a foreign body in the organ; an opinion borne out by the fact, that such individuals had never manifested any symptoms of calculus previously to the foreign substance becoming lodged in the bladder. The great difficulty is, to explain the cause of the formation of renal calculi; for these are often the nuclei of the deposits formed in the bladder. So again, the separation of a small quantity of fibrin or mucus may become a nucleus, around which the urinary salts are more or less rapidly deposited. The increase of the stone may depend greatly, in the author's view, on the state of health of the individual; and the thickness of the lamellæ may serve as a criterion of the vicissitudes of health and disease in a person so affected, the thickest lamellæ being deposited while the individual is in a bad state of health.

A very curious part of the enquiry relates to the singular alternations in the chemical characters of the deposits: the nucleus, the body, and the cortex being perhaps different from each other. It is often observed that this transition is not abrupt, but gradual, indicating slow and secret changes in the constitution, by diet, medicine, or variable state of health. Thus M. Scharling has remarked, in several mulberry calculi, the transition from lithic acid to oxalate of lime to take place through a deposit of the lithate of lime. (p. 41.) The effect of diet in calculous disorders deserves to be more deeply studied than it has hitherto been; we should probably hereby discover the source of many of these singular chemical changes. When in one part of a calculus the lamellated character abruptly ceases, it is probable, as Crosse imagines, that the calculus may have become adherent by this part to the parietes of the bladder. On the whole, the increase in the size of a calculus bears much analogy to the slow growth of crystals placed in saline solutions.

On chemical solvents. Since the composition of calculi has been known, various remedies of a chemical nature have been recommended. The alkalies and their carbonates have been employed to neutralize the lithic acid diathesis; but the author thinks they may be also efficaciously employed against the oxalate of lime and the phosphates. We agree with him, that an alkaline carbonate can decompose the oxalate of lime; but we should hardly have supposed, although he gives it as the result of his own observation, that the natural heat of the body would have sufficed for this decomposition. We have found that a heat of 212° was required for some minutes, to bring about a partial interchange of acids and bases; for under no circumstances is the decomposition complete. Thus, then, in the most favorable case, we substitute a carbonate of lime for an oxalate of lime calculus; but he thinks the carbonate is susceptible of removal, by the subsequent use of carbonated water. In

his opinion, the alkaline carbonates will easily decompose the phosphates, especially the basic and neutral phosphates of lime. The alkali should be used in the state of bicarbonate. (p. 43.)

It is difficult to explain how it is that the carbonates of lime and magnesia have had a beneficial effect in calculous disorders, where those of potash and soda have failed. The author thinks, with Wöhler, that any vegetable neutral salt of potash or soda may be substituted for the carbonates, since in the animal body those salts are actually converted to carbonates. The mineral biborate of soda (borax), which is a more powerful solvent of lithic acid than the carbonate, has been used in calculous affections; but this does not become transformed into carbonate like the vegetable salts. The acetate of soda was tried, by M. Scharling, as a solvent for lithic acid, but he found it to have less power than the biborate; yet, when lithic acid was mixed with the basic phosphate of lime, the solvent power of that salt was greater than that of borax, so far as the calcareous phosphate was concerned. (p. 44.) Either of these salts should be exhibited in the form of injection. Acetate of potash resembles the acetate of soda, but the acetate of magnesia has no more power than the carbonate of that alkaline earth. Acids, both mineral and vegetable, have been used with variable effects; the latter have been preferred.

M. Scharling answers the objections which have been made to the use of chemical antidotes; and he thinks he has had good physical evidence of the solvent action of these remedies on calculi which he has examined. The surface has appeared cellular, as if worn away or dissolved. The figures of five calculi are referred to by him, in confirmation of this view, although perhaps other explanations might be brought forward to account for this cellular state of the surface. On the whole, there is much difference of opinion on the subject of chemical remedies; but we agree with the author, that those who advocate their use have far greater reason on their side than those who entirely reject them. He recommends the remedies to be employed by injection, taking care that the solutions are not so strong as to excite irritation of the bladder; but he very judiciously adds, these are only remedies for the *local* mischief: to remove the disease itself, the system must be put under active medical treatment.

We thus complete our review of M. Scharling's essay, which, although published in an unpretending form, contains more useful and practical information on the subject of which it treats than many voluminous treatises. We could have wished that it had appeared in any other language than Latin, since this is but ill adapted for the diffusion of scientific knowledge in modern times; yet the author has perhaps taken the best means he could adopt, his own tongue, the Danish, being but little understood by professional men. As a literary composition, it may, perhaps, be destitute of what is called classical polish, but it possesses qualities which have far greater value in our eyes, namely, clearness and simplicity.

Art. IV.

Guy's Hospital Reports. Nos. VIII.-IX. *April and October*, 1839.—*London.* 8vo, pp. 448.

The successive numbers of this publication keep up the character of general excellence, which we have more than once had the pleasure of assigning to it. On the present occasion, as formerly, we shall best evince our sense of its value by giving a detailed account of its principal contents.

No. VIII.

1. *On the Disorders of the Brain connected with Diseased Kidneys.* By Dr. Addison.—The object of this communication is, according to its author, threefold: "First, to point out the general character and individual forms of cerebral disorder connected with interrupted function of the kidneys, from whatever cause such interruption may arise. Secondly, to show that, in recent as well as in chronic disease of the kidney, the cerebral disease is not unfrequently the most prominent, and occasionally the only obvious symptom present. And, thirdly, to establish a means of diagnosis, in such obscure or in unsuspected cases, upon the peculiar character of the cerebral affection."

It seems hardly fair to deal harshly with an author who assures us that the details of his production are imperfect, and his aim rather to excite enquiry, than to promulgate truths, which he presumes he has established: nevertheless we cannot forbear asking Dr. Addison, why—with the consciousness that his essay is deficient, deficient in the most essential particular, proofs of the novel dogma it advances,—he has shown himself so anxious to lay it before the public? Why present the profession with "imaginings" and "beliefs," instead of patiently waiting, until he could substantiate these by the evidence of closely observed and faithfully related cases? Dr. Addison, however, was at liberty to choose his own method of proceeding; let us enquire into its results.

The general character of cerebral affections connected with renal disease is believed by this writer to be "marked by a pale face, a quiet pulse, a contracted or undilated and obedient pupil, and the absence of paralysis;" and the individual forms of this cerebral disorder are stated to be the five following: "1. A more or less sudden attack of *quiet stupor;* which may be temporary and repeated, or permanent, ending in death. 2. A sudden attack of a *peculiar modification of coma and stertor;* which may be temporary or end in death. 3. A sudden attack of *convulsions;* which may be temporary or end in death. 4. *A combination of the two latter;* consisting of a sudden attack of coma and stertor, accompanied by constant or intermitting convulsions. 5. A state of *dulness of intellect, sluggishness of manner and drowsiness,* often preceded by *giddiness, dimness of sight, and pain in the head,* proceeding either to *coma* alone, or to *coma accompanied by convulsions;* the coma presenting the peculiar characters already alluded to."

We really can perceive nothing novel in this enumeration, except the style of classification adopted: there is not a single statement of the least importance in Dr. Addison's account of these five cerebral conditions, of which the substance may not be found clearly and explicitly

laid down in Dr. Christison's recent treatise on Bright's disease. Of these forms of cerebral disorder, with which all experienced observers are perfectly well acquainted, we expected no illustrative cases at the hands of our author; but we do require him to bring forward a good supply to demonstrate to us what he asserts, and what the essay putatively "*shows*" to be the fact, that cerebral symptoms will sometimes lead to the diagnosis of renal disease, when every ordinary symptom of such affection is absent. The practical bearing of this proposition is manifest and important; but almost the sole evidence adduced in its support, consists of the following loose statement, for which Dr. Addison seems to have trusted to his memory alone: "the most exquisite example I ever saw of the state of quiet torpor occurred in a man who at the time presented no dropsical symptom, whose urine was not albuminous, and who made no complaint of pain or uneasiness in his loins." The man's perception of pain was of course blunted by the state of his cerebral functions, but was no suffering evinced under forcible and long-continued pressure in the renal regions? What were the physical and chemical characters of the urine, as detected with and without the aid of the microscope? Was there vomiting or not? And above all, admitting that an enquiry into these particulars is liable to be neglected in the hurry of practice, and that a peculiar state of the brain would be likely to arouse the attention of even a very careless practitioner, in what proportion of cases does Dr. Addison affirm that the ordinary symptoms of nephritis are supplanted or masked by this disturbance of the brain, for upon this ultimately depends the bedside utility of the alleged discovery? If, as we strongly suspect, such cases are of most extraordinarily rare occurrence, (we doubt if they can *ever* occur in the practice of the philosophic observer, who studiously examines the condition of every organ, whether presumed to be diseased or not,) there would be no little temerity in grounding a diagnosis on what must be termed a bare possibility. Be this as it may, "the cortical part of the kidneys was found highly injected, of a deep red or almost chocolate colour, and somewhat softened in its texture," in the subject in question.

This observer purposely omits to notice the morbid changes discovered in the brain after death, because "they are well known to be very often, in appearance at least, extremely slight." We should have considered the prevalence of this opinion respecting the unaltered condition of the cerebral tissue a strong motive for adopting an opposite line of conduct: if the general impression be a wrong one it should be corrected; if the contrary, the greater the mass of evidence of its justness we have the better, for if there be a science in existence, in which negative facts occasionally acquire the full weight and importance of positive ones, that science is pathology.

II. *On Perforations of the Stomach from Poisoning and Disease.* By Mr. Alfred S. Taylor.—Mr. Taylor here endeavours to establish the distinctive signs of perforation of the stomach, when caused by chronic disease of its coats, and when resulting from the action of poisons, with greater precision than had hitherto been attained. And his efforts have been far from unsuccessful; with but little really novel

matter to assist him, he has, by his excellent arrangement of previously ascertained facts, his close investigation and shrewd inferences from their details, managed to place the whole subject in a clearer point of view than any of his predecessors. The nature of the essay, however, founded as it is for the most part on materials with which the reader of modern works on forensic medicine is familiar, will prevent us from analyzing it at any length: a sketch of its plan, and an occasional notice of some of the arguments with which it is interspersed, will, we are persuaded, give the reader a desire to become acquainted with the original.

Mr. Taylor enumerates four cases in legal medicine, wherein a knowledge of this subject will be required of a medical practitioner: "1. A person may have died from perforation of the stomach through disease and not from poison. 2. A person labouring under the disease may be the subject of poison. 3. A person labouring under the disease may have received blows or injuries on the abdomen; in which case it will be necessary to state whether the perforation did or did not result from the violence used. 4. Perforation of the stomach from post-mortem changes may be mistaken for perforation from poison." The author opens his investigation of these cases by successively considering the peculiarities of perforation caused by *poisons*, by *morbid causes*, and by *post-mortem changes*.

Perforation of the stomach from natural causes, that is, from ulceration, simple or scirrhous, and from solution of the parietes of the organ, involves no few points yet open to debate. These, however, rather belong to perforation from the latter cause, than to that originating in ulceration. Indeed, the characters of this insidious and most formidable morbid process are so well defined in the vast majority of cases, both by the almost total absence of important symptoms, until perforation is actually accomplished, and by the anatomical peculiarities of the opening, that there has been very little scope for variance in the descriptions of authors. From the cases collected by Mr. Taylor, the following conclusions clearly derive: Ulcerative perforation seems most frequently to occur in young females from eighteen to twenty-three years of age, but has been observed in subjects of advanced age and of both sexes. The preceding illness rarely amounts to more than simple dyspepsia, or slight irritation of the stomach after eating, with want of appetite. These ailments may have existed for weeks or months, without exciting much attention on the part of the patient, when she is seized with excruciating pain in the abdomen, (generally, but not always, soon after a meal,) in the majority of cases accompanied with pain between the shoulders. Vomiting commonly ensues, but not invariably; there is no diarrhœa; on the contrary, obstinate constipation is usually observed: with the progress of the disorder the symptoms become distinctly those of peritonitis, death supervening in from eighteen to thirty-six hours. Mr. Taylor's cases are defectively reported in respect of the condition of the pulse in the early stage of the abdominal pain; if the statement of Mr. Travers (*Med. Chir. Trans.* vol. viii.), to the effect that its frequency remains natural for some time, until the symptoms merge in those of peritonitis, be correct, the diagnostic importance of the state of arterial pulsation needs not to be insisted on.

It will be readily admitted that there is quite enough in the character and course of the symptoms enumerated to excite the suspicion of poisoning, which almost always arises in the minds of non-professional persons, when death occurs under circumstances of the kind. The previous good health of the patient, the seat of the pain, its appearance soon after eating, and the suddenness of dissolution form a strong chain of evidence against the arguments of the practitioner, who is shrewd enough to suspect the true nature of the case. The post-mortem appearances—a characteristic aperture in the stomach, the presence of the contents of that organ in the cavity of the peritoneum, and the marks of recent peritonitis—will convert his suspicion into certainty. The aperture is commonly of an oval or rounded form, from half an inch to an inch in diameter, and almost invariably situated in or near the lesser curvature between the cardia and pylorus. The characters of the margin vary, according as the ulceration is scirrhous or simple. In the former case the edges are smooth and fleshy-looking; the tunics of the organ are bevelled off from within outwards; there is no appearance of laceration in the peritoneal coat; near the circumference of the aperture the tissues of the stomach are thickened and indurated, the thickened part forming a narrow ring around it, or extending to some distance over the surface of the organ. In cases of simple ulcerative perforation there is thickening, and marks of ulceration are apparent; the edge of the opening may be very slightly or not at all raised above the surface of the surrounding membrane. Such is Mr. Taylor's description, from actual observation, of this very important lesion; his experience closely agrees with that of other observers.

It is difficult to understand how it happens that marks of laceration are not usually found in the perforated peritoneum: Mr. Taylor's belief that this membrane is reduced by slow absorption to the thinnest possible stratum, before the opening is actually formed, in nowise removes the difficulty; for, admitting the conjecture to be just, it would merely explain excessive delicacy, but certainly not absence of the fringed detritus, which *à priori* views would lead us to look for. The resistance to the ulcerative process offered in some instances by the peritoneum is well illustrated by a remarkable and, we believe, unique case recently published by Mr. Crowfoot. (*Lancet*, January, 1839.) The patient died in thirty hours after the supervention of well-marked symptoms of perforated stomach: six months previously she had had an attack of profuse hematemesis, and in the interim been subject to "stomach attacks." The mucous membrane of the stomach was found healthy; the perforation, about the size of a sixpence, had only traversed the muscular and mucous coats, and led to a cavity almost equal to the stomach in size, lined with a pseudo-mucous membrane, and bounded by the peritoneal and muscular coats of the organ. Several pin-hole openings were discovered in the peritoneal portion of this pouch; through these the effusion of the contents of the stomach had taken place. Now it is exceedingly probable that, at the period when the patient was seized with hematemesis, ulceration through the two internal tunics took place, and that the formation of the cavity between these and the external coat then commenced. The case curiously corroborates the observations made by Professor Sebastian, of Groningen, on a similar pouch-like dilatation

of the investing peritoneum consequent on perforation of the intestine. (*Br. and For. Med. Rev.*, Vol. VII. p. 248.)

Perforation by corrosive poisons, such as sulphuric acid, is certainly not likely to be confounded with the lesion just described; we refer to Mr. Taylor's pages for their distinctive characters. But the case may be different, when the destruction of the walls of the stomach proceeds from the action of certain irritants, such as arsenious acid and the chloride of barium: fortunately, these substances very rarely induce perforation. In making our diagnosis, when doubt arises, we must be guided by the relative frequency of the two kinds of perforation, the period at which the symptoms appear, the character and degree of suddenness of the pain, the absence or presence of vomiting and diarrhœa, the time at which death supervenes, the anatomical state of the peritoneum and of the mucous coat of the alimentary passages, the appearance of the aperture, the results of analysis of the contents of the stomach, and of the matters, if any, vomited during life. In considering these various points, Mr. Taylor most ingeniously avails himself of his intimate acquaintance with the phenomena of arsenical poisoning: he is occasionally speculative, but justifiably so, for in truth the extremely limited number of cases of total destruction of the coats of the stomach by arsenious acid on record (there are only three well-authenticated instances of such an occurrence), makes it impossible to draw decisive inferences in every instance from past experience; and legal medicine is a *science of action.*

There is one statement in this part of the essay with which we cannot agree: Mr. Taylor observes, "when the deceased has taken food alone, chemical evidence cannot be dispensed with, but when in company with others, the simple fact of no other but the deceased having afterwards suffered will go to disprove the fact of poisoning." It is manifest that it will do no such thing, for one of the party may have been the murderer, and will have taken care either to feign eating, if the whole dish be poisoned, or simply to supply the plate of his victim with the poison. And, again, we affirm that there is no possible combination of circumstances, where a suspicion of poisoning has, on any grounds whatsoever, arisen, in which it is not imperative on the practitioner to take measures for the thorough analysis of the products of vomiting, and of the contents of the alimentary canal.

It frequently becomes a matter of serious moment, medico-legally speaking, to determine whether acts of volition and locomotion are possible after the occurrence of certain spontaneous traumatic lesions. In the instance of perforation we can conceive such a question being raised, and Mr. Taylor answers it by anticipation in the affirmative, on the evidence of a case in which it was clearly ascertained by the attendant surgeon that the patient walked to her home, a distance of a quarter of a mile, after the first alarming symptoms had manifested themselves. Disturbance of the intellect is not mentioned among the symptoms in any case, and in some the cerebral functions appear to have maintained their integrity to the last.

We have briefly alluded to the trifling derangement of the digestive functions, which ordinarily forms the only symptomatic evidence, that the process of ulceration is going on in the stomach; but it would even appear, judging from an extraordinary case related by Dr. J. Beck, that

actual perforation may take place, unannounced either by pain or any of its usual symptoms, unless the contents of the organ be effused into the peritoneum. The subject of this case, a female, aged sixty, is said to have died evidently from disease of the chest, and never to have complained of uneasiness in the stomach: the mucous membrane of the latter viscus was found in a highly vascular condition, and there were four ulcers, besides the aperture, along the lesser curvature; the aperture was about the size of a shilling. The stomach contained about a pint of fluid; none of its contents had escaped into the peritoneal cavity.

Mr. Taylor's disquisition on perforation of the stomach by solution is sagaciously sustained, and shows him to be possessed of accurate pathological knowledge and considerable talent for close reasoning. Some of the opinions current among the profession on this subject are shown to be of very questionable correctness, and the experiments of Müller and Schwann, with pieces of dried stomach immersed in artificial acidulous mixtures sensibly rejected, as incapable of furnishing any just inference on doubtful points in the history of perforation by the gastric juice. We cannot follow our author through this section of his essay, but subjoin his conclusions.

"1. Perforations of the stomach from solution are very rare in the human subject. 2. They may occur in healthy and diseased states of that body. 3. The perforation takes place after death, and depends on the action of the gastric juice, which, in the opinion of some, is facilitated by a diseased state of the parietes of the stomach. But that the secretion is the chief, if not the sole cause, seems probable, from the fact that the liver, spleen, and diaphragm have also been found softened, the latter even perforated where lying near the aperture in the stomach. 4. The secretion cannot be the healthy gastric-juice, but some altered state of that liquid. This is rendered probable by the facts—*a.* if it were the ordinary secretion, perforation would be much more common in healthy persons, dying suddenly, soon after a meal; *b.* it would not be met with in diseased subjects, or those labouring under disease of the stomach. 5. The exact nature of the liquid producing the change, and the circumstances to which it owes its solvent power, are unknown."

Mr. Taylor having thus laid his foundation for an examination of the medico-legal questions, which we have already transcribed, proceeds to illustrate these by reference to cases. Death from ulcerative perforation is exemplified by the case of a young woman observed last year by Mr. Hilton and himself; the description of the stomach is exceedingly well done, and the form and situation of the aperture shown in an engraving.

A person labouring under the disease may, we have said, be the subject of poisoning: of this, which may be regarded as a rare coincidence, there are two recent examples on record. The principal question here is whether the poison caused the symptoms and death; the fact of its having been swallowed is not supposed to be disputed. The first case referred to is that, related by Wildberg, of a woman, who swallowed by mistake half an ounce of hydrochlorate of baryta, and died in convulsions two hours after; the second that of a man to whom arsenic was administered with his dinner by his wife. In the former instance perforation, with the majority of the characters assigned to the species resulting from chronic disease, existed in the stomach; but so imperfect is the report of the case, that the state of the peritoneum even is not noted.

It is therefore impossible to affirm positively that perforation was produced by disease, but, from the nature of the symptoms, no doubt could be entertained of death having been caused by the poison. In the arsenical case, a scirrhous ulcer, evidently of long standing, was discovered in the stomach near the pylorus, but there was no perforation: it was contended by the prisoner's counsel on the trial, which took place about two years since on the Western Circuit, that the symptoms and their fatal termination were owing to this chronic disease and not to poisoning. But the course of the symptoms, as well as the lesions detected in the stomach, clearly announced the action of some powerful irritant, and this, combined with the fact that death was very unlikely to have occurred from mere ulceration of the stomach without perforation, decided the medical witnesses in giving it as their opinion, that life had been destroyed by the arsenic swallowed.

The third question, respecting the influence of external violence on perforation, receives very imperfect illustration from a wretchedly reported case by Mr. Watson.

Perforation of the stomach from post-mortem changes may be mistaken for that produced by poison, when symptoms, resembling those of irritant poisoning have been observed during life, though no poison has really been administered. Such a combination of circumstances must be singularly unusual. "This, however," observes Mr. Taylor, "signifies little in a legal point of view, for persons charged with crimes are frequently acquitted on the barest medical possibilities." The notorious case of Miss Burns, for whose supposed murder a Mr. Angus was tried a good many years since at Liverpool, is adverted to, in proof of such a difficult point as that just stated being occasionally submitted to the judgment of the medical witness. The case is, however, not examined at length, and for this very excellent reason, that the reports of the post-mortem appearances are imperfect.

We recommend this excellent paper to the notice of all our readers.

III. *On the Diurnal Variations of the Pulse.* By Dr. W. A. Guy.—This is a continuation of the author's previous enquiries, and is marked by the same clear and philosophical views. The inferences drawn by Dr. Guy, respecting the diurnal changes in the frequency of arterial pulsation, from a series of experiments conducted on his own person, are no doubt applicable to the future healthy action of his own pulse. But it may be questioned (as the experimentalist himself admits), to what extent these observations, made on a single individual, allow of general application. Dr. Guy urges, in support of their catholic pretensions, that he enjoys most excellent health, and has never suffered from any disease affecting the organs of circulation; that his pulse beats with, as nearly as possible, the same average frequency, as that of such healthy male adults as he has had an opportunity of comparing it with, and is affected to a similar extent by change of posture as theirs. Whether these arguments be convincing or not, no one will, we imagine, be captious enough to deny that the results of Dr. Guy's patient and carefully performed experiments are worthy of incomparably greater confidence than the vague impressions on the subject with which medical men have

been hitherto satisfied. For our parts, we declare our determination to abide by the following propositions, until a series of equally accurate experiments on a more extended scale shall have shown that they do not in all cases hold good.

"1. The pulse of a healthy male adult in a state of rest, unexcited either by food or exercise, is most frequent in the morning, and gradually diminishes as the day advances. 2. The pulse diminishes in frequency more rapidly in the evening than in the morning. 3. The diminution of the frequency of the pulse is more regular and progressive in the evening than in the morning. 4. The effect of food is greater and more lasting in the morning than in the evening; and in some instances the same food which in the morning produces an effect considerable, both in amount and in duration, has no effect whatever in the evening."

IV. *Observations on Poisoning by the Vapours of Burning Charcoal and Coals.* By Dr. GOLDING BIRD.—If Dr. Bird's essay can lay claim to no other merit, it at least possesses that of seasonableness: the recent exposure of ignorance respecting the most obvious and invariable morbid appearances after death from the inhalation of charcoal fumes, certainly rendered the want of some clearer, fuller, and more methodized account of this description of poisoning, than those to be found in existing works, painfully evident. Dr. Bird has, accordingly, if we may be permitted to be poetical, "seized fleet occasion by the hair," and shown himself particularly anxious to improve the state of his brethren's knowledge with the least possible outlay of time. But it would perhaps have been as well had Dr. Bird recollected that the race is not always to the swift; at least it can hardly be doubted that if he had extended his researches in periodical works, and allowed himself time for the performance of a greater number, and especially of more important experiments, than those he has engaged in, he would have produced a paper, not only more creditable to himself, but one more likely to put an early period to the perplexities of medical witnesses, than the compilation we are about to notice.

It has been remarked by Sachs, as we learn from Dr. Bird, that "what we most accurately know of carbonaceous vapour is the fact of its deleterious nature; but what this really is, or which is the poisonous ingredient, we really do not know." This if not a cheering is certainly a correct estimate of our knowledge on the subject. Let us consider for a moment the number and contradictory character of opinions held on the nature of the toxic principles of charcoal fumes, and the truth of this will become apparent. The mass of practitioners assuming that, what, they fancy, *ought to be*, actually *is*, believe carbonic acid to be the sole destructive principle evolved: but it is alleged that deleterious effects have been produced by charcoal stoves, when the proportion of carbonic acid generated must have been so trifling as to be incapable of accounting for those effects; besides the fallacy of this notion has, it is said, been still more conclusively proved by the fact, that after every trace of free carbonic acid has been removed by lime water from an apartment, in which individuals have been found in a lifeless condition, others on entering it have experienced the usual symptoms of poisoning by charcoal vapour. Hunefield affirms that the poison consists of a volatile mixture

of empyreumatic oil, resin, naphthalin, and pyro-carbonic acid; Dr. Bird is quite certain that this result should be considered to refer to wood-smoke—rather a positive mode of deciding the matter, when, as he admits, he has not seen the German Professor's work, and the term employed in the quotation he has met with (*kohlendunst,*) is not, so far as we can ascertain, open to such interpretation. Sachs conjectures that the poisonous ingredient may consist merely of carbon, suspended in a pulverulent form in the air and aspired into the air-passages: this hypothesis is unsupported by experimental proof. Again, Dr. Bird, reflecting on the property possessed by charcoal of absorbing gases and aqueous vapour by exposure to the atmosphere, became anxious to determine whether charcoal might not, when heated in the air, evolve cyanogen or some of its compounds, as moist charcoal saturated with air contains all the elements for the formation of hydrocyanic acid: an experiment was accordingly instituted, but without leading to the detection of any of the acid. It is but just to state, especially as Dr. Bird has not himself done so, that Mr. Coathupe, of Bristol, some time since announced in the Lancet (Jan. 1839), that he had discovered in the peculiar volatile product of burning charcoal, which, according to him, constitutes its really toxic ingredient, a body closely resembling cyanogen in most of its properties. Finally, though last, assuredly not least, Berzelius expresses himself as follows: "Charcoal fume consists neither of carbonic acid nor oxide, nor of light carburetted hydrogen, but of a peculiar combustible body, that soon produces vertigo, headach, vomiting, and even death, notwithstanding that sufficient oxygen may be present in the apartment to support animal life." The analysis of Orfila of the products of dimly burning charcoal, and of the same substance in a state of active combustion, as well as the support they lend to Dr. Babington's hypothesis, respecting the agency of carburetted hydrogen in producing the noxious influence of charcoal vapour, are too well known to justify more lengthened mention here.

Curiously enough, Dr. Bird's conclusion on the subject is "that carbonic acid obviously forms the most important ingredient in the products of charcoal combustion:" elsewhere he goes further, indeed, and forgetful of, or at least indifferent to, opinions emanating even from Berzelius, he rather dogmatically terms the gas in question "*the* poisonous ingredient of charcoal vapour." That carburetted hydrogen is not the destructive principle of the fumes of dimly burning charcoal appears, according to Dr. Bird, to be proved by the fact that it is frequently present in coal mines in explosive proportion, without causing mischief to the miners respiring it. But this is an unsound argument, for, as we shall have occasion to mention in noticing the next paper, most serious consequences have been known to ensue from the inhalation of an atmosphere impregnated with coal gas, though not to a sufficient amount to admit of explosion. Dr. Bird suggests that the more deleterious influence of the fumes of slowly burning charcoal in comparison with those of the same fuel in a state of vivid combustion, should be ascribed to the greater quantity of heat evolved in the latter instance, whereby stagnation of the carbonic acid generated is more effectually prevented.

Having settled the point at issue in his own way, our author proceeds

to enquire whether the carbonic acid, evolved from a burning chauffer in an unventilated apartment, descends towards the floor or mounts towards the ceiling. Its specific gravity carries it down; its elevated temperature tends to raise it to the upper strata of air; the law of the mutual diffusion of gases, to disperse it equably through the apartment. The proportional influence of each of these agencies admits of calculation, but the result of direct analysis of the air at different heights is a method of deciding the question less open to cavil. Now, Dr. Bird declares, "he has had more than one occasion to observe that a much larger comparative proportion of carbonic acid gas has been found mixed with the lower strata of air, than with the upper:" it were to be wished, however that he had substituted an accurate account of his experiments (and, especially, stated the temperature of the room at the time they were made), for this somewhat vague statement, more particularly as it clashes to a certain extent with the opinion of one of the most practised toxicologists of the day—M. Devergie, of Paris. (*Brit. and For. Med. Rev.*, Vol. VI. p. 243.)

There is a strange difference in the results obtained by different observers, regarding an apparently very simple enquiry, the proportion in which carbonic acid renders atmospheric air unfit to support combustion. Cavendish found that a wax candle was instantly extinguished in a receiver containing 11 per cent. of the gas; Mr. Taylor, that a taper burnt very well in air containing $\frac{1}{10}$ of carbonic acid, and was slowly extinguished when the bulk of gas amounted to 25 per cent.; whilst Dr. Ure fixed 15, and Turner 20 per cent. as the proportion capable of extinguishing a candle. These discrepancies, Dr. Bird has no doubt, may be traced to the differences in the diameter of the receiver used and in the kind of candle employed: the thinner the taper, according to this gentleman, and narrower the receiver, the more readily, *cæteris paribus*, does the former become extinguished. His experiments induce him to consider the statements of Turner and Mr. Taylor the most generally accurate. The practical value of such experiments is however materially lessened by the ascertained fact, that a candle will burn in an atmosphere containing such a proportion of carbonic acid as will prove rapidly fatal to animals: and, on the other hand, it is affirmed, that an individual has remained with impunity for several minutes in the atmosphere of a coal pit, in which ignited candles were repeatedly extinguished.

Dr. Bird's paper is illustrated by three cases: one already published by Mr. Chapman in the Lancet for December, 1838; another in which death was produced "by the inhalation of air containing carbonic acid evolved from a lime kiln;" a third, the notorious case of Trickey, who met his death in the year 1838 in St. Michael's Church, Cornhill. A report of the last by Mr. Gear had already appeared in the Lancet; the only important difference in the former and present versions being that, according to Mr. Gear, the deceased had voided his feces and urine, while no mention is made of his having vomited, immediately before death; whereas in the account transcribed by Dr. Bird from the notes of another practitioner, vomited food is said to have been found near the mouth of the deceased, but the contents of the rectum and bladder are not alluded to. It is needless to refer at any length to this case, which has already furnished the material for prolonged debate elsewhere.

The reader will observe that Dr. Bird, in ranging the second case in the same category with the others, carries into practice his assumption that carbonic acid is the toxic agent of charcoal vapour.

The most useful portion of Dr. Bird's production undoubtedly consists in two tables, exhibiting the most remarkable external appearances, as well as the results of dissection, of a certain number of individuals killed by carbonaceous fumes. The almost invariable presence of meningeal or cerebral lesions or both, is clearly demonstrated by these tables; and in the legitimacy of the following conclusions, drawn from them by our author, we are disposed to acquiesce, provided the words *charcoal vapour* be everywhere substituted for *carbonic acid.*

"1. Carbonic acid gas, sufficiently diluted with air (as in charcoal vapour), does not act fatally, by closing the glottis spasmodically, or by excluding oxygen, but as a specific poison. 2. Such an atmosphere will produce death, although it may contain a sufficient amount of oxygen to support life *per se*, and to allow the arterialization of the blood to proceed; on which account no dependence can be placed on the dark or florid colour of the blood, as arguments for or against poisoning by carbonic acid gas. 3. Such diluted carbonic acid acts most probably on the nervous system primarily; and, secondarily, by no means essentially on the circulating fluid. 4. The death of persons inhaling an atmosphere, vitiated by carbonic acid, is produced by the accession of apoplexy, often attended by serous effusion into the ventricles or on the surface of the brain, and occasionally even by extravasation of blood from some cerebral vessel. 5. No dependence can be placed on the bloated and red, or pale and contracted features; on the liquidity or coagulated state of the blood; on the injection or paleness of the mucous membrane of the intestinal tube or air-pipes; or on the flexibility or rigidity of the limbs, as positive arguments for or against the action of carbonic acid as a cause of death."

v. *Two Cases of Poisoning by the Inhalation of Carburetted Hydrogen.* By Mr. J. P. Teale.—These cases are essentially deficient, inasmuch as the subjects of them were not seen during life either by medical practitioners or even by nonprofessional persons; nevertheless, as the post-mortem examination was well conducted, and from the appearances thereby disclosed, as well as the circumstances under which death took place, no doubt can be entertained that the individuals perished from the inhalation of coal gas, Mr. Teale's paper is not an unimportant contribution to the pathology of gaseous poisoning.

The sufferers, Mrs. B. æt. sixty, very corpulent, and long subject to sudden attacks of loss of muscular power, and Miss W. æt. twenty-two, active and enjoying good health, occupied three small rooms in an almshouse at Leeds. On the 21st December, an explosion took place in one of these rooms on the introduction of a light, and removed a disagreeable effluvium of coal-gas, of which the inmates had complained during the day. At half-past ten both females retired to the same bed, and the following morning were found stretched upon it as if asleep, the bedclothes appeared scarcely at all disturbed. On the arrival of some medical gentleman, the body of Mrs. B. was found to be quite cold, and the whole muscular system rigid; Miss W. appeared lifeless, but the trunk and extremities were warm, artificial respiration was accordingly performed, but without producing any indication of vitality.

A rent of about two inches in length was discovered in an adjoining

gas-pipe, from which there was a ready transit for the effused gas to the rooms occupied by the deceased.

The dissection was made at half-past six the same evening: for the full particulars we must refer to the original paper, and confine our notice to the following appearances observed in both bodies in common: 1. A pallid appearance of the integuments and of the internal tissues, with the exception of some portion of the mucous membranes. 2. Florid discolorations on the neck and back. 3. Florid tint of the muscles. 4. Absence of venous congestion. 5. Fluidity of the blood. 6. Florid colour of the same fluid. 7. Infiltration of the lungs. 8. Injection and ecchymosis of the mucous membrane of the small intestines, and injection of the mucous membrane of the air passages. 9. The early occurrence of rigidity of the muscles after death.

In 1830, M. Devergie published an interesting account of poisoning with coal-gas in the Annales d'Hygiène. In this instance five individuals suffered more or less from its inhalation, and one of the number perished. As it is possible, however, that in the fatal case, a bean, discovered in the right bronchus, may have entered that tube before death, and so modified in some important particulars the post-mortem appearances, we shall not allude to them further than to say that the same pallor of the tissues, observed by Mr. Teale, existed also in the subject opened by M. Devergie. The latter pathologist noticed in his case a particular earthy discoloration of the liver, and seems inclined to invest such a condition with considerable importance as an anatomical sign of the poisoning under examination; our countryman's cases prove him to have been too precipitate in his inference. The prominent symptoms manifested by his patients were general prostration, marked debility, and somnolence, and the conclusion drawn by him from these and the morbid appearances is that coal-gas exercises a special deleterious influence on the economy, that it modifies the nature of the blood, and acts principally on the brain and liver. He also informs us that it produces its toxic effects, when it constitutes less than $\frac{1}{11}$ of the atmosphere; for a lamp remained burning all the night the accident occurred; and when the inmates of the house were roused, several candles were lighted without any explosion occurring.

The reader will derive instruction from a close comparison of the French and English contributions on the subject.*

VI. *Case of Imperforate Uterus, with Remarks.* By Mr. A. TWEEDIE. —Mr. Tweedie continues the history of a case of presumed imperforation of the uterus, in which delivery was effected through an opening made by incision, in the supposed situation of the occluded os. The operation with its immediate results were described in the second volume of the Reports, and briefly noticed in one of our early Numbers. The patient, Mrs. P., was again seized with labour-pains on the 31st of last December: on examination, Mr. Tweedie discovered an irregular opening, about as large as a penny, at the uterine extremity of the vagina, bounded anteriorly by a strong, firm, unyielding ridge, on which the cicatrix of the original incision was plainly felt. On the evening of the third day after

* See also Ollivier, Annales d'Hygiène, 1838, t. xx., p. 120.

labour had commenced (the pains had been urgent and energetic for about fourteen hours), no enlargement of the orifice having taken place since the morning, the pulse becoming quick, the skin and the vagina hot and dry, Mr. Tweedie deemed it unadvisable to wait longer, and, accordingly, made an incision, an inch long, through the cicatrix. Three quarters of an hour after the operation, the head was forced through the uterus, an additional rent of that organ having, it is believed, taken place, and the delivery was completed without further trouble. Three weeks after, the woman was able to walk a distance of four miles.

VII. *On Incision, in Cases of Occlusion and Rigidity of the Uterus.* By Dr. Ashwell.—Dr. Ashwell comments at some length on the case just referred to, and on a number of others of a similar character recorded by different writers.

It is manifest that, in cases which have been reported as examples of an imperforate condition of the uterus, the real state of the parts may have been either of the three following: there may have been total occlusion of the cervical orifice; or the os tincæ may have been shut out from all communication with the cavity of the vagina by adhesion of the posterior wall of the latter to the anterior surface of the uterus; or the womb may have been in possession of a normal inferior opening, tilted upwards and backwards beyond the reach of the finger of the accoucheur, in consequence of the organ being in a state of anterior obliquity. The difficulty of Mrs. P.'s first labour is referred by Mr. Tweedie and Dr. Ashwell to the presence of the first of these conditions; but no actual proof of the fact has been adduced, and it is purely hypothetical to consider the smooth attenuated part of the organ, where Dr. Ashwell practised his incision, as the site of the os closed up with adventitious structure. An example of the second condition was long since recorded by Gauthier,* in which that practitioner divided the anterior wall of the uterus by a transverse incision from right to left, and delivered his patient of a healthy full-grown infant with the forceps. A case of very similar character is related in Mr. Tweedie's paper from the practice of Dr. Hamilton. In these instances, as might be foreseen, one of the most remarkable peculiarities in the anatomical condition of the parts, was the shallowness of the vagina at its posterior aspect. The same anomaly existed in Mrs. P.'s case; and this, coupled with the fact that no cervix could be discovered, seems to lend some plausibility to the supposition that the orifice may have been excluded from the cavity of the vagina by adhesions of the latter to the uterus. But, as the reader is no doubt aware, the majority of reputed instances of imperforate uterus should,—if we be disposed to agree with a very numerous body of practitioners, among whom we may mention Desormeaux, Velpeau, Denman, Dewees, and Baudelocque,—be classed with our third category; the difficulty of finding a uterine orifice having, according to them, depended not on its deficiency from occlusion, but on its abnormal position. Now the fact, that from the time of Mrs. P.'s marriage up to the occurrence of labour-pains, "she had not a day's ill health," seems to militate in favour of

* Nouveau Journal de Médecine, t. vii., p. 36.

her case being one of persistent anterior obliquity, for it is hardly probable either that adhesion of the vagina to the cervix or conglutination of the orifice could have taken place without some local suffering and general reaction.

Dr. Ashwell dissents from the opinion of the above-named obstetricians, chiefly because "he has never met with any seriously protracted labours from obliquity." But if, not to speak of the experience of others, Mrs. P.'s uterus, at the period of her first labour, had really been affected with anterior obliquity,—and where is the conclusive evidence that such was not the fact?—Dr. Ashwell has most indubitably encountered at least one case of obstructed labour depending on that malposition; hence, in the assertion quoted, this writer has fallen into a palpable *petitio principii.* He *thinks* he could mention very respectable authors, whose experience corresponds with his own in this particular; if so, we cannot comprehend the advantage gained by concealing their names. He further alleges that, allowing the hypothesis of obliquity its full weight, it must be recollected, every hour of urgent uterine effort tends to rectify the displacement, and that if, after really powerful pains have been in action for ten or twelve hours, the os uteri be still undiscoverable, the legitimate inference is that no orifice really exists. We cannot subscribe to the justness of this argument, except its application be confined to cases of very trifling obliquity; unless the efforts of the patient be assisted by simultaneous attempts at reposition on the part of the accoucheur, whether the uterus be in a state of simple *anteversion* or of *anteflexion*, (in the latter case the os tincæ is of course within reach, but the progress of labour is not the less interfered with by the malposition of the body of the organ,) the play of the diaphragm and muscles tends to counteract the influence of uterine action, and augment the obliquity. In a case of this kind, in which no progress had been made after several hours' continuance of powerful pains, the first care of M. Velpeau, when summoned to the patient's assistance, was to direct her to moderate the violence of her efforts as far as possible: had he participated in the opinion expressed by Dr. Ashwell, his advice would have been widely different.

We must not omit to state that there is one very strong argument in favour of Dr. Ashwell's opinion, that Mrs. P.'s was originally a case of conglutinated os uteri. It seems, in truth, hardly possible that an artificial aperture in the body of the uterus could have remained open for a year, and that, if such an extraordinary occurrence had taken place, the foramen could have borne a sufficiently strong resemblance to the natural orifice, to be mistaken for that opening by the accoucheurs, who attended the woman in her second confinement. Extreme obliquity of the uterus is, besides, stated to occur rarely in primiparous women; this is, however, a point of inferior importance. The fact that Dr. Ashwell himself seems to have conceived no doubt of the reality of occlusion, carries considerable weight with it in our minds; for though in our character of reviewers we freely criticise his opinions, we have long learned to entertain the highest respect for that gentleman's scientific and practical acquirements.

But whatever be the cause of the non-expulsion of the fœtal head from the uterus, persistent obliquity, adhesion of the vagina round the cervix,

or occlusion of the os, the cases become, practically speaking, identical, after the efforts of nature, aided by medical means, have been allowed to act unavailingly for a certain time. The principle on which relief is afforded is one and the same, an artificial opening must be made in order to lessen the chance of extensive and irregular laceration of the womb. The determination of the time at which this decisive measure should be had resource to, is, perhaps, one of the most difficult points in practical midwifery: Dr. Ashwell's directions for guiding our conduct are judicious, and to these we beg to refer the reader. A longer trial should be given to the natural efforts, assisted by attempts at reposition, in cases where the obstruction depends more on obliquity than on the other causes mentioned; but no reasonable argument can be adduced against the propriety of incising, when the malposition is found to be unchangeable, and, as in a case recorded by Lauverjat, the anterior wall is forced down to the genital fissure by each pain, at the imminent risk of rupture. In the case referred to, the operation was successfully practised, though *superficial* laceration of the substance of the uterus had already taken place. Instances of obliquity, in which such a measure becomes really called for, are, we hasten to add, extraordinarily rare.*

We have already described the practice of Gauthier in a case belonging to the second class: Dr. Hamilton, in the analogous instance referred to, "opened completely *the upper part of the vagina*;" this, we presume, means the portion of the uterus bounding that canal superiorly. The Professor does not inform his correspondent (the case is related in a letter to Mr. Tweedie,) whether his incision was transverse, antero-posterior, or crucial. From a fortnight after the lady's delivery, till she had perfectly recovered, and this is a point worth attending to, a dipt candle was, by his directions, daily introduced into the orifice.

In cases of occlusion, the practice is either to break up the adhesions with the finger, a bougie, or catheter, or divide them with a bistoury, and carry the incision to a certain extent beyond the lips of the orifice. Nägele, junior, is the advocate of the former proceeding; and where the occluding tissue is thin and fragile, it is evidently the most judicious,—provided also the surrounding substance of the uterus be healthy, and capable of normal dilatation.† But Dr. Ashwell justly observes, that Nägele's method of procedure is inapplicable, when the substance blocking up the os is thoroughly and firmly organized: admitting that such adhesion might be broken up by forcible traction with the finger, it would be, to say the least, imprudent to resort to the violent manipulation necessary for the purpose; the resulting contusion of the parts would be likely, by the local or general inflammation it engendered, to diminish materially the probabilities of recovery. We believe, too, with Dr. Ashwell, that

* Lauverjat reports another case of extreme anterior obliquity, in which bleeding, baths, and "other ordinary means" were employed, without producing any marked benefit. After sixty hours' continuance of pains, the orifice was sufficiently dilated to allow of the anterior border being reached with the finger; labour now advanced pretty quickly, but the infant was expelled in a putrid state; inflammation and gangrene of the uterus carried off the mother. The question is whether an operation, timeously performed, would or would not have averted this disastrous result. See *Nouvelle Méthode de pratiquer l'Operation Césarienne*, p. 72.—Paris, 1788.

† A case, successfully treated in this manner by M. Felix Hatin, has been recently reported in the French Journals. See p. 263 of our present Volume.

"it may, perhaps, be fairly assumed, that the risk of unlimited laceration of the uterus and adjacent parts, is much less, where incisions of tolerable extent have been discreetly made, than where merely a diminutive central aperture has been formed by a blunt instrument."

Our author next proceeds to consider those cases of much more frequent occurrence (such as that of Mrs. P. in her second labour, admitting that hers was in the first instance a case of *occlusion*), in which a *contracted os uteri* coexists with extreme rigidity, depending on organic disease of the surrounding tissue. He attempts to prove the general impropriety of persisting, for any length of time, in forcible attempts to dilate the orifice with the fingers, in cases of this description; and certainly succeeds in showing, from the recorded experience of others, that such dilatation is far from being the same non-hazardous and effectual operation here, that it is known to be when rigidity is unassociated with structural change. Regarding the precise moment when the operation should be performed, Dr. Ashwell observes, that *time* cannot be the sole element for regulating our interference; we ought not to hesitate about operating, "if the violence and frequent returns of the uterine efforts threaten rupture of the womb,—if there be distressing and constant pain about the neck or body of the uterus, or in any other part,—if the countenance becomes turgid and dark,—if perspiration issues at every pore,—if the pulse is full, strong, quick, and incompressible, and all this continue in spite of free venesection, and the exhibition of tartarized antimony, with or without opium."

There is a curious case transferred to our Sixth Volume (p. 235), from the Medicinische Zeitung, showing that the obstruction caused by a disorder of a different description, may require similar treatment for its relief. Here a large portion of the uterus prolapsed between the thighs, and all attempts to dilate the orifice to a greater diameter than that of half a dollar proving fruitless, an incision three inches in length was made in one side of the os, about thirty-six hours after the commencement of labour. Similar cases, terminating like it by the recovery of the mother, are related by Chopart and Capuron.* In two of these instances, it is not distinctly stated that diseased induration, beyond the general hardening consequent on the exposure of the lower portion of the organ to the air, and abnormal friction, existed in the cervix; in one of them, indeed, the close constriction exercised by the vagina on the neck, seems to have constituted the main hinderance to dilatation; in the other, the cervix was hard and callous. There are a few more such cases on record. The direction and length of the incision are obviously points of importance. In Mrs. P.'s case (her first labour), Dr. Ashwell carried his incision an inch forwards towards the bladder, and the same extent backwards to the sacrum: the preference appears to have been given to the antero-posterior incision, from apprehension of wounding the branches of the uterine arteries; no such accident, however, occurred in Gauthier's case, in which transverse division was practised. It is scarcely necessary to add, that the rectum and bladder should be evacuated before the incision is attempted; the other points of manipulation are such as must occur to every individual experienced in the surgery of the female genital

* Vide Churchill, Diseases of Females, p. 284.

organs. The use of the speculum, as originally had recourse to by Dr. Simson, is certainly not called for. Dr. Ashwell is, on *à priori* grounds, favorable to a crucial incision, as likely to prevent extensive laceration more effectually than the simple; we are disposed to coincide with him in his predilection. There is seldom much blood lost: should collapse and fainting occur, ammonia and brandy may be administered. The forceps has, in the majority of cases, been required for the termination of the labour.

Before quitting this subject we would, with Dr. Ashwell, enter our strongest protest against rash employment of the knife; the medical attendant should never have recourse to it, unless upon the deliberate conviction that his patient's safety cannot be otherwise secured; and to strengthen him in this conviction, as well as to shield himself from undue responsibility, the sanction of another practitioner should always be obtained.

VIII. *Observations on Fibrinous Concretions in the Heart.* By Dr. Hughes.—Dr. Hughes, like his predecessors, divides cardiac concretions, in respect of their cause, into two classes,—accumulations of fibrine resulting from retardation of the blood, and from inflammation of the endocardium. He conceives that the presence of the following characters in concretions, renders the fact of their formation, previous to death, incontrovertible; though he by no means affirms that the absence of these is conclusive of the cadaveric origin of the bodies so deficient.

"1st. When strict adhesion exists between them and the plane surfaces of the heart; and particularly when the membrane, from which they are detached, is found rough, vascular, and sprinkled with bloody points. 2dly. When a firm, white, fibrinous mass is found in one of the cavities, entirely separate, and detached from a dark purple, or mixed coagulum, filling up the remaining portion of the same cavity. 3dly. When, after the removal of the easily separable coagulated blood, we leave behind a smooth unbroken layer, or coating of fibrine, attached to a portion or the whole of the cavity in which that coagulated blood was previously lodged; though the layer or coating may be adherent, simply by passing behind and mixing with the musculi pectinati of that part of the organ to which it is attached. 4thly. When any changes, vital or chemical, the result of organization or degeneration, are observed in the concretion, which are not to be discovered in any other coagula existing in the same heart."

Some of the principles on which these propositions are founded are not so obviously correct as the author takes for granted; but a discussion of their merits would occupy more space than we can afford for the purpose,—we must, therefore, pass on to Dr. Hughes's description of his four varieties of concretions, caused by retardation of the blood, the *polypoid*, the *massive*, the *parietal*, and the *globular*.

The *polypoid* variety usually occurs in the form of a solid rounded mass of fibrine, varying in size from that of a filbert to a pullet's egg; it is sometimes attached by a broad base, at others by a narrow pedicle to the surface, and is often covered with a delicate membrane, capable of being injected. Its internal structure varies with its age, and the degree of decomposition it may have undergone.

The *massive* is of exceedingly irregular shape, from the number of prolongations arising from its surface, and passing into the depressions, or through the orifices, of the cavity in which it is placed. There is often

much difficulty felt in determining its date, but when most decidedly antecadaveric, it is thin and expanded, of a pinkish white colour, firm in texture, easily separable into layers, and attached to some of the plain surfaces of the heart by simple adhesion. The adhesion is, however, principally dependent on the intertwining of the processes alluded to among the musculi pectinati, or chordæ tendineæ, and the prolongation of the foreign body into other cavities, whereby the channel for the passage of the blood is considerably narrowed.

Of the *parietal* species, Dr. Hughes has seen only one recent example; this formed a general lining for the cavity in which it was found, and consisted of an irregular sort of network, about a line thick and two broad, firm in point of consistence, and with a smooth glistening surface.

The *globular* concretion varies in size, from that of a pea to that of a pullet's egg, is of an opaque white, or light dirty brown colour, smooth and even externally; when large, cystiform, and containing fluid. In the latter case, the wall of the cyst is seldom more than a line in thickness, but separable into layers; the contained fluid consists of blood, either healthy or in an abnormal state; of serum, or of a fluid which bears a certain degree of resemblance to pus: of this, more by and bye. These concretions are either with or without pedicles. When situated in the appendices of the auricles, they affect different characters from those we have enumerated; for these we must refer the reader to Dr. Hughes's pages.

Dr. Hughes illustrates the relative frequency with which the different cavities contain fibrinous concretions, by a tabular view of sixty-two cases, collected from various sources. From these we learn the following proportional numbers.

32 on the right side of the heart.
38 on the left.
15 in the right auricle.
21 in the right ventricle.
14 in the left auricle.
27 in the left ventricle.
7 in the right and left sides at the same time.
4 in the right auricle and ventricle.
3 in the left auricle and ventricle.
2 in both auricles.
6 in both ventricles.
1 in all the cavities.
1 in the right auricle and ventricle, and left ventricle.
1 in the right auricle and left ventricle.

These numbers seem to show that the opinion generally held by authors,—at least Dr. Hughes ascribes such opinion to them,—namely, that these bodies are most frequently met with on the right side of the heart, is incorrect. M. Bouillaud does, we think, make an assertion to the effect stated by our author, but he evidently intends it to refer to concretions of all kinds, and not simply to those caused by retardation of the blood. Now had Dr. Hughes included in his table the forty-nine cases of cardiac concretion scattered through the French writer's volumes, and not simply reckoned the six examples given in the chapter especially devoted to the subject, he might have still found the common opinion to be substantially correct.

Dr. Hughes proceeds to speculate on the mode of formation of his first class of concretions. We differ with him respecting the alleged peculiar proneness of the blood to coagulation, in long-standing diseases of the heart; and when, in attempting to account for the production of certain concretions, assumed to belong to this category, he has recourse to the influence of an inflamed endocardium—even though that influence be an indirect one—he forgets that the fact of such causation entitles them to be ranked with his second order.

No new hints are thrown out for determining the exact age of these concretions. Respecting the changes occurring in the globular species of the bodies under consideration, and the nature of their central fluid, Dr. Hughes, after enumerating the principal hypotheses brought forward in explanation of the presence of a puriform liquid in their interior, declares his own persuasion to be, that such liquid is never composed of true pus. M. Magendie states, in the first of his lectures on the blood, already reviewed by us, that in one particular instance he found that the fluid, contained in a fibrinous sac entangled in the walls of the right ventricle, was not composed of pus, but of tuberculous matter: this observation (which, strange to say, he quotes incorrectly,) is adduced by our author as confirmation of the opinion he holds; we confess we do not share in his belief of its importance. Be this as it may, Dr. Hughes conceives that the formation of the variously-coloured fluid may be "simply explained" as follows. "A concretion containing many, few, or no red particles, according to the celerity or tardiness with which it originally assumed, exteriorly, a solid form, and more or less serum in proportion to the amount of subsequent contraction, may be incapable of organization. Subject, therefore, to the laws of unorganized or unorganizable matter, that portion containing the most serum, or first coagulated, gradually softens and breaks down, or degenerates into a fluid, the physical characters of which depend on the materials of which the concretion was originally composed." So far as we can perceive, this theory is nothing more than a somewhat pompous enunciation of the doctrine of "softening," "breaking down," or "decomposition" of fibrine, advocated by Dr. G. Burrowes, by Dupuytren, and recently by Mr. G. Gulliver.*

On fibrinous concretions induced by and complicating endocarditis, Dr. Hughes makes no observation of novelty,—except that, when attached to the surfaces of the valves, bodies of this class are uniformly found on the side opposed to the direct current of the blood.

The section devoted to the symptomatology of cardiac concretions, is neither full nor satisfactory; Dr. Hughes appears to have had very limited clinical experience on the subject. He suggests, on the authority of one of M. Magendie's recently broached crotchets, the administration of the salts of potass and soda, in the hope of dissolving such portions of fibrine as may have been deposited in a concrete state, and of decreasing the coagulability of the remainder.

Among our Foreign Selections will be found an analysis of a paper on the same subject, by M. Bouillaud, which may be regarded as the complement of the present essay.

* Medical Gazette, March, 1839.

IX. *Analysis of Bones affected with Mollities Ossium.* By Dr. Rees. —Dr. Rees has analyzed those specimens of bone affected with mollities, in order to ascertain whether the law, observed to regulate the relative proportions of animal and earthy matter in the healthy tissue of different portions of the skeleton, does or does not hold good in respect of bones so diseased. The bones examined were the fibula, one of the ribs, and a vertebra; and it will be perceived, from the subjoined table, that in the morbid as well as in the natural condition, the first-named bone contains more earthy salts than either of the others, and the second more than the third.

	Mollities.		Health.	
	Earths.	Animal Matter.	Earths.	Animal Matter.
Fibula - -	32·50	67·00	60·02	39·98
Rib - -	30·00	70·00	57·49	42·51
Vertebra - -	26·13	73·87	57·42	42·58

X. *Case of Division of the Tibia for the Cure of Deformity, occasioned by a Gunshot Wound.* By Mr. Key.—This is a case of very marked deformity of the right leg, resulting from the loss of a considerable portion of the inner surface of the tibia, accompanied with fracture. The injury, caused by a musket-ball, occurred in the East Indies, in August, 1835. Examined by Mr. Key, in October, 1838, the broken ends of the tibia were found to have united at an angle, and the bone to have deviated from its natural line, in relation to the femur. The head of the fibula had, besides, been forced away from its articulation with the tibia, and formed an unnatural prominence above the usual position, in reference to the last-named bone. As the fibula was not broken at the time of the accident, it maintained its natural straight figure, the direction of the whole bone being parallel to that of the lower division of the tibia. The limb was shortened to such a degree, as to oblige the patient to walk on his toes; and the heel raised an inch and a half from the ground, when he stood upright. It was decided, in consultation with Sir Astley Cooper, that the limb might be restored to a useful state, and the deformity remedied. On the 14th of October this decision was acted on by Mr. Key. This gentleman sawed the tibia across, in the situation of the fracture, just below the attachment of the soleus muscle, and found, that by this operation, the lower part of the leg became capable of being freely moved in every direction; there was consequently no necessity, as it was apprehended there might be, for dividing the fibula. The limb was allowed to rest on pillows, without much restraint, for about ten days after the operation; at the end of this period, the wound had gone through its several stages, without exciting more than inconsiderable constitutional disturbance. The limb was thenceforth kept straight, with an under and two lateral splints, well padded,—a tourniquet being applied at either end of the splints, as it was found that the constant tendency to displacement could not be effectually prevented by common tapes and bandages. The process of union and consolidation was necessarily tedious, from the limited extent of contact between the corresponding surfaces of the tibia; at the beginning of January last, union had, however, taken place, and on the 24th of the same month, the patient was able to leave town. On the

10th of March, the long splints were discontinued, and the leg maintained as good a position as when the patient had been in town. The cicatrization of the wound had been retarded, by the exfoliation of several pieces of bone; in other respects, the patient was going on well.

This operation reflects the highest credit on the distinguished surgeon who performed it. The change effected in the bearing of the leg is well shown in an engraving.

XI. *Case of Spermatocele, or Varicocele, treated by Excision of a Portion of the Scrotum.* By Mr. Bransby Cooper.—We have here a case of varicocele (treated by excision of a portion of the scrotum) which is, according to the narrator, as well as the inventor of this operation, a most favorable instance of its successful adoption. The perusal of Mr. Bransby Cooper's narrative convinces us, notwithstanding, that the new operation is scarcely, if at all, less perilous than Professor Breschet's method, of obliterating the dilated veins by instrumental pressure.* Mr. Bransby Cooper dreads phlebitis, and finds fault with former modes of operating, on the score either of their acknowledged proneness to bring on, or of their being actually designed to produce, that inflammation. He is perfectly correct, in looking on the dangers of phlebitis as the main objection to their general employment. But that a similar inconvenience appertains to the present operation is abundantly evident, from the details of his own case; nay, more, that Mr. Cooper himself considers phlebitis to be a necessary element in ensuring a successful issue, appears from his own conception of the *modus operandi* of the novel procedure. To the truth of this, his own words shall bear witness:—" It appears to me, however, and indeed seems apparent, from the daily report of the above related case, that the excision of the portion of the scrotum leads to the cure of spermatocele, *by inducing inflammation, and consequent obliteration of the diseased veins;* and without the same risk as attends upon the application of any immediate means to the veins themselves, as must be the case, either by the employment of a ligature, or the excision of the varix." It will be perceived that Mr. Cooper differs in his notions, respecting the manner in which the cure is effected, from his gifted relative. Sir Astley believes the success of the operation depends on the constriction exercised on the veins, in consequence of the reduced bulk of the scrotum. Mr. Cooper's assertion, that phlebitis, produced in the present manner, is less likely to prove dangerous than the same disease when otherwise caused, is, we need hardly point out to the reader, purely hypothetical.

XII. *Observations on Abdominal Tumours, and Intumescence: illustrated by Cases of Renal Disease.* By Dr. Bright.—The contents of this valuable contribution to renal pathology will be noticed elsewhere.

* Vide Walshe on Breschet's operation for varicocele, in *Medical Gazette*, Dec. 1834; and *British and Foreign Medical Review*, Vol. VIII. p. 254.

No. IX.

I. *On the Dislocation of the Os Humeri upon the Dorsum Scapulæ, and upon Fractures near the Shoulder Joint.* By Sir ASTLEY COOPER. —This communication opens with an account of a doubly remarkable luxation of the humerus into the infra-spinata fossa. In the first place, the displacement occurred during the convulsive struggles of an epileptic fit, at a time when the patient was watched by his wife, who avers that he neither fell from his bed, nor dashed his shoulder against any resisting surface; and secondly, the head of the humerus could, by extension, be drawn into the glenoid cavity, but the moment the traction was discontinued, the extremity of the bone slipped back, with a grating sound, on the dorsum scapulæ. The attendant surgeons felt persuaded, that the mobility of the head of the humerus depended on a piece of the rim of the glenoid cavity, or of the lesser tuberosity, or of the head itself, having been broken off. The incorrectness of this notion was, however, established on the patient's death, seven years after the occurrence of the dislocation, which had never been reduced; no trace of fracture could be discovered. "But the tendon of the subscapularis, and the capsular ligament, had been torn from the smaller tubercle of the os humeri, and the bone was consequently drawn back by the action of the infra-spinatus and teres minor muscles, there was no support given to the head of the bone, when returned into its cavity: but so soon as it was replaced, it was drawn back by the action of the two posterior muscles, and all the appearances of dislocation reproduced." "Under these circumstances," adds Sir Astley, "the bandage, required to keep the bone in its place, should be placed on the fore part of the chest, and behind the shoulders, to keep the head of the bone forwards; and a pad ought to be placed behind the head of the bone, to prevent it from slipping from its cavity; . . . . to this bandage a sling must be added, to support the elbow, and to keep it back." The reader will do well to consult the description and engraving of the ingenious apparatus, constructed on this principle, and employed by M. Sédillot in a case of dislocation into the infra-spinata fossa, reduced by that gentleman *a year and a fortnight* after its occurrence.*

Sir Astley next proceeds to examine the diagnostic and anatomical characters of certain accidental lesions, which are liable to be confounded with simple dislocation. He reduces these to three classes: 1. Dislocations forwards, with fracture, and detachment of the head of the bone. 2. Fractures through the anatomical neck of the humerus. 3. Fractures through the surgical neck. We need only thus present the reader with the bill of fare, and the name of the master of the feast, to induce him to become a guest at the board.

II. *Case of Poisoning by Sulphuric Acid.* By Mr. A. TAYLOR.—There is nothing particularly remarkable in the history of the patient here referred to, a man aged forty, who died twenty-five hours after swallowing a wine-glass full of sulphuric acid. The pharynx, œsophagus, and stomach, presented the usual evidences of the action of the acid; but

* Mémoire sur une luxation de l'épaule en arrière, &c. Paris, 1833.

not the slightest indication of free, or combined sulphuric acid, was obtained from the analysis of the contents or substance of the latter organ: the poison had been effectually removed by vomiting and purging, aided, perhaps, by the neutralizing action of magnesia, which had been freely exhibited. The aortic lining membrane was of a bright scarlet colour; but the vessel was filled with blood, and the body examined three days after death; hence, so far as Mr. Taylor's details go, they justify the conclusion, that the red tint resulted from imbibition. The state of the blood is not noted with sufficient precision to enable us to determine, whether the case furnishes evidence for or against M. Magendie's notions respecting the action of sulphuric acid on that fluid.

III. *On Amputation.* By Mr. Bransby Cooper.—Mr. Cooper, alarmed by the rapid extension of railways, and increasing employment of machinery, revives the *vexata questio* of the propriety of amputating, after certain severe injuries, or of attempting to save the limb by treatment. The enquiry into this point is rather confusedly mixed up with the examination of a very distinct problem: namely, the necessity of amputating being incontestable, what is the precise time, after the infliction of the injury, at which the operation should be performed? The latter point is thus decided: "when there can be no hope of saving the limb, its removal should be effected at the earliest opportunity, before inflammation and swelling have commenced, ... unless the patient be in a state of collapse; in this case, the surgeon must wait for reaction." Such is, if we mistake not, the doctrine inculcated by surgeons generally: Mr. Bransby Cooper brings forward no novel argument in its support.* In respect of the former, we find an admission, that it is very difficult to define the precise extent of injury requiring immediate amputation; of this, all who have simply witnessed the practice of a large hospital, without ever having visited the "tented field," will recognize the justness: our author's information is, unfortunately, not calculated to lessen the embarrassment of the surgeon called upon to decide such a question.

Before taking leave of this paper, we would notice the statement, that in three or four cases of want of union of fractured bone, depending on constitutional causes, Mr. B. Cooper "has found the administration of mercury, to an extent to produce ptyalism, of the greatest benefit; and has succeeded in producing consolidation, after seton, pressure, and other violent means had failed." It seems only necessary for its admirers to try this marvellous mineral in a new way, to ascertain its possession of a new virtue: it promotes absorption, or gives new energy to nutrition, lowers or excites vascular reaction, &c. &c., just at the will of the exhibitor; all depends on the *animus* with which it is given.

IV. *Cases of Hernia.* By Mr. Bransby Cooper.—The practical value of this paper atones for the somewhat unsatisfactory character of its predecessor. The patient referred to in the first case, aged sixty-eight, had for some years been the subject of right inguinal, and, for some months, of left inguinal hernia. He was suddenly seized

* The reader interested in the subject will find this point excellently discussed in the Cyclopædia of Practical Surgery. Art. Amputation, p. 129.

with abdominal pain, chiefly epigastric, not increased by pressure, and frequent vomiting, on the 26th of April; the bowels had been twice moved in the course of the day. Purgatives were ordered, and one stool procured the following morning; from this time, however, the symptoms of strangulation became progressively more evident; the patient had no further motion, the matters vomited acquired the quasi-fœcal aspect, and hiccup supervened, followed by death, in about eighty-four hours after the first symptoms were noticed. The inguinal regions had been attentively examined on the second day of the patient's illness, by Mr. Toulmin: the right, in which a truss had been worn for years, appeared unnaturally flat; in the left, a small hernia, reducible without the least difficulty, was easily discovered, and this side seemed otherwise unusually full, from an accumulation of fat. On the evening of the fourth day, Mr. Bransby Cooper found besides, by closer examination, that no suspicious condition of any kind existed in the inguinal region of the right side. The fullness of the left, was taken for evidence that the mischief lay in that direction; the inguinal canal was, in consequence, laid open, and found to contain a small empty sac, but nothing to account for the symptoms. The patient died the morning after the operation. On examination, a piece of intestine, as easily reducible as during life, was discovered in the sac on the right side; but, on attempting to draw the intestine out of the situation of the internal ring, some resistance was experienced, and, on enquiry, ascertained to depend on "a portion of intestine having become strangulated in a small hernial sac, situated anteriorly to the larger one, containing the reducible hernia." Now as the strangulated portion of intestine lay within the abdomen, it is manifest that an operation would not have led to the relief of the stricture, unless the large reducible hernia had been cut down on while within the scrotum. "It may be learned from this case," observes Mr. Cooper, "that when, after the reduction of a hernia by the taxis, the symptoms of strangulation remain unabated, the tumour should, if possible, be again made to protrude, and then be submitted to operation; as it is evident its reduction has not relieved the constriction of the implicated viscus." The general advisability of the practice here recommended, may not be very questionable; but as, under the circumstances supposed, no proof can be obtained that the obstruction resides in the portion of bowel connected with the sac, the surgeon must be exceedingly guarded in promising ultimate benefit from the proceeding. The case very strongly illustrates the propriety of examining into the exact state of a hernia, when the stricture has been divided external to the sac, before returning it into the cavity of the abdomen.

A second, and very similar case, which is next related, leads Mr. Cooper to the further conclusion, in respect of treatment, that if the hernia cannot be reproduced in the manner above referred to, "the outlet of the abdomen through which the protrusion had occurred, should be laid bare, and dilated, so as to facilitate the descent; or even, if necessary, so as to expose the hernial sac, and enable the operator to divide the stricture." The probable success of this manœuvre is less than that previously spoken of, as the operation is confessedly both dangerous and difficult,—nevertheless, we agree with Mr. Cooper, that it is better to give it a trial, than to consign a patient to inevitable dissolution. However, we should, most

indubitably, first put into practice the mode of treatment introduced by Mr. O'Brien, and which has apparently led to very fortunate results. To this important addition to surgical therapeutics, Mr. Cooper, strangely enough, does not advert.

v. *Case of Empyema and Pneumothorax; with Observations.* By Dr. Barlow.—This is a case remarkably confirmative of the doctrine advanced by Drs. Houghton and Stokes, in opposition to the opinions of Andral and Louis, that perforation of the lung, in tuberculous subjects, is not only not necessarily mortal, but may actually prolong life, and arrest, for a time, the constitutional symptoms of phthisis.

Anna G——, aged twenty-one, previously enjoying good health, according to her statement, was seized about the middle of 1835,* with pain in the left side, dyspnœa, and short dry cough, no marked febrile attack being recollected; at the commencement of 1836, she became unable to lie on the left side, and was conscious of a sensation like the splashing of water within the left side of the thorax; subsequently, the dyspnœa became extreme, her strength suddenly failed, and she expectorated copiously, *for the first and only time.* Seen, ten days after, by Dr. Barlow, she was found lying on the right side, respiration very hurried, pulse 120—130, cold sweat, profuse diarrhœa; her voice was reduced to a mere whisper. The physical signs were, on the *left* side, considerable dulness under the clavicle, sound more natural a little lower down, preternaturally clear below the inferior angle of the scapula; no respiratory murmur; amphoric sound audible, on deep inspiration, below the axilla, and over a considerable part of the posterior surface of this side; splashing sound heard distinctly, on succession: on the *right* side, doubtful dulness under the clavicle, resonance elsewhere good; respiration imperfect in the former situation, puerile in the latter. The heart appeared to be in its natural situation. The following year the patient had improved so much as to be able to walk about, the sound of splashing ceased to be audible, but the amphoric respiration persisted; the diarrhœa and sweats had been completely controlled, the pulse had sunk to 70. From this period until that of her death, which occurred on the 26th of February, 1839, the patient's condition underwent some slight change for the worse each winter, with a corresponding improvement in the summer. At the beginning of 1838, when the splashing sound became again perceptible, the impulse of a fluid in the left side of the chest was felt, by placing one hand on the scapula and the other under the clavicle, and briskly shaking the patient; the upper part of this side was now found to be preternaturally resonant, and in the month of September dislocation of the heart, to the right side, was discovered.

The body was examined twenty-four hours after death. The *left* side of the chest, lined with false membrane coated with curdy matter, contained about a pint of inodorous pus; at the upper part of this cavity lay the superior lobe of the lung, about half as large as an egg, and composed almost wholly of truncated fragments of bronchial tubes (some of them with open mouths) and tuberculous matter. The lower lobe, non-tuberculous, was firmly compressed against the bodies of the vertebræ.

* Dr. Barlow's dates are very loosely stated.

There were universal old pleuritic adhesions on the *right* side, with cavities in the upper lobe; the middle and inferior lobes nearly filled with tubercles, or gray granulations. The heart and aorta were healthy, but small; the former situated to the right of the middle line.

Dr. Barlow conceives that the unusual continuance of life in this case resulted from the suppression of the disease in the left lung, its slow progress in the right, and the diminution of constitutional irritation, which occurred subsequently to perforation. There can be little doubt of the correctness of this notion, for it is hardly more than an expression of facts; but the same degree of certainty does not attach to the inference that all these fortunate circumstances were the direct consequence of the establishment of hydropneumothorax by perforation of the lung. In the first place, it is exceedingly hypothetical to suppose that the condition of the left lung could have any influence in retarding the deposition of tuberculous matter in the right. Dr. B. presumes, with no little ingenuity, that the secretion of pus in the pleura may have acted in respect of the opposite organ in the manner of external counter-irritants; but there is no proof even that this secretion was constantly or habitually going forward. On the other hand, that the deposition of tuberculous matter in the lower left lobe may have been prevented by the pressure of the effused fluid, is highly probable: herein, therefore, the intercurrent pleuritic disease was a favorable condition for the patient's recovery. Thirdly, the obliteration of the diseased lung is considered by Dr. Barlow, in imitation of Dr. Stokes,* to have acted on the constitutional state of the subject, as the removal by art of a scrofulous joint; the recovery of flesh, strength, and health, which occasionally follows the latter operation, needs not to be dwelt on. This is a shrewd and plausible conjecture of the Irish pathologist, and in the present case derives support from the fact, that the *right* lung was very slightly affected at the time perforation of the *left* supervened. A somewhat analogous occurrence is occasionally, we may add, observed in the instance of external carcinomatous growths: in a fair proportion of the cases in which removal of the morbid structure has been followed by permanent cure, Nature, by setting up gangrene in the surrounding tissues, and enucleating the foreign mass, has been the successful operator.

There is a peculiarity in the physical phenomena of Dr. Barlow's case, to which that gentleman does not allude. When careful examination was first instituted, the sign of the presence of air was well marked at the *lower part* of the chest, while considerable dulness prevailed at the anterior *summit;* nor does the existence of tympanitic sound in the latter position, appear to have been established until two years afterwards. This apparent infringement of the laws of gravitation may be explained, by supposing the tuberculated lung to have been adherent at its summit to the thoracic parietes in the outset, while its gradual destruction by the progress of the tuberculous disease slowly effected its separation therefrom, and permitted the circulation of air around it. But objections may, we are aware, be raised against this conjecture; Dr. Barlow's details, unluckily, do not supply materials for a better.

The proportion of cases in which perforation causes death, during the acute stage of the consequent pleuritis,—or, on the contrary, exercises

* Diseases of the Chest, p. 531.

no apparently baneful influence on the health of the patient, is a point to be decided by the application of the numerical method. M. Andral has materially exaggerated its fatality, while Dr. Stokes appears to have fallen into the opposite error. Can the existence of a third class of cases, in which actual benefit has apparently accrued to the patient from the effusion of air into the chest, be admitted as evidence that Dr. Carson's idea of treating phthisis by inducing artificial pneumothorax, is not quite so infatuated as has generally been believed?

VII. *On the Physical Diagnosis of Incipient Phthisis.* By Dr. HUGHES. —VIII. *Observations on the Pulse in Phthisis Pulmonalis.* By Dr. GUY.—We have noticed these papers in the First Article of the present Number.

IX. *On the supposed Existence of Fluoric Acid as an Ingredient in certain Animal Matters.* By Dr. REES.—Dr. Rees, continuing his sagacious enquiries into the chemical constitution of bone, has arrived at a result subversive of the commonly received notions, founded on the analyses of Gay Lussac and Berzelius, respecting the presence of fluoruret of calcium in that and other animal substances. He is firmly convinced that no such salt exists either in recent ivory, the enamel of teeth, human bone, or urine,—that it should be expunged from the list of constituents of animal compounds, and that its presence in fossil ivory is the result of partial mineralization of that substance. The error of the continental chemists, he conjectures, must have originated from their using glass of inferior quality as a test for the fluoride. It appears that on distilling equal parts of sulphuric acid and water on bone earth, till the measure of water passes over, the latter is found to contain a proportion of phosphoric acid, and that on evaporating the fluid, the acid partially volatilizes with it: now it is well known that phosphoric acid, if heated to volatilization on glass of *inferior quality*, will corrode it with considerable energy.

X. *Observations on the Incubated Egg.* By Mr. JOSEPH TOWNE.—Mr. Towne relates some ingenious experiments and observations on certain phenomena of incubation. He conceives he has demonstrated the fallacy of the chief element in the received theory of the decarbonization of blood in the chick,—namely, the passage of atmospheric air through the shell. His first experiments on this point were made by investing the shell with four coats of paper and albumen, and watching the subsequent development of the chick, which, as he avers, underwent no perceptible modification. Additional security against error from permeability of the investing substance was obtained by using "five thicknesses of stout paper, and then adding three thick coats of oil-paint, composed of white lead, with a large portion of sugar of lead used as a drier: this I did, with the double intention of offering an additional obstruction to the air, and also to prove whether the paper was, or was not, entirely sufficient for this purpose; concluding that, if it were not, the fume from so noxious an application must inevitably prevent the progress of incubation. All this, however, had no effect: I opened the egg at twelve and a half days, and found that no interruption had occurred, the venous and arterial circulation being perfectly natural."

Mr. Towne further concludes, from these experiments, that the air

contained in the *folliculus aëris* is not used, as stated by Sir Everard Home, to aerate the embryo during the early stage of its development; they are, in this point of view, confirmed by the fact that the internal paries of the folliculus remained unruptured in the covered eggs. The following passage will allow the reader to judge of Mr. Towne's capability for physiological investigation.

"I next examined a number of eggs, to ascertain when the folliculus aëris becomes ruptured: and this is best done by dividing the shell with a sharp file, about one inch from the small end of the egg; because in this way it can be separated without fraction, and consequently without injury to the membranes: the chick may then be withdrawn, and the chorion removed with a pair of forceps: after which a large aperture should be made at the large end; and then, by gently blowing in, it will be evident whether the membrane be ruptured or not. I found this had not occurred on the eighteenth day; so that the air contained in this cavity would appear to have some other use than the one assigned to it by Sir Everard Home. I also observed, that up to this time the chick had not the power of using its vocal organs: but, on the nineteenth day, so soon as the forceps were introduced, it uttered a loud note of distress; and in this egg there was a large rupture in the membrane of the folliculus aëris. I believe, therefore, that the air which is contained within the egg is not rendered available for decarbonizing the blood until the nineteenth day: certain it is, that then the membrane is ruptured; and then it is that the chick first has the power of using its vocal organs. Now if it be considered, that at this period the chick has nearly reached maturity, and is soon to leave the shell in which it also leaves the chorion,—and that the large umbilical vessels, through which the blood has hitherto been conveyed to that membrane, are beginning to shrink, and have a dried appearance,—in short, that they are assuming a character which will allow of their division without loss of blood, and consequently are no longer capable of performing their original function—it will, I think, be sufficiently apparent why thus should be furnished a reservoir of air adequate to supply the lungs of the chick, until it has effected an opening through the shell."

We cannot continue this notice further than to add, that the remaining part of the paper exhibits much sagacity in the conception and interpretation of experiments; but however ingenious these may be, they obviously require to be repeated frequently, before the important conclusions now drawn from them can be looked on as fully established.

Some very exquisite coloured lithographs illustrate the text; and the models placed by Mr. Towne in the Museum at Guy's, representing the different stages of the process of incubation, are indeed *models* in their way.

XI. *Report of Primary Syphilitic Cases.* By Mr. ASTON KEY.—We postpone our examination of this paper, until its author has completed it.

XII. *On the Preservation of Subjects for Anatomical Purposes.* By Drs. BABINGTON and REES.—The object of this article is to recommend the injection of pyroxylic spirit as a preservative against the putrefaction of subjects intended for dissection. The special advantages of employing it are stated to be its extreme fluidity, its freedom from colour, its cheapness (it is however half as dear as alcohol), and its innocuous nature. The smell of subjects injected with this ingredient appears to be vastly disagreeable; a full description of it will be found in the Annals of Philosophy, New Series, viii. 69: it is perfectly distinct from pyroligneous acid and pyroacetic spirit.

Art. V.

Pathologische Untersuchungen. Von Dr. Henle, Prosector und Privatdocenten in Berlin.—*Berlin*, 1840. 8vo, pp. 274.
Pathological Investigations. By Dr. Henle.—*Berlin*, 1840.

We are rarely sanguine of finding much that is profitable in German works on Pathology; but if there be a writer in that country from whom more than from any other we should anticipate solid excellence in whatever subject he undertakes, it is Dr. Henle, the laborious and clever assistant of Professor Müller. Though young, his reputation in anatomical science is already among the highest in Europe, and the character of his annual account of the progress of pathology, in Müller's *Archiv*, is sufficient proof of his learning, if not of his knowledge in that department.

The work before us is the first of Dr. Henle's that we have seen in which excellence is alloyed. With much that exhibits an extensive theoretical knowledge of medicine, it contains much more that affords evidence of ignorance of its practice. But we will give, in a connected form, the substance of the first two and best of the four papers, whose subjects are Contagion and Sympathy.

1. *On Miasmata and Contagions, and on Miasmo-contagious Diseases.* The object of this paper is to establish a probable, if not certain, theory, that the material of contagion is composed of organic matter, and "not only an organic but a living matter, endowed with individual life, which stands to the diseased body in the relation of a parasitic organic being." (p. 15.) The chief grounds for this view are drawn from the phenomena of certain contagious diseases attacking the lower animals, especially those of the muscardine in the silkworm; and, from the connexion lately proved to exist between fermentation and the development and increase of infusoria and the minute algæ. With these as facts, and with some ingenious reasonings, the author endeavours to show how all the phenomena of those diseases, which he presumes commence from miasma and spread by contagion, including smallpox, measles, scarlatina, dysentery, cholera, typhus, plague, certain forms of coryza and catarrh, one kind of puerperal fever, and some others, may be explained by his theory.

For the organic nature (whether independently living or not) of the matter of contagion, the chief arguments he advances are these: 1st, The same epidemic will rage under all circumstances of atmospheric and other physical external conditions; and we are, therefore, obliged to assume for its cause some poisonous material mixed in the air, and capable of being carried about in it and separated from it. 2d, The organic nature of this material is rendered probable, by its power of increasing by the assimilation of foreign substances, a power which, so far as we know, is peculiar to organic beings. Fermentation is, indeed, often advanced as a proof of the contrary; but the observations of Cagniard-Latour and Schwann prove that it is itself only the decomposition of organic fluids effected by the agency of vegetables of the lowest class, which grow and increase at the expense of the azotic and saccharine materials in the fermenting liquid; and hence the analogies between

the mode of development of an infectious material and fermentation are stronger arguments for than against the organic and vital nature of the former. Thus, 3dly, the action of contagion is analogous to fermentation in that the quantity of the effect produced bears no relation to the quantity of the ferment employed; a circumstance depending, in the propagation of contagion as it does in fermentation and putrefaction, on the prolific power of the organic beings in each. 4th, The accurately typical course of the miasmo-contagious diseases is evidence of a periodical development, such as occurs only in organic beings. The whole course of these diseases is marked by peculiarities in regard to *time*, such as never occur in diseases from common causes, and such as show that the cause of the disease has itself a regular course of periodical development; a peculiarity known only in living beings. The continuance of an individual case may perhaps be compared with that of one generation of parasites; the continuance of an epidemic with that of a genus. One observes in organic mixtures that certain genera of infusoria appear, exist for a time, and then vanish and make way for new ones; one genus seeming to serve for the generation, or rather for the nourishment, of another; and in this there is a striking analogy to many epidemic diseases, which, having affected the body once, leave it for the rest of the time that they may last in the neighbourhood, or for the rest of life.

The matter of miasma (that is, of that which, generated out of the living body, produces a certain disease when applied to it,) is evidently identical with that of contagion, or of that which is separated from the living body that has been attacked by the miasma. "Contagion is, as it were, miasma in the second generation; a miasma which has passed through the first epoch of development within the living body." ....They may both be called *matter of infection;* and this "is constantly the same for each specific disease: it appears as contagion when its origin can be directly traced to a diseased body, and as miasma in the opposite case." The phenomena of the production of the matter of infection in diseases which are not contagious, are deducible as analogies to the same phenomena in contagion; and among the former, there are found, as evidence of the vitality of the poison in both, 1st, That miasmatic diseases, as agues, are endemic where there is a constant decomposition of organic substances going on, as near bogs, after floods, on board ships, &c.; but this decomposition is, in fact, nothing else than the change of organic matter produced by infusoria and the minute algæ; so that every putrefying body is, as it were, a breeding-place of these living beings; and where organic matter is thus exposed widely, and in great quantity, the whole atmosphere must be filled with their germs; 2d, That the same means which favour, or limit, or destroy the formation of these lowest living beings, also favour, limit, and destroy the action of infecting matter, as acetic acid, whose beneficial influence in hospital-gangrene is well known. Warmth and moisture also favour the development of both; oxygen is essential to the life of all organic beings, and miasmata are destroyed in irrespirable air; but the ova of infusoria, &c., may remain alive without oxygen, and so also contagion sometimes remains latent for years, without increasing or developing itself. In like manner, both infusoria and parasitic vegetables will

remain dry for years, and yet be capable of revival; and so will the matter of contagion. For the difference between merely miasmatic and miasmo-contagious diseases, it may be assumed, that in the latter the contagion can produce germs in the diseased body, in the former not.

But the theory is not all drawn from such analogies; one contagious disease, at least, is known to depend on the development of a vegetable parasite—the muscardine of the silkworm.* The characteristic signs of this disease first appear after the death of the worm, whose body becomes covered with a white powdery efflorescence, and then dries and mummifies. This efflorescence is a minute vegetable, according to Montagne, a species of botrytis, by the contact or inoculation of which the disease is communicated. The germs of this plant increase by the consumption of the body in which they are placed; after the death of the worm, they perforate its skin, and grow up the more, the warmer, moister, and more quiet the surrounding atmosphere is. They then gradually dry, and form a light powder containing their germs, which, on the least motion of the worm's body, is scattered in the air. The germs are so light and so numerous on a single individual, that they spread about with the greatest rapidity, adhering to bodies of all kinds, and remaining long suspended in the air. They retain their contagious power for as much as three years; and the ova of silkworms affected by the disease may communicate it to healthy worms, by the germs of the parasite that adhere to them. In how many thousand other ways the disease may be carried cannot be imagined. It is most severe in the spring, and its spreading is favoured by warm and dry weather; it chiefly attacks well-nourished worms, and the common disinfecting means are the best for putting a stop to it. The tissue in which the parasitic plants chiefly grow, is the pigment beneath the skin and the subcutaneous adipose substance; they spread from the point of inoculation by root-like growths, and by single globules separating and floating in the adjacent fluids, and perhaps passing into the blood.

It must be admitted that, in all these essential points, the muscardine is analogous to the miasmo-contagious diseases of the higher animals.

"Under favorable circumstances (in its case in mouldy moss) the cause of the disease forms independently, as miasma; with heat and dryness it becomes epidemic and contagious, and spreads further only by contagion. On its decrease, the epidemic diminishes, and its contagiousness ceases. Currents of air convey the contagion to great distances, so that the disease may break out at any place, as if from a fresh miasmatic origin. The contagion is thus at once aeriform and fixed, so that it may be inoculated; it is destroyed by the common disinfectants; it retains its power, when dry, for years; an imponderably and immeasurably small quantity of it is sufficient to develope the disease into the most virulent epidemic. The most vigorous worms suffer most, and form most fresh matter of contagion. Chemical remedies are useless, either for the prevention or cure of the disease." (p. 40.)

Nor is this the only disease of the kind that occurs in the lower animals. Ehrenberg has discovered a vegetation growing on fish, and

* The most important facts relating to this subject may be read in the Annales de Sciences Naturelles, tom. viii., ix.

generating disease in them. Henle has found vorticellæ growing on the toes of tritons kept in a glass vessel, destroying and producing a kind of gangrene of the parts (chiefly between the toes) to which they are attached. And Hannover has made exactly similar observations on another species of triton, and proved that the mould growing on them is capable of inoculation, and is distinctly contagious.*

It is admitted, that "we look round in vain, through the whole range of epidemic diseases in men and the higher animals, for an observation parallel to the above, to prove the similarity of a contagious matter to known animal or vegetable bodies." (p. 41.) But the difficulties of obtaining such observations are many, evident, and perhaps insuperable; so that the absence of such facts is not sufficient to negative the deductions from analogy and reasoning; and the theory may fairly claim reception, if it will stand the test of being applied in the explanation of the phenomena produced by a contagion operating in the body. "How, then, on the supposition of a *contagium animatum*, can the symptoms and course of miasmo-contagious diseases be explained?" (p. 20.)

The parasitic beings which constitute the matter of infection may be considered as belonging to the lowest and most minute, yet the most prolific, of creatures. By their action on the living body, inflammation or putrid decomposition is produced. Now, putrefaction is, in fact, not a mere decomposition of organic matter into its elements, but an alteration of it, effected by infusoria, as the vinous fermentation is a change of organic matter by the action of minute vegetables; and an evident formation of such parasites is often observed on the bodies of living insects, and even birds. Hence the supposition of parasitic beings as the material of contagion will hold in the cases of epidemics with putrid decomposition, as hospital gangrene.

"In general, the individual power of an organized part has a stronger affinity for the nutritive materials of the body than a foreign organization has; but that the latter may gain the ascendancy, is proved by the formation of entozoa. One may say, that while in common putrefaction the infusoria decompose organic matter already dead, in these cases it is at once killed and decomposed by the contagious parasites." (p. 22.)

In the acute miasmo-contagious diseases with inflammation of the membranes, and especially in the contagious exanthemata, as they are called, the disease may be supposed to commence with the reception of the parasites or their germs. They are received from without only on mucous membranes or injured parts of the skin, and often, on particular spots, varying according to the nature of the parasite, just as particular parts are liable to be infested by particular entozoa. It is remarkable that the mucous inflammations often occur without any cutaneous eruptions, but the latter are always preceded by the former, unless the disease be communicated by direct inoculation, and then the former are often altogether wanting.

In the first period of the disease, the parasites produce no evident symptoms; they either remain as yet undeveloped, or it requires some

* These observations, which were made subsequently to the writing of his book, are alluded to in the author's preface.

time for them to increase enough to have a sensible effect. In the next period, inflammation or putrefaction takes place; the latter (as in hospital gangrene, angina maligna, &c.) resulting, on the one hand, from the nature of the contagion, on the other, from the depressed constitution of the patient; the quantity of contagion has probably no material influence. (Many of the early symptoms are ascribed to reflex actions from the spinal cord, as the sneezing in measles, the cramps and spasms, the shivering, &c., which are produced according to the laws of sympathy, explained in the second paper.)

The inflammation which the contagion produces is more or less superficial, whether it affect the skin or mucous membrane. Its passage from the spot it first attacks may take place in three ways:

1st. By the parasites, as they increase, extending on the body, and passing along or under the cuticle. In the latter way, they may travel, as the inflammation usually does, from the mucous membranes to the skin, or, as it does more rarely, in the opposite direction. Perhaps the parasites travel especially towards the skin for freer access to the oxygen; and it is remarkable that all acute exanthemata which begin in the mucous membranes of the nose, mouth, and eyes, spread from the head towards the trunk; while in dysentery, of which the contagion acts chiefly on the lower bowels, the eruption when it breaks out generally commences on the abdomen. It would argue strongly for this view, if, after inoculation, the eruption always spread from the point of insertion; but this is not certain, though there are some facts in favour of it.

2dly. The inflammation may spread by sympathetic irritation of parts of the skin which the parasites have not yet reached, as in the itch the eruption spreads to parts in which no acari can be found. But this explanation will not hold for the cases in which every inflamed part of the skin contains contagion, as in smallpox.

3dly. The inflammation may spread through the medium of the blood, but this (though it cannot be fully disproved) is considered as highly improbable.

The fever and the other general symptoms may be referred to a twofold source: 1st, They may be considered consequences of the local inflammation, as in non-miasmatic inflammations; or, 2dly, as the result of the alteration of the blood or the organic substance on which the parasites have fed and increased. The blood must be altered, just as a fermenting liquid is, by the decomposition which the infusoria produces in it; and in many cases the blood is evidently changed both in physical and chemical properties. This alteration is probably the most influential cause of the fever; for it is more intense than the fever which arises from a common membranous inflammation of the same extent, and unless it is produced, the disease is not fully developed, nor is the patient secured against a second attack.

The disease ends favorably when the parasites cease to live or to grow, perhaps after they have produced contagious germs. The pustules and vesicles which may be formed (and which must be regarded not as producers of contagion, but as simple cutaneous inflammations, producing common pus, according to common pathological laws) open and discharge their fluids impregnated with contagious germs, or they dry up, and their scales, containing dry contagion, fall off. In this manner,

and in the desquamation of the skin, as in measles and scarlatina, the contagion must become mixed as dust in the atmosphere around the patient; or, if the exanthema were seated in the lungs or intestines, then the sputa or the excrements are impregnated with the products of the inflammation and the germs of the parasites contained in them, and thus with the close of the inflammation the disease may be terminated.

On the other hand, the fever and the other accidents which ensue on the action of the contagion, may continue after the latter has ceased to have any effect as a cause of them; they may continue as a common fever, or common suppuration, &c., and may produce a long convalescence, or death.

The metastases observed in exanthemata (when they are real and not merely the result either of local disease establishing itself in the general depression which the contagion produces, or of some other accidental complication,) may be explained either by the parasites being hindered from developing themselves externally, and therefore spreading more in the opposite direction, or by the loss of equilibrium which ensues when a preponderant excitement of any part is suddenly checked—a phenomenon which will be explained only when all the causes of the sympathetic connexion of parts are discovered.

Such is Dr. Henle's theory of animate contagion. Many of our readers are aware that the same subject was very ably treated by Dr. Holland,* in his Medical Notes and Reflections; and, as far as was possible, by lucid arguments, and by fair deductions from analogies, it was there shown that, for an explanation of the phenomena of cholera (selected as an example of the whole class of epidemics), no hypothesis was open to so few objections as that of minute insects infesting the body, and transmitted from one individual to another, through the medium of the atmosphere. Whoever adds to the arguments of that paper the facts of this, must confess that a strong case is now made out in favour of taking the direction which they indicate in future investigations of epidemics. The case stands simply thus: an analogy has long been observed between the fermentation of organic fluids and the influence of contagions on the blood; fermentation is closely connected with, if not caused by, the influence of parasitic animals and vegetables in the fluids in which it is going on; there is, therefore, a reasonable probability that the matter of contagion may be composed of parasitic beings or their ova; and this probability is strongly supported not only by its hypothesis fitting the phenomena better than any other yet imagined, but still more strongly by the fact of some contagious diseases in the lower animals being visibly the effects of parasites.

The defects of the hypothesis, however, are evident, though not desperate; the chief of them being the distance of the analogy of the diseases of worms or amphibia and of men, and the absence of any actual observation of such parasites as are supposed to constitute contagion in the bodies of the higher animals.

* Dr. Henle does not quote this part of Dr. Holland's work, though he has extracted facts from other portions of it. Probably the paper on contagion was printed before Dr. Holland's work reached Berlin.

The explanation of all the symptoms of contagious diseases, by the direct influence of the parasites, is quite unnecessary; and we think Henle has weakened his case by throwing this burden upon it. If the course of the epidemic, up to the period of its attacking the individual, and its very first symptoms, can be shown to depend on the development of the parasites, enough will be proved; after that time, other causes of symptoms, as the altered state of the blood, and the impression of the nervous system, must at once come into operation, and there are few morbid phenomena which these, with their long train of subordinate influences, are not amply sufficient to produce.

II. *On Nervous Sympathies.* This important and difficult enquiry is the subject of Dr. Henle's second paper. The phenomena are all arranged as illustrations of this proposition: "It is matter of observation, that nerves communicate their excitement one to another; and it is proved that this communication takes place (at least in the case of the animal nerves) only within the central organs. Communication of excitement in the central organs is the foundation of all nervous sympathies. The laws according to which excitement spreads in the central organs, are also the laws of (nervous) sympathy." (p. 83.)

It is impossible to imagine this communication of excitement, except as taking place from one point to another adjacent to it; nor is the distance of the parts that sympathize any hinderance to this view; for it may be that in the central organs nerves are near each other which diverge as they proceed to the periphery. Suppose the origins of two fibres, A and B, to lie near each other in the brain or spinal cord, and that one of these fibres goes to the right and the other to the left side of the body; the stimulus of the portion in which they originate must produce sensation or motion on both sides of the body. If a communication of excitement is effected between two such filaments in the central organ, the stimulus of A extends to B; and since sensitive nerves propagate their excitement centripetally, the effect is the same whether one be stimulated at its peripheral or at its central extremity, and the excitement of the peripheral end of A will, as in the preceding case, be communicated to B, and its influence will appear at its (B's) peripheral extremity; because, in whatever part of its length a nervous filament is stimulated, its excitement is evidenced at its peripheral end.

Were our knowledge of the arrangement of the origins of nerves in the nervous centres complete, sympathies were thus easily explained; but at present all that can safely be assumed is that the origins of the nerves are arranged *nearly* in the same series in which they come out from the brain and spinal cord, and that *in general* the nerves of adjacent parts of the body arise near one another in the central organs.

From a lengthened consideration of the functions of the sympathetic system of nerves, and an interesting comparison of the analogies of cellular tissue and organic muscles, the author concludes as follows:

"1st. The nerves may also communicate their excitability to one another within the ganglia, and thus the ganglia may be considered as in some degree central organs, and as media of sympathies. Since, however, all nerves

which are connected together by ganglia do not terminate in them, but are prolonged to the central organs, it is in most cases, when the brain and spinal cord are perfect, impossible to determine whether the communication is effected in one of them or in the ganglia. The ganglia must always, therefore, occupy a subordinate place in the enumeration of the sympathies, unless more be ascribed to them than is warranted by observation, or unless they are made to bear the burden of everything, which nothing else can explain. 2dly. The organic nerves are the motor nerves of the involuntary muscles, and perhaps of the cellular tissue and the vessels, and as such form a subdivision in the class of motor nerves." (p. 106.)

There are three directions in which excitement may be communicated in the spinal cord: 1st, in its breadth, from one nerve or column to the corresponding one on the opposite side; 2d, in its length along the same column to a similar nerve above or below; 3d, in its thickness from one column of one side to the other on the same side. To communication of excitement in one of these three ways, all sympathies may be referred.

1st. Symmetrical communication between the corresponding nerves of the two sides of the body. Instances of this are seen in the pain of a healthy tooth, when its fellow on the opposite side is painful and carious, and in the contraction of both pupils when strong light is applied to only one.

2d. Vertical communication in the same column. Almost every severe pain is felt around the part which is diseased; as that from a tooth over all the face, from a finger up the arm, &c. Strong light produces tickling in the nose; a disagreeable sound produces pain in the teeth and corrugation of the skin. (And so on, through a number of similar examples; but the facts that may be placed in each division of this excellent arrangement of sympathies, the reader must himself supply.)

3d. Antero-posterior communication; to which may be referred all the phenomena of reflex motion, in which motor nerves are excited by communication from the sensitive. Many of these are phenomena of healthy action, as the contractions of the pupils, the pharynx, &c.; but the reflex motions in the voluntary muscles usually occur only in abnormal states, as in narcotism, paralysis, &c. In this class also may be included not only the sympathies in which motions follow sensations, but those also in which increased secretion follows sensation, as in the flow of tears when the nose or eye is irritated, &c.

"What conditions favour the communication of nervous excitement in the central organs, and thereby the production of consensual excitement?" The extent of communication depends on, 1st, The strength of the stimulus; as Mr. Grainger has shown, the extent of reflex motion depends in all cases on the intensity of the stimulus;—2d, on the kind of stimulus; one blow, or prick, or other application of stimulus, does not so easily excite reflex motion as repeated slight irritations, &c.;—3d, on the excitability of the whole system, and the degree of vital power.

These observations relate rather exclusively to normal sympathies; and this portion of the paper presents, on the whole, by far the most rational account of the subject that we remember to have read; every illustration is apt, and every argument judicious; but in the rest, which treats of "Morbid Sympathies," the author is less happy, because less familiar with the facts of his subject.

The sympathies of the nerves, he says, may be anormal in two ways; either when the communication takes place more speedily or in a greater extent than in the healthy state, or when parts act in association which in general do not communicate their excitement to one another.

"A. Anormally increased consensus. 1st. Since communication is the more sure and more extended, the more violent the stimulus; and since, with equal external influences, the effect of the stimulus is the greater, the greater the excitability; therefore increased excitability of the sensitive nerve, which is first stimulated, must favour the diffusion of the excitement." (p. 121.) Thus, for example, in congestion and inflammation a common degree of warmth produces a burning sensation, the skin being in a state of exalted excitement; and again, in inflammations, many of the common reflex actions are greatly increased in intensity, as the violent contraction of the muscles of the eyelids, when an inflamed eye is exposed to the light, the spasmodic contraction of the bladder, when (though the urine is unaltered) the mucous lining is inflamed, &c.

2d. "The communication must be more easy and more extended when the primarily stimulated nerves have their normal excitability or excitement, but the nerves associated with them by contiguity have an exalted excitability," (p. 122;) for the first are to the second in the relation of an external stimulating substance. An instance of this morbid state is found in the impotence that arises from increased irritability of the seminal vesicles, so that after the slightest irritation emission takes place. Hence also, in general irradiation from any point of the nervous system, the associated excitement is especially great in the muscular or sensitive nerves which are already morbidly excited; as patients with diseased lungs cough more from the same slight irritation than healthy persons, &c.

3d. The same effect occurs in certain conditions, not merely of increased excitability of a particular set of nerves, but where the whole nervous system is thus altered. Thus, among healthy individuals, the nerves seem to be more closely connected with each other in women, and in those of a sanguine temperament, than in others; and to this may be referred in disease all the phenomena of nervous excitability, irritable debility, or erethism, &c. In this category may be placed also the over-excitement produced by watching, by narcotics, &c., in which hallucinations and spontaneous cramps occur; here, too, come tetanus, and the condition produced by excessive loss of blood: in all these cases, the increased communication of excitement is only the consequence of the generally increased excitability. Another series of similar phenomena are seen in those sympathies which occur in the reflex motions of paralyzed bodies and during sleep, in which the author thinks that the cramps, contractions, and exalted sympathies do not depend, as Dr. Marshall Hall (in our opinion more correctly) supposes, from the mere want of the influence of the brain, but from an organic alteration of the spinal cord, and commonly from the same as that on which the paralysis depends. A third series of these phenomena of generally increased communication from generally increased excitability, are found in the greater ease and extent of reaction upon the same

stimuli, when the mind is intently occupied, as in starting when, being closely occupied in some mental effort, a sudden noise is heard, or a sudden contact felt.

From all these considerations, the author concludes that "there is no peculiar organic disposition to sympathies, but that the diffusion of associations depends only on the strength of the stimulus and the degree of excitability." (p. 133.)

B. Acquired abnormal or individual sympathies, in opposition to the congenital, normal, and general, are those which are established between parts, the origins of whose nerves do not stand in direct anatomical connexion. In these cases it appears as if the associated stimulus, instead of passing from one nerve to certain others, as in common sympathy and in the healthy state, or instead of passing, as in diseased excitability or unusual excitation, beyond its natural limits, or over the whole system, passed as if by leaps from this to that point, missing others; and it seems as if disease, habit, or idiosyncrasy could open new ways for communication in the central organs. These uncommon sympathies are seen when one part of the nervous system is preeminently excitable; and so in a general excitement its affection is most prominent, and it appears as if it were in sympathy with all parts of the organization. This part is what the older pathologists called *pars minoris resistentiæ*. Hence, when a tooth is carious, anything unusual may produce toothach, whether heat or cold, or errors in diet. These sympathies are seen also in the reactions that take place after peculiar affections of the senses. In the impressions on our senses, we feel ourselves excited not only in a greater or less degree, but also in a certain manner, or by a certain quality; this quality of the sensation excites ideas, and these again sensations or motions; and these influences of the mind appear like sympathies following the peculiar impressions.

Antagonism is the next subject considered:

"When any part of the skin is stimulated, the reaction, congestion, and inflammation extend over a greater or less portion of the surrounding skin; this reaction is sympathetic. When, therefore, fresh inflammation is excited in the neighbourhood of an inflamed part, the first is sympathetically increased. But if a stimulus producing congestion or inflammation is applied at a certain distance from the affected part, the reaction in the latter is lessened, and this decrease is effected by *antagonism*. Sympathy and antagonism are both consequences of a connexion between the stimulated parts. According to the violence of the stimulus and the excitability, an increased excitement diffuses itself in a definite circuit; but as with respect to time excitability is in general destroyed by stimulus, and the more rapidly the greater the portion of the body stimulated, so also, with respect to space, an exaltation of excitement in one part produces a decrease of it in another. .... And in general, the nearer the antagonist and derivative excitant is applied to the diseased part, the more efficient it is, provided only it is not placed so near that a sympathetic affection may ensue." (p. 138.)

The central organs are in like manner subject to be affected by the connexion of parts, sometimes by sympathetic and sometimes by antagonist excitement, though it is scarcely possible to say under what circumstances each is observed; but in general it is observable that certain

parts of the nervous system are so connected, that when the excitement of one is increased, that of the other is also; that in others the excitement of one part sometimes exalts and sometimes lowers that of the other; and that, lastly, under certain circumstances, the connexion of parts is always shown by antagonism.

The evidences of this antagonism are seen in the common processes of counter-irritation and derivation, whether the antagonist phenomena be merely pain, or other simple nervous excitement, or inflammations of adjacent parts; but the applications which Henle makes of the principle of antagonism are shown in the parts which follow his account of the "sympathies of the organic nervous system." (p. 142.)

He believes, from many and some good reasons, that the cellular tissue of the skin and blood-vessels has an energy analogous to that of the muscles, and since this *tone* of the cellular tissue may be altered by various circumstances, he asks, May not such alterations be also induced indirectly by sympathy? May they not depend, like the motions of the muscles, on an altered state of the sensitive, or, to speak more generally, the centripetal nerves? Facts are not wanting to prove a connexion between the sensitive and the organic nerves; the flow of tears on stimulating the nose or conjunctiva, and many others such, are sufficient. And in these it is usual to consider the increased secretion as a sign of increased activity, and it is regarded as the result of sympathy between the organic and the sensitive nerves. But the vessels of the gland, during increased secretion, are probably enlarged and relaxed; and this dilatation can only be the consequence of their decreased excitement; and thus it would appear, that the increased secretion, following the excitement of sensitive nerves, is not sympathetic but antagonistic, an increased excitement of centripetal nerves producing a decreased tone of the cellular tissue. And Henle now sets himself to show how this theory will adapt itself better to the various phenomena of inflammation, &c., than the old one of increased action and exalted tone.

It is in the first place evident, he says, that the nerves of the involuntary muscles are sometimes in an antagonist state to those of the voluntary, as in the tympanites and retention of urine, which often accompany the spasms of the voluntary muscles in hysteria, and in other like cases. If we next consider the cellular tissue under the idea that its contraction corresponds to increased, and its relaxation to decreased excitement of the organic nerves, we find it sometimes in sympathetic, sometimes in antagonistic connexion with the animal nerves. *Sympathetically*, the dartos contracts in tenesmus, the nipples become erect when the skin around them is rubbed, &c. *Antagonistically*, the joints feel loose in the hysterical, goose-skin often coincides with paralysis, &c.

The contraction of the cellular tissue on applying cold, and its relaxation on applying heat, also appear antagonistic, because, for many reasons, we must believe that cold depresses the excitement of the sensitive nerves, and that heat exalts it. In all these changes, to which cellular tissue is liable, the blood-vessels participate by their external cellular and contractile coat; their tone is expressed in the varying turgor of parts; if their turgor is increased, they contract, and secretion diminishes; if it is decreased, they dilate, and redness, swelling, and

congestion, with increased secretion, are produced. Now whether the vessels can be sympathetically excited with the animal nerves is uncertain; but that they are antagonistically, is evident from many facts, as in the sweating of exercised parts, the flow of saliva on working the muscles of the mouth, &c.

The cases of consent between the sensitive nerves and the nerves of the vessels, are divisible into two series:

1st. Excitement (antagonistic) of the nerves of the vessels from internal causes, as shown by the increased secretion, œdema, redness, &c., which are often seen coincidently with neuralgia, in which the sensitive nerves, having their excitement exalted, that of the organic nerves is depressed, and the cellular tissue of the vessels is dilated. The same connexion is shown in the ease with which parts, whose sensitive nerves are already highly excited, become inflamed.

2d. Excitement of the sensitive nerves from external, chemical, or mechanical stimuli, in which the antagonism of the organic nerves of the vessels is always shown by the congestion and inflammation that ensue. From this fact, the increased fullness of the vessels that always follows the pain in any part, Henle finds himself, "almost unexpectedly, on the way" to the ultimate object of all of which we have made the preceding abstract, "a theory of inflammation."

"Congestion and inflammation depend for their proximate causes on relaxation of the capillaries, and this is antagonistically produced by stimulus of the centripetal nerves." (p. 152.) It will be sufficient if we present the most favorable illustrations of this theory, those drawn from "the prototype of inflammation, that which is traumatic." Here all the cardinal symptoms result from the primary affection (excitement) of the sensitive nerves; the pain, directly, as it is in fact always the first, and the redness and swelling from the antagonistic paralysis of the vessels. The objective heat also depends, as is now unquestionable, upon the nerves, either directly or through the medium of nutrition. Thus, for inflammation there are two factors, increased excitement of the sensitive nerves, and dilatation of the vessels; and these are produced the one by the other.

Now since, according to this supposition, the cause of all the objective symptoms (the heat excepted) is atony of the vessels, which, in true inflammation, is antagonistic, but which may occur primarily, it is evident that redness, swelling, ulceration, and gangrene, but without pain and heat, must take place whenever the nerves of the capillaries are primarily paralyzed. And that these symptoms do occur, in such circumstances, is clear from Magendie's observations of the sloughing of the cornea after division of the trigeminus, the frequent ulcers, &c., in paralytic limbs, the gangrene in limbs in which the ischiatic nerve has been divided, &c.

As we have already said, the first part of this paper affords the best arrangement we have yet read of the various nervous sympathies, and clearly indicates the laws to which a great number of the bizarre collection of facts, once heaped together under that name, are referable. But as the author approaches more nearly to his theory of inflammation, his excellence rapidly diminishes. To the theory itself there appear

two chief objections; that it is founded on too many assumptions, and opposed to too many facts. Thus, there are assumed for it—the probability that the coats of capillaries have a tone or contractility; that this is governed by the organic motor nerves; that these are capable of antagonistic association (itself a relation, which is in any case only probable) with centripetal nerves; that the result of their depressed excitement would be dilatation of the capillaries; and that the result of this would be increased secretion or inflammation. Now, even if we granted a fair share of probability to each of these assumptions, it is yet certain, from the common and calculable laws of probable events, that the chances are at least 1000 to 1 against the hypothesis composed from them all, containing the expression of the true proximate cause of inflammation. And against this small chance many facts may be set; as (to mention but a few), that if this antagonism followed the common laws of nervous association, the intensity of inflammations should bear a direct proportion to the intensity of pain precedent or coincident, whereas many diseases have great pain without inflammation, and many inflammation without pain, and in none is there any constant relation between the two; that, again, persons or parts with the least tone of cellular tissue should be subject to the most severe inflammation; that, were this hypothesis true, local bleedings should do harm by increasing the excitement of centripetal, and decreasing that of motor nerves, and so on.

The author does not recover himself in the rest of the work. The other papers, "*On the Course and Periodicity of Disease*," and "*On Fever*," are most strange productions. Choosing for consideration only those subjects which are least illustrated by facts, the author raises difficulties hitherto unimagined, as if only to make confusion of their ruins when he has thrown them down again; but we threaded the maze of words in vain to find one explanation of subjects which, though always obscure, never before seemed so densely dark.

It is singular, that a person who, like Dr. Henle, is so well acquainted with the difficulties of investigating one branch of science, should imagine that all the uncertainties in another far more difficult branch, may be disposed of by endeavouring to adapt a plausible theory to ideas which have as yet scarcely the semblance of facts. No one is more cautious than he of receiving, as *fact*, a microscopic observation; and yet he gives credence to assertions in medicine which are scarcely more certain than matters of popular belief. As an *observer*, however, these papers do him no discredit; he has only committed the errors of believing it possible to write correctly on medicine without personal observation, and of building loftier theories on loose assertions, than even the solid truths of demonstrative science could safely bear.

ART. VI.

Traité des Etudes Médicales, ou de la Manière d'étudier et d'enseigner la Médecine. Par E. FRED. DUBOIS (d'Amiens), Professeur agrégé à la Faculté de Medecine de Paris, &c. &c.—*Bruxelles*, 1838. 12mo, pp. 460.

A Treatise on Medical Study, or on the Method of Studying and of Teaching Medicine. By E. FRED. DUBOIS (of Amiens.)

IT can scarcely, we think, be doubted that important changes will take place, within the next few years, in the character of our Medical Faculties, and in the qualifications which will be required from those who seek to enter the ranks of our profession. We feel equally confident that, in whatever changes are effected, *one additional* demand will be made—*a sound preliminary education.* Our reason for assured belief on this point is the universal feeling of its necessity among those bodies who have publicly expressed their opinions by petitions or resolutions. Whatever differences there may be in the plan of the building which is to be erected, all are agreed that it should have this deep and solid foundation. We can scarcely, then, invite the attention of our readers to any topic of more immediate importance than the question—What constitutes a sound preliminary education for him who intends entering upon medical study? This question we propose now to discuss. We shall have to revert, in so doing, to the fundamental principles of all education; and to consider the utility of various branches of knowledge to the growing mind. In taking such a survey, it may appear as if we were quitting our own sphere, and entering upon a range of topics which we have no right to treat of. But nothing can be really foreign to our province which concerns the right training of those to whom our profession is to look for the future maintenance of its dignity and the elevation of its scientific character. Moreover, the *principles* which should guide the purely medical instructor are, or ought to be, derived from those on which education should be conducted from earliest youth. As to readiness for acquiring new information, even the seniors amongst us need aspire after no higher title than that of "children of a larger growth;" and, whilst the varying character of the mind at different epochs is kept in view, the fundamental *principles of education* must be the same for the child and the gray-haired sage, for the man of letters and the physical philosopher.

We believe it to be a most important principle in physiology, which has hitherto received but little attention, that the lower the degree of organization, the more is the character of the being susceptible of influence from external sources. The simpler plants and animals have been found to produce germs capable of being developed into a great variety of forms, according to the conditions under which they live and grow; so great a variety, that naturalists have created, not only species and genera, but families and orders, among beings which really sprang from a common origin. The resemblance between the embryos of higher beings and the permanent forms of the lower is now universally acknowledged; and the resemblance is in no respect more striking than this. It is during the *early* period of the development of the organism that external influences have the greatest effect upon its form and character; and

changes may be induced in it at that time, which, at a later period, it would either resist, or entirely succumb to. We shall take another opportunity of enforcing this principle, as one of great importance in physiology. At present we merely bring it forward in illustration of the principle with which we shall start, as lying at the foundation of the educational training of the mind—that it is at the period of the *first development* of each faculty, that it may be most advantageously encouraged, guided, or repressed, according to the strength and tendency which it then manifests.

This principle is by no means new. It was enforced by Bacon in his noble treatise on the Advancement of Learning, to which we shall frequently refer in the present article. "As the wronging or cherishing of seeds or young plants," he remarks, "is that that is most important to their thriving; so the culture or manurance of minds in youth hath such a forcible, though unseen, operation, as hardly any length of time or contention of labour can countervail it afterwards." He was fully alive to the influence of different studies upon the mind; and their consequent value as means of cultivating faculties which might be deficient, or repressing tendencies of injurious strength. "There is no defect in the faculties intellectual but seemeth to have a proper cure contained in some studies; as, for example, if a child be bird-witted, that is, hath not the faculty of attention, the mathematics giveth a remedy thereto; for in them, if the wit be caught away but a moment, one is new to begin; and as sciences have a propriety towards faculties for cure and help, so faculties or powers have a sympathy towards sciences, for excellency or speedy profiting; and therefore it is an enquiry of great wisdom what kinds of wits or natures are most proper for what sciences."

Before entering upon this last question, however, we must pause to enquire what are the real objects of education. We shall confine ourselves at present to the cultivation of the *intellect*;—not overlooking the relative bearing of various branches of knowledge upon the moral feelings, but considering this influence as subordinate to other modes of training more peculiarly applicable to them. That no amount of intellectual attainments can afford a guarantee for the moral rectitude of their possessor, we have a melancholy proof in the profound philosopher from whose writings we have just quoted. But in the choice of the means of intellectual education, we may justly give a preference to those which exercise a beneficial influence on the moral sentiments, over those which have no such tendency. A comprehensive view of the objects of education ought to include the consideration of the best mode of preparing the mind, not only for active participation in the duties assigned to man, in his present state of being, but for entrance on a nobler and purer existence. But as, by the wise ordination of the Creator, the greatest intellectual satisfaction and moral happiness result from that course which is most fitted to discipline or educate man for eternity, the two objects may be regarded as coincident; and, for obvious reasons, we shall direct our present attention to the former only.

In the education of the intellectual powers, then, two chief ends are to be kept in view: *first*, the most advantageous development of these powers themselves; and *second*, the communication of the greatest amount of knowledge capable of being brought into useful application.

Neither of these should be adopted to the exclusion of the other. The system of education still pursued in many of our public seminaries professedly aims at the first only. In some, the study of the classics is relied on as the sole means of accomplishing it; and youths are brought up to make extempore Latin verses, and compose with fluency in Greek, with a profound ignorance of every other branch of knowledge, excepting such as may have been fortunately instilled into them at an earlier period. Against such a system we need scarcely contend; since the class of readers we are addressing must have long since renounced it. Even the addition of pure mathematics does not, we think, furnish a sufficient corrective of its injurious effects. That this study is most valuable, when judiciously conducted, as a means of intellectual discipline, we most readily admit; and we shall presently advert more in detail to the influence it is capable of exerting. But if the pupil is left entirely ignorant, as he too often is, of its *practical* applications, its utility is grievously restricted. A tendency is now gaining ground among a large class of intelligent persons in this country, to regard as the *principal* that which we consider the *subordinate* object of early education. It is thought that the communication of practical knowledge may be itself the instrument, and a sufficient one, of intellectual cultivation. But we are not prepared to go this length; although, as we shall presently show, such a course of instruction may be made to exercise almost all the intellectual faculties in an advantageous manner.

Education may be regarded as either general or special. By *special* education we of course mean the preparation of the mind for some particular employments, which are to be its chief occupation for the remainder of life. Now in *this* department, the acquisition of the greatest amount of knowledge, and of the power of practically applying it, is manifestly the chief end. No one would set a young man to the practice of medicine or law, however well developed might be his intellectual powers, or however large an amount of general knowledge he might have attained, without instructing him in the particular branch which he is to pursue. But it is evident that these instructions will be more efficient, in proportion to the *previous* cultivation of the intellectual faculties, and the *previous* amount of well-digested knowledge, on the foundation of which the newly-communicated ideas may be orderly arranged and firmly upreared. Our opinion, then, is, that *general* education ought to be directed especially to the *first* of the objects we have named; and *special* education to the *second;* but in neither case exclusively. We shall endeavour to show that general education may be so directed, as not only to prepare the intellect for the most advantageous employment of its powers, in the department which may be subsequently chosen for its exercise, but to store up an amount of practical information, which will be useful in almost every subsequent pursuit. It will be obvious that, of two systems equally efficacious for the first object, *that* will be preferable which goes furthest towards the second. And, if our principles be correct, the preliminary or general education will be nearly the same, whatever is to be the ultimate destination of the individual.

We shall now briefly consider some of the branches of knowledge usually included in the early training of the young, in their relations to

the development of the intellectual powers, and to their own prospective utility. In doing so, we shall derive many excellent hints from the volume whose title we have placed at the head of our article. With a good deal of French diffuseness of style, and a little of French dogmatism of opinion, this work has much of that sound practical character which marks the other productions of M. Dubois. It is evidently the result of much thought; and coming, as it does, from one who holds an elevated position in the Parisian School of Medicine, we recommend it to the attention of all those who are interesting themselves in the subject of medical education in this country. With his leading principle, that all education ought to proceed from what is general to what is special, we fully coincide. It is the course pointed out to us by many natural analogies; and it is that which is in soundest harmony with the varying powers and wants of man at different periods of his existence. We shall now trace the application of this principle.

The mind of the infant, on its entrance into the world, has been compared by some to a sheet of white paper, on which the educator may impress anything he pleases. We by no means coincide with this doctrine; since it supposes that there is no variety of mental *capability*, at this period, amongst different individuals; but, still, there is much truth in it. We are convinced that the intellectual education of the infant should begin with its moral training; and that both these are closely connected, in the early years of infancy, with attention to its physical development. Nothing, for example, is more prejudicial to the future temper of the child than the habit, so common amongst indulgent mothers, of putting it to the breast as often as it cries. The habit of indulgence once commenced is seldom or never discontinued; and the evil extends itself into every part of the character. But such a matter is rather foreign to our present purpose; and we have referred to it only in illustration of our principle, that the influence exercised on the powers and tendencies of the mind, at the period of their first development, is more important than any which can be put in operation at a subsequent period. Now, in directing our attention to intellectual education, we notice that the first-developed powers of the infant's mind are those connected with *observation*. And the rationale of this is obvious. The intellect at that period may be compared to a seed. It is in a state of dormant capability. It possesses faculties which need to be called into activity; and as warmth, moisture, and oxygen arouse the energies of the germ, and cause it to exert those powers with which it is endowed, so as to develope itself into its perfect form, so do external objects operate on the mind. If all inlets to sensation were closed, we can scarcely imagine that any use could be made of its faculties. It would remain like the seed buried deeply beneath the ground. But as soon as it receives impressions from external objects, and ideas are excited within it, a series of changes or operations commences, which can only terminate with the loss of its powers.

The cultivation of the observing faculties, then, is the first branch of education in the order of time; and it is that which has the most general relation with all others. In no pursuit, no situation in life, will the habit of acute and correct observation be valueless; and we are persuaded that to the medical student much subsequent labour would be saved, if

he were earlier taught to have not only *his eyes* but *his ears about him.* The habit can scarcely be commenced too early. "An infant, intently gazing upon an object, or examining it with its little hands and lips, is as usefully employed in the cultivation of intellect as the fondest parent can wish. In the early periods of mental culture more is, however, to be done, in this connexion, by allowing a child full scope for its own efforts, than by any direct exertions which can be made by others. When its attention is fixed upon any object, let it remain so; if possible, let the objects of sense be brought into view under different aspects, and exposed to the examination of different senses. Before words become to a child the signs of voluntary action, all that can be done is to expose it to sensations, and to allow them to fix the attention; but afterwards, more direct efforts may be made, and the attention may be fixed by various other means, beside the mere action of the sensations themselves. .... It is a most erroneous idea in education that nothing is done except when children are engaged in the usual rudiments of instruction. A child watching the motions of objects around, observing their figure and sounds, examining their structure, is employed in a work which it should be our aim, as much as possible, to aid and encourage, and from which we may expect very valuable results, both on the faculties and furniture of the mind."* In the essay from which we have been quoting will be found many valuable remarks on the proper discipline of the observing faculties, and on the distinction between quickness of sensation and quickness of perception. Those who possess acute sensibility are often deficient in that power of attention which is required for correct observation; whilst, on the other hand, those whose reflective powers are cultivated too exclusively are often so immersed in abstractions as to give no heed to the impressions of external objects. Each of these tendencies may be detected in early childhood; and the efforts of the instructor should be directed to their due regulation. With regard to the extensive, the universal operation of the habit of observation, we may add the judicious remarks of the same author. "The successful acquisition of every science which depends upon experiment, indeed, the acquisition of knowledge of every kind which depends upon the exercise of the perceptive power, the cultivation of the taste, the common concerns of life, the intercourses of civility, and the efforts of benevolence, require the constant exercise of this habit. Whatever method is found to invigorate and correct the observation should be frequently made use of. Till the understanding has made considerable progress, this should be made a leading object in the intellectual culture; and in every period of it, the habit should be frequently brought into exercise. By a proper cultivation of it, the memory and judgment are directly cultivated; and while it strengthens and rouses the energy of the mind, it furnishes it with some of the most serviceable materials for the understanding and imagination."

Next, in generality of character, to the formation of ideas by the impressions of the objects themselves is the acquisition of them through the medium of *language*. This power it is, therefore, the object of the instructor to convey at as early a period as possible. A clear and correct

* Rees's Cyclopædia; Art., *Intellectual Education.*

understanding of the force of words is of the utmost consequence in every period of the intellectual progress. On the care which is early taken in furthering this object depend, in a great measure, the future development of the understanding and the acquisition of knowledge. The tendency of many systems of education has been to familiarize the pupil rather with words than with things; and whilst we guard against this, by cultivating the observing faculties, not only in childhood, but throughout the period of youth, we should also take especial care that *definite* ideas be attached to the words employed. A simple method of attaining both these purposes, is to require from the pupil minute verbal descriptions of objects to which his attention has been directed; and in so doing, we shall be training him up in a habit which is itself of great utility. Too often, as soon as the study of language is commenced, we see the child's attention withdrawn from the world around him, and directed only to his books; and hence it is that medical students need so much time and training to learn "how to observe." On the other hand, if the study of language be not systematically pursued, we rarely find that definiteness and propriety in the use of words, which are necessary alike for the communication of knowledge to others, and for the reception of it ourselves. A deficiency in this respect is evident in the writings of many self-taught men. Take, for instance, the works of Hunter: and if he, instead of learning from the great book of nature, had devoted himself to the acquisition and condensation of the opinions of others, we venture to say that (until his mind had been disciplined by such a process,) his language would have been even more obscure than it is. Every one knows how many fierce controversies have taken place on abstract subjects, which have depended only on a slight difference in the meaning attached to words; and a mere vagueness of expression will often be a serious obstacle to the advancement of truth, by preventing the secure employment of the fact or opinion stated, as a basis for others. This is particularly the case in those departments of physiology which bear upon metaphysics, as we have formerly taken occasion to show.

M. Dubois lays great stress on this department of preliminary education; and, we think, with justice. There must, he remarks, be a complete harmony between the teacher and his pupils in regard to the use of language. They should be able to follow, without difficulty, all his trains of thought, and to become thorough partakers in his ideas; placing their feet, as it were, in the footsteps of him who is advancing in their front. This they cannot do, if they are incapable of appreciating his instructions, from want of aptitude in comprehending the force of language; and we have known many instances, in which a lecturer, who has succeeded in carrying along with him, through an abstruse discussion, a considerable proportion of his class, has been misunderstood by some, whilst others have given up all idea of following him, in consequence of such a deficiency. That mere plain English will not always convey the speaker's or author's meaning so well as some fanciful almost absurd, expression, newly coined, perhaps, from his own brain, who that has read the Essays of Elia will not acknowledge? For ourselves, we seldom think it accordant with our critical dignity to depart from the forms of language; but the lecturer may often have recourse to an exuberance of illustration, or pointedness of expression, which it might not

become the author to adopt. We believe, then, that felicity in the use of language is a habit which may, like acuteness of observation, be most advantageously cultivated in very early life; and that, if so cultivated, the best foundation will be laid for the subsequent discipline of the higher intellectual powers, and for the attainment of knowledge on any subject.

We are disposed to agree with M. Dubois, that the acquisition of a foreign language, of almost any nation, is the best means of making us well acquainted with our own; and, further, that the study of the classics has an advantage, in this respect, over that of any modern tongue. Moreover, to the scientific man, some amount of classical knowledge is almost essential to the object we have just been adverting to—the ready comprehension of the meaning of terms. That the learned in all ages should have agreed to base their nomenclature upon the languages most universally understood, cannot be wondered at; and we do not perceive how the system can be advantageously changed. No one can tell how difficult it is to communicate knowledge on some subjects (e. g. Natural History,) to those who are ignorant of the classics, and consequently of the import of the words derived from them, until he has tried the experiment, as we have. But we have a strong impression that, for those whose taste does not lead them to the study of the higher classic authors, the acquisition of modern languages will serve nearly the same purpose, as a means of intellectual discipline, and will be of far more practical utility.

Now that we are upon this topic, we may briefly state our opinion of the value of classical study, as a branch of general education, and therefore of that training which should precede entrance on medical study, and which should be tested by preliminary examination. We think that no other *single pursuit* can exercise the same beneficial influence on the mind, by cultivating so large a portion of its faculties in an advantageous manner; and that none is so well adapted to *indiscriminate* application in large establishments, from its simultaneous action on many distinct powers and tendencies of the intellect. Command of the attention, ready and capacious memory, exact and delicate discrimination, judgment in the application of principles and in the choice of words, a sort of indescribable aptitude in the conception of things as a whole and in their details, the power of realising abstract notions, a facility in combining simple and in analyzing complex ideas—these are but a few of the objects whose fulfilment may be reasonably anticipated from a well-conducted course of classical study; and a youth thus trained will go forth into the world with many advantages over him who has received only a scientific education, besides those which result from the cultivation of the taste and imagination. But these advantages are rather in the discipline which his mind has received than in the knowledge with which it has been stored; and the youth thus trained must not forget that his education, though of a higher order, has advanced less far in preparing him for the business of the world than if his attention had been, from the first, directed towards objects of more practical utility. If he make good use of his time and opportunities, he will generally make rapid progress in the attainment of the latter, unless the early cultivation of his observing faculties have been much neglected. But he will have to educate himself, as it were, over again. It is not a little remarkable that a large proportion of those who have most distinguished themselves in extending

the boundaries of human knowledge, have either been almost self-taught, or, if at school, have been reputed dull boys; and the exceptions have been principally among those who have chanced to be placed under an instructor of more than ordinary enlightenment and sagacity; whilst, on the other hand, it is a common remark, that few of those who distinguish themselves in scholastic exercises are subsequently heard much of by the public. The first class men and senior wranglers in our universities, after enjoying a short-lived reputation amongst a limited circle, too often feel satisfied with what they have done; and, considering their past acquirements as the end and not the means, allow their mental powers to fall into disuse, at the period of their greatest vigour, or exercise them only upon unprofitable subjects. It is not only in our own country that this may be observed. *Les héros du College*, as M. Dubois calls them, seem to be similarly inert in France; and, in his opinion, as in ours, the system must be faulty which produces such results.

For the reasons we have above given, we fully accord with the practice pursued by most of our medical boards—of obtaining proof of the candidate's knowledge of the Latin language; and we are not disposed to quarrel with the College of Physicians for requiring Greek also. But we think that this knowledge should be tested at a much earlier period than that of the medical examination; and the regulation of the London University appears to us a very judicious one. We have known cases in which individuals have gone through a regular course of classical study, and have derived great benefit from its discipline; and who yet, from natural deficiency of literary taste, and from thorough devotion to the acquirement of professional and scientific knowledge, may have so far forgotten, in a few years, the languages they had acquired, as to need much preparation for undergoing an examination in them. If it be acknowledged that their chief use is the preparation of the intellect for being advantageously employed on other subjects, the proper time for examining into the qualifications of the student in this respect, is when he is just entering upon his new pursuits; not when he has completed his medical curriculum, and been devoting weeks or months to *get up* as much Latin or Greek as will enable him *to pass*, which he forgets again as speedily. We have met with young men who were more afraid of being rejected for their Latin at Apothecaries' Hall (this was before it was made, as now, a separate examination,) than for ignorance of any other department. Is not this a practical satire upon the system? If they were well informed upon professional subjects, what benefit would they derive from the *cramming* process, which enabled them, by means of interlinear translations and other such aids, to master as much of Celsus and Gregory as would enable them to put on a bold front before the worshipful examiners? The object of this and of every other requirement comprised in the preliminary examination of the University of London, we take to be twofold: first, to secure such a *preparedness* of mind, on the part of the student, as may enable him to enter with advantage on medical study; and second, to ensure his being able to mix in the world with such an amount of general knowledge as should be possessed by every man practising a liberal profession. On neither of these grounds do we think the requirements in the least too high; in fact, we should be very glad to see them in some departments a little more stringent.

We shall now return to the point at which we diverged from our regular path to consider the benefits of classical study; and shall enquire into the best means of cultivating those reasoning faculties which so rapidly unfold themselves in childhood, when early preparation has been made for their exercise by the culture of the perceptive powers. The direct training of a judicious instructor we believe to be more effectual in guiding the development of these powers than any course of study can be. The phenomena of the external world may be made the subjects, not only of observation, but of inferential processes; and the habit of employing the reasoning powers upon these can scarcely be too early commenced. At first, the inferences to which the pupil is led, must be of the simplest character; they may gradually be rendered more complex. Among the methods of cultivating the purely reasoning faculties, none is, perhaps, so available as that which exercises them upon the simple relations of *number*, *time*, and *space*. Although these ideas are for the most part abstract, yet they are so immediately connected with sensorial impressions, that they are in general readily comprehended. When the fundamental notions are once established, the reasoning processes based upon them may be carried to almost any extent. It will be seen that, in placing the acquisition of these ideas next in order to that of words, we are following out the principle we formerly laid down; since they are the *most general* of any of the more precise notions connected with external objects; and the simple reasoning processes, founded upon them, are the best that could be devised for the exercise of the mind in that stage of development.

Many persons feel a difficulty in comparing numerical or mathematical reasoning with that of any other kind whose results are less certain. The difference is simply this. In the former, the premises are so simple and restricted, that the mind, feeling that no disturbing causes *can* affect the conclusion, assents to it with *certainty*. At every step of a calculation or demonstration, (supposing it, of course, to have been correctly performed,) the mind is as well satisfied of truth as at the commencement. The highest knowledge is thus derived, by inferential reasoning, from the axioms and definitions on which the simplest conclusions are at first erected; and the mind, which has pursued the enquiry throughout, and given its full assent at every step, feels as thorough a conviction of the final truth as of the fundamental propositions. In that kind of inferential reasoning, on the other hand, which forms a part of what is called *moral evidence*, the premises are not so restricted, and it seldom happens that the mind can exclude the *possibility* of a contrary result. Each step is, therefore, attended with a greater or less amount of doubt; and when many consecutive inferences are drawn, the probability of error increases in a very rapid ratio. We may illustrate this by the process of the surveyor, in laying down an extensive country. He first measures a base line, which is but short, in comparison with the distances he is afterwards to calculate. He then makes this one side of a triangle, whose angles he measures, and whose other sides he can thus calculate. These sides he makes the bases of other triangles, and carries on this process to any required extent. Now it is curious that, in such a train of reasoning, there should be a mathematical certainty with regard to the reasoning itself, and a moral probability only as to the data upon

which it is founded. Any error in the measurement of the base line will affect the whole series of calculations; and, if there be any imperfection in the instrument by which the angles are measured, a new error will be created at each stage, so that the final result will be very wide of the truth. Here, the mind will give full assent to the reasoning itself, provided it can feel assured of the data. It is worth notice, in regard to the perfection to which processes of this kind have been carried, that in some of the extensive government surveys in this country and in India, the *base of verification*, measured at the conclusion, has differed from the calculated distance by not more than a few inches, when the triangulation has been carried over hundreds of miles.

We can scarcely imagine any intellectual discipline more adapted than mathematical study to develope and direct a certain section of the powers of the understanding; but their deficiency lies in their limited scope. "The pure mathematics," says Bacon, most truly, "do remedy and cure many defects in the wit and faculties intellectual. For if the wit be dull, they sharpen it; if too wandering, they fix it; if too inherent in the sense, they abstract it." "If there were nothing valuable in the mathematical sciences," observes another excellent writer, "for the uses of human life, yet they are well worth our study; for by perpetual examples, they teach us to conceive with clearness, to connect our ideas in a train of dependence, to reason with strength and demonstration, and to distinguish between truth and falsehood. Something of these sciences should be studied by every one; and that, as Mr. Locke expresses it, not so much to make us mathematicians as to make us reasonable creatures." But it must not be forgotten that, in the exclusive pursuit of this study, the powers of observation, of discrimination, and judgment, are almost all neglected; so that the mere mathematician comes into the world with a much less advantageous preparation than the mere classic. By being conversant with truths of an obvious and simple character only, "he does not learn that kind of caution and severe examination, which are required in other sciences—for enabling us to judge whether the statements on which we proceed are true, and whether they include the whole truth which ought to enter into the investigation. He thus acquires a habit of too great facility in the admission of data or premises, which is the part of every investigation which the physical or mental enquirer scrutinizes with the most anxious care,—and too great confidence in the mere force of reasoning, without adequate attention to the previous processes of investigation, on which all reasoning must be founded. It has been accordingly remarked, by Mr. Stewart, and other accurate observers of intellectual character, that mere mathematicians are apt to be exceedingly credulous, in regard both to opinions and matters of testimony."*

To remedy what is injurious, and to supply what is deficient in mathematical study, we regard the study of the physical sciences as most admirably adapted. In the more exact among them, such as mechanics and astronomy, the close reasoning of the mathematics is applied to phenomena which are the objects of sense. The perceptive faculties are, therefore, exercised in observing these; the powers of discrimination and

* Abercrombie on the Intellectual Powers. Fifth Edition, p. 259.

comparison are employed in classifying them according to their analogy or dissimilarity; and the habit of generalizing with caution and certainty is acquired. The converse process of deduction cannot be better learned than by tracing the complex influence of the simple principle of gravitation, or rather mutual attraction, among the various bodies which form the solar system.

There is one influence of the study of astronomy, well pointed out by Sir J. Herschel, which is most valuable to all, and to none more so than the medical student. "In entering upon any scientific pursuit, one of the student's first endeavours ought to be to prepare his mind for the reception of truth, by dismissing, or at least loosening his hold on, all such crude and hastily-adopted notions respecting the objects and relations he is about to examine, as may tend to embarrass or mislead him; and to strengthen himself, by something of an effort and a resolve, for the unprejudiced admission of any conclusion which shall appear to be supported by careful observation and logical argument, even should it prove of a nature adverse to notions he may have previously formed for himself, or taken up, without examination, on the credit of others. Such an effort is, in fact, a commencement of that intellectual discipline, which forms one of the most important ends of all science. It is the first movement of approach towards that state of mental purity which alone can fit us for a full and steady perception of moral beauty, as well as physical adaptation. It is the 'euphrasy and rue' with which we must purge our sight before we can receive and contemplate, as they are the lineaments of truth and nature. There is no science which, more than astronomy, stands in need of such a preparation, or draws more largely on that intellectual liberality, which is ready to adopt whatever is demonstrated, or concede whatever is rendered highly probable, however new and uncommon the points of view may be, in which objects the most familiar may thereby become placed. Almost all its conclusions stand in open and striking contradiction with those of superficial and vulgar observation, and, with what appears to every one, until he has understood and weighed the proofs to the contrary, the most positive evidence of the senses."*

We are unable to account for the very general neglect of the physical sciences in early education. To us it seems the most natural thing possible, to show to the pupil the practical applications of the abstract notions of number, space, and time, and of the processes by which he builds upon these, at the very period when he is undergoing this discipline. What, for example, can be simpler than to make a child understand the properties of the lever at the time when he is learning the rule of proportion? Simple sums may, by an easy process, be worked mechanically; and thus the numerical and physical notions will be fixed in the mind together, and will strengthen each other. In the same manner, when the pupil learns to *square* a number, he may be *shown* that similar areas are to each other as the squares of their linear dimensions;†

* Treatise on Astronomy, p. 1.

† A very curious and apposite illustration of the importance of this kind of knowledge to the physiologist may be found in the erroneous statement, which has been copied from one work to another, that the capacity of the arterial tubes increases in proportion

and, by a very simple mechanical contrivance, the fact that the intensity of light, attraction, &c., diminish, as the squares of the distances increase, may be rendered obvious to him. In like manner, we would make what practical applications are possible, at every stage of the pupil's progress in more abstract investigations; and, in this way, not only will the intellectual discipline be more complete, but a fund of useful knowledge will be laid up. A young person thus trained will be prepared to enter with advantage on investigations of a much higher and more complex nature, when others must commence a previous discipline, which, at that late period, will be comparatively painful, and perhaps inefficient.

We are very glad to perceive that the principle on which we have thus been dwelling has met with so able and influential an advocate as Professor Daniell. Near the commencement of his very excellent Introduction to the Study of Chemical Philosophy, we find the following remarks. "There are two great mistakes, which are commonly committed by those who enter upon a systematic course of physical enquiry; and not only by those who are commencing, but by those who undertake to direct such studies: the first is, the neglecting to form a proper connexion with previously-acquired knowledge,—the under-valuing the results of their ordinary experience as parts of the system, as the first rounds of the intellectual ladder by which they aspire to scale the loftier heights of philosophy; and the second is, the substitution of names for things,—the vague acquirement of certain terms, certain forms of expression, instead of a real understanding of objects and principles to which they have been applied—'terms of ignorance and of superficial contemplation,' as Lord Bacon calls them. The process may be repulsive to the too-common self-sufficiency of imperfect knowledge; but when invited to reflect and reason upon the simple observations of childhood, (simple indeed, but not more easy than those which he will be called upon progressively to make,) the student ought to feel no more offence than when, in the outset of his geometrical studies, he is referred to the axioms or self-evident truths, that 'things equal to the same are equal to one another,' and 'the whole is greater than its parts.' It is from the known that he must ascend to the unknown; and it is all-important that he make his footing sure, and miss no step by the way."

This last observation we hold to be of great importance. The learner

to their distance from the heart; it being supposed that at each bifurcation, the capacity of the branches is greater than that of the parent trunk. This notion is manifestly inconsistent with the facts ascertained by Poisseuille, who has shown that the pressure of the blood against the sides of the vessels is almost exactly the same in every part of the body, instead of diminishing, as it would do, with the increased capacity of the tubes. The cause of the error has been recently explained by Mr. Ferneley, (Med. Gaz., Dec. 7, 1839.) It is true that the sum of the *diameters* of the branches is considerably greater than that of the trunk. Thus a trunk 7 lines across may divide into two branches of 5 lines each, or a trunk of 17 into three branches of 10, 10, and $9\frac{1}{2}$; but when their areas are compared, the correspondence is as close as can be reasonably expected, when the nature of the measurements is taken into account. In the first case, the area of the trunk is represented by the square of 7, that is, 49; whilst the area of each branch will be 25, and the sum of the two will be 50. In the second instance, the area of the trunk will be 17 squared, or 289, whilst that of the branches is the sum of 100, 100, and $90\frac{1}{2}$, making $290\frac{1}{2}$. Physiologists are much indebted to Mr. Ferneley for making this correction. Like the problem of Columbus's egg, the thing is simple enough when explained.

should, if possible, have nothing to unlearn. Every one knows how much easier it is for truth to find admittance into a mind unoccupied than into one prejudiced by the residence of error. In our progress through the sciences, therefore, we should pass from what is certain to what is uncertain, from what is simple to what is complex; and we shall find that this is exactly what we do in commencing with what is general, and pursuing our enquiries into what is special. For example, medicine is one of the most special of all the sciences, and at the same time the most uncertain; it concerns only the abnormal or disordered vital actions of one species of living beings. Pathology and physiology are, therefore, but branches of a higher science—that of the vital actions in general. But man is only one out of many hundred thousand species of living beings with which this globe is peopled; and whatever may be the difference in practical value between the physiology and pathology of his system and those of the zoophyte or plant, in a merely scientific point of view they are on a level. Medicine, then, is but a small section of the vast science of biology. Now, although organized beings have actions peculiar to themselves, and governed by their own laws, they are still subject to the more general laws of matter. It may, indeed, be argued, with little possibility of error, that what are called *vital* properties are originally inherent in matter, and only developed or caused to manifest themselves when that matter is, by the process of organization, placed in the circumstances required for their operation; just as iron, when brought within the influence of a magnet, itself shows magnetical properties which previously lay dormant. If this be true, the science of organized matter is itself a section or branch of that of matter in general. We find, on a little consideration, that matter possesses certain universal properties, and others restricted to particular forms of it. Thus the mutual attraction of its masses is never absent; it is common to every form of matter, and operates under all circumstances. It is probable that all matter possesses electrical properties, but these are manifested only under peculiar circumstances; and we do not, therefore, regard it as unphilosophical to suppose that all matter may possess what we term vital properties, which are only manifested under circumstances still more restricted. However this may be, it is certain that living organized beings are subject to *all* the other laws of matter; and that we cannot investigate their strictly *vital* properties with any hope of more than limited success, until we are in a condition to exclude all the operations of more general causes.

Hence, then, it is obviously important, that he who aspires after something more than the power of applying practically the knowledge which others have attained of the structure and actions of the human body, who aims at giving to medicine a more certain character, and thus raising it from the opprobrium in which it lies at present, should prepare himself by a command of those general principles to which the laws of life are but subordinate. The profound remarks of Bacon are applicable to this as to every department of knowledge: "After the distribution of particular arts and sciences, men have abandoned universality or philosophia prima, which cannot but cease and stop all progression; for no perfect discovery can be made upon a flat or level; neither is it pos-

sible to discover the more remote and deeper parts of any science, if you stand but upon the level of the same science, and ascend not unto a higher science." Of this *philosophia prima* he elsewhere says, "But because the distributions and partitions of knowledge are not like several lines that meet in one angle, and so touch but in a point, but are like the branches of a tree that meet in a stem, which hath a dimension and quantity of entireness and continuance, before it come to discontinue and break itself into arms and boughs; therefore it is good, before we enter into the former distribution, to erect and constitute one universal science, by the name of philosophia prima, primitive or summary philosophy, as the main or common way, before we come where the ways part and divide themselves."

By this *philosophia prima*, or *scientia scientiarum*, as Bacon elsewhere terms it, we understand those principles which should be our guide in the pursuit of any kind of knowledge that can be brought under the domain of general laws. They depend upon the relation between the mind of man and his position as "minister et interpres naturæ." They are nowhere more clearly set forth than in the admirable "Introduction to the Study of Natural Philosophy," by Sir J. Herschel; a work which we should rejoice to see made a text-book in the higher classes of our schools and colleges. But, alas, the day which shall behold Terence and Aristophanes put aside for Herschel or Somerville is not yet come. When every student of medicine shall be thus prepared, we do indeed anticipate the rapid advance of our science.

These principles of philosophising cannot be taught in any method so advantageous as that which we have already suggested as the best suited to the general wants of the student's mind. With the early foundation we have recommended, he will have little difficulty (always supposing him to have the advantage of an instructor who can properly adapt his particular plan to the mind of his pupil) in making himself acquainted with those general laws of matter, at rest and in motion, which govern the phenomena comprehended in the sciences of statics and dynamics. He will thus have a few comprehensive ideas, which may be pursued into greater or less detail, as his time and inclination permit, in those subordinate branches which are commonly spoken of as mechanics, astronomy, hydrostatics, hydraulics, and pneumatics. In such a course, he would gradually perceive that he was coming into a new domain, and that in passing from solids to liquids, and from liquids to aeriform bodies, the operation of the laws which he at first deemed certain is modified in a way he cannot altogether comprehend; and thus he is led to the conception of new forces resulting from the operation of properties which he has not been hitherto led to perceive. Of these, the most important is heat, which is the chief cause of these differences in form; and he accordingly applies himself to the examination of its properties and laws.

In regard to heat, light, electricity, and magnetism, the opinions of philosophers are at present undergoing a very remarkable transition. These were formerly described as *imponderable* forms of matter; but the advance of knowledge and a sounder mode of philosophising have led many to discard this opinion, which may be looked upon as a remnant

of the ancient dogmas; and to attribute the phenomena so designated to properties of matter whose laws are to be investigated like those of any other forces. The knowledge of the true nature of sound, and the almost complete demonstration of the analogous character of light, have mainly contributed to the relinquishment of these "personified abstractions;" and, when they are so disposed of, we hope that the vital principle will go along with them. When the forces which produce the phenomena separately referrible to heat, light, electricity, attraction, &c., have been investigated by the student, he will be in a condition to enter upon the science of chemistry, whose phenomena are due to the combined action of these forces. This is a science which we regard as peculiarly adapted to exercise the intellectual powers. The observing faculties are constantly and actively employed; the mind is habituated to the analysis of complex phenomena, and to the search for the true cause of each change out of many possible ones; and reasoning processes of a high order are concerned in the more abstract departments of the science. Its obvious practical utility causes it, as might be expected, to be one of the most popular branches of knowledge amongst those who derive little intellectual gratification from its pursuit. It is now taught in a large proportion of the well-conducted seminaries, both for male and female education, in Scotland; and it is gradually making its way southwards. We believe that to Dr. D. B. Reid's labours this very useful change is principally due.

We are of opinion that, if the pupil be well grounded in the principles of chemistry, whilst passing through his preliminary course of education, a great deal of time may be spared which is now devoted to this science as a branch of medical education. Having got a general knowledge of the science, he will require nothing further than a special knowledge of those departments of it which bear peculiarly upon medicine. Of these, pharmaceutical chemistry may very properly be included with materia medica; whilst some additional knowledge of organic chemistry may be communicated in the courses of animal and vegetable physiology. The student of forensic medicine ought to be conducted through a regular course of practical analysis, for the detection of poisons; but we are disposed to doubt whether so minute a knowledge on this subject need be diffused among the general body of the profession as some imagine; or whether such investigations may not be better conducted by some person especially skilled in them, such as the lecturer on chemistry or forensic medicine in the nearest medical school. To this question, however, and others strictly connected with medical education, we shall return in our next number.

From chemistry the student may advantageously proceed to mineralogy; on which, however, we would not have him dwell long. He should make himself acquainted with the external characters and chemical composition of the minerals which principally compose the crust of the earth; and he will then be prepared to enter upon the study of geology, some knowledge of which now may be regarded as essential to every professional man who wishes to be regarded as a well-informed and liberally-educated gentleman. Here, too, as in chemistry, the science itself makes abundant demands upon the perceptive and rea-

soning powers, and advantageously exercises almost all the intellectual faculties. As in astronomy, too, there is in the outset a necessity for the absolute dismissal of prejudice, and a determination to submit the mind wholly to the dominion of truth. There is no science which affords so much scope for ingenious speculation; and none in which new phenomena are so constantly arising to confirm or overthrow the hypotheses which have been invented to account for the facts already known. This exercise of mind is extremely advantageous to every one, and especially so to him who is preparing to enter upon the study of medicine. A facility in framing hypotheses, "if attended with an equal facility in laying them aside when they have served their turn, is one of the most valuable qualities a philosopher can possess; while, on the other hand, a bigoted adherence to them, or indeed to peculiar views of any kind, in opposition to the tenor of facts as they arise, is the bane of all philosophy."*

We do not think that a preliminary education can be regarded as complete, which does not include some knowledge of natural history and general physiology. As means of intellectual discipline, we have a very high opinion of them; and there are probably no branches of knowledge that afford, on a very slight acquaintance, so many sources of rational pleasure and moral improvement. We have known many, who received but a trifling amount of early instruction on these subjects, resort to them in after life as their most refreshing and delightful recreation. It seems to us a neglect of the means of instruction placed by the Creator within our reach, if the young are brought up in ignorance of the teachings of the volume of nature. On the advantages of such preliminary instruction to the medical student, we need scarcely now dwell. Such a general acquaintance as he will thus gain with zoology, is the best foundation he can have for the more extended pursuit of comparative anatomy; and upon the latter all certain physiology must be based. We do not see that any higher knowledge of botany is needed by him than he would gain at the same period, except that merely technical acquaintance with the characters of medicinal plants which he should learn from the course of materia medica. But we are again forestalling our future discussion of these subjects.

It may be thought that the range of study we have thus suggested is by far too extensive for ordinary students. Whether it is so or not, however, will depend mainly upon the method in which they are taught. If a knowledge of *facts* is that which the instructor seeks to communicate, the attention had better be confined within narrower limits; if, on the other hand, he deems his pupil ready for the reception of *principles*, his scope can scarcely be too extensive. It is a remarkable characteristic of the present state of almost all the sciences, that while the facts are being multiplied with unprecedented rapidity, the principles are advancing as rapidly; and this not by the uprearing of showy and unstable hypotheses, but by that sure process of rigid induction which not only lays one course upon another, but binds them all more and more firmly together. The aptitude of the pupil for the reception of

* Herschel's Preliminary Discourse, p. 204.

principles will depend very much upon the early training to which he has been subject. If this have been such as we have suggested, and there be no unusual deficiency of mental power, we feel assured that there are few who are not capable of understanding them, and appreciating their value, if they are put forwards in their most simple form, and illustrated in modes sufficiently simple and various. Even if the student be detained until the age of seventeen or eighteen years, in order to pass through such a course, instead of commencing his medical studies at fifteen or sixteen, we should think the time well bestowed in the greater preparedness of mind which he will possess for entering upon the special study of his profession.

The following extract from a valuable essay to which we have already referred expresses in a striking and concise form the advantages which may be expected from a connected course of study in natural science: "Another source of the utility of the mathematics is their subserviency to natural philosophy. To describe the phenomena of the universe, to investigate their causes, and the connexion of these causes, are the principal objects of this science. To mention these objects is nearly all which is necessary to indicate its valuable effects on the mind. The habits of accurate and persevering observation, of investigation, of abstraction, and of correct reasoning, are more or less produced and cultivated by the study of the philosophy of nature. It furnishes abundant scope for the most sublime speculations, and calls forth the noblest exercises of the imagination, yet restrains the mind within the limits of reality. It carries us beyond the boundaries of sense, and lessens our interest in self by increasing our concern with everything around us. It enlarges the comprehension of the soul; for it offers for contemplation the laws of the universe. It prepares the student for an acquaintance with the human mind; for the strictness with which its investigations are conducted prevents that wildness of theorizing which is the bane of science; and forms the habit of cautiously attending to phenomena, in order to ascertain the general laws which regulate them. It aids the cause of religion; for it accustoms the mind to seek for the causes of observed appearances, and leads it from design and regularity to infer an intelligent First Cause."*

To these remarks we shall add, that such a course of study will tend to cultivate that philosophic spirit which is so peculiarly desirable for the medical practitioner; a spirit which, as Dr. T. Brown justly remarked, "is more valuable than any limited attainments in philosophy, and the cultivation of which, therefore, is the most precious advantage that can be derived from the lessons and studies of many academic years; a spirit which is quick to pursue whatever is within reach of human intellect, but which is not less quick to discern the bounds that limit every human enquiry, and which, therefore, in seeking much, seeks only what man may learn; which knows how to distinguish what is just in itself from what is merely accredited by illustrious names; adopting a truth which no one has sanctioned, and rejecting an error of which all approve, with the same calmness as if no judgment were opposed to its

* Rees's Cyclopædia; Art. Intellectual Education.

own; but which, at the same time, alive with congenial feeling to every intellectual excellence, and candid to the weakness from which no excellence is wholly privileged, can dissent and confute without triumph, as it admires without envy; applauding gladly whatever is worthy of applause in a rival system, and venerating the veiy genius which it demonstrates to have erred."

The mind which is imbued with the principles of general science has a far greater chance of success in any particular branch than that which has directed all its energies to one restricted department alone. When this preparation has been made, there can be no question that the more closely the attention is bent on any one point, the more probably will that point be gained. But much fruitless labour will often be thrown away, and many errors committed, if this be neglected. "It can hardly be pressed forcibly enough on the attention of the student of nature," observes Sir J. Herschel, "that there is scarcely any natural phenomenon which can be fully and completely explained in all its circumstances, without a union of several, perhaps of all, the sciences." Whatever walk of science the enquirer may pursue, he will find it well to be guided not only by the light of his own intellect, but by the reflection of that light from surrounding objects. The higher we rise in philosophical generalization, the more do the principles of the different sciences become interwoven with each other; so that phenomena which at first sight appear to have nothing in common are found to be intimately connected. The rules of investigation and the general method of conducting it, are, or ought to be, the same in all sciences; and he who has fully entered into them in one department will have but little difficulty in following them through others, especially when he is conducted gradually from the simple to the complex.

There is an intellectual satisfaction in the possession of such a range of acquirements, which strongly contrasts with the spirit which characterizes the mind of the trader in science. The latter views every extension of the boundaries of knowledge with anxiety; since it occasions him fresh labour, or diminishes the value of his former ones. The less his acquirements reward him in and for themselves, the larger remuneration does he crave from others. The irksome, the insignificant in his employment press him to the earth, because he cannot oppose to them that high and cheerful courage which accompanies only a knowledge of the real value of his pursuit, and a stedfast devotion to it. Just as sedulously as he severs his own peculiar science from all others, does the true philosopher strive to extend its dominion and restore its connexion with them. New discoveries in the field of his activity are to him a source of pure and intense gratification. Perhaps they fill a chasm which the growth of his ideas had rendered more wide and unseemly; or they place the last stone, the only one wanting to the completion of the structure of his ideas. But even should they produce an opposite effect,—should a new series of truths, a new aspect of nature, a newly-discovered law, overthrow the whole fabric of his knowledge, *he has always loved truth better than his system*, and gladly will he exchange her old and defective form for a new and fairer one—

Impavidum ferient ruinæ.

It is not among the least of the advantages to be expected from such a course of preparatory study as that which we have been recommending, that he who has been judiciously conducted through it is likely to enter upon the study of medicine with sounder notions regarding the value of knowledge, and upon the practice of his profession with more elevated views in regard to its application. We should hear less of those angry bickerings which arise from mercenary jealousy, and from disappointed ambition,—men would be more ready to act like brethren in one noble community, when the question is not who shall amass the greatest fortune, but who shall contribute most, by the extension of the boundaries of his science or by adding to the knowledge of that which it includes, to the welfare of his kind.

Let us once more turn to the pages of the father of the inductive philosophy, and seek his estimate of knowledge: "But the greatest error of all the rest is the mistaking or misplacing of the last or farthest end of knowledge: for men have entered into a desire of learning and knowledge, sometimes upon a natural curiosity and inquisitive appetite; sometimes to entertain their minds with variety and delight; sometimes for ornament and reputation; and sometimes to enable them to victory of wit and contradiction; and seldom sincerely *to give a true account of their gift of reason to the benefit and use of men*: as if there were sought in knowledge a couch whereon to rest a searching and restless spirit; or a terrace for a wandering and variable mind to walk up and down with a fair prospect; or a tower of state, for a proud mind to raise itself upon; or a fort or commanding ground, for strife and contention; or a shop, for profit or sale; and not a rich storehouse, for the glory of the Creator, and the relief of man's estate. But this is that which will indeed dignify and exalt knowledge, if contemplation and action may be more nearly and straitly conjoined and united together than they have been. Howbeit, I do not mean, when I speak of use and action, that end before mentioned of the applying of knowledge to lucre and profession; for I am not ignorant how much that diverteth and interrupteth the prosecution and advancement of knowledge, like unto the golden ball thrown before Atalanta, which, while she goeth aside and stoopeth to pick up, the race is hindered. Neither is my meaning, as was spoken of Socrates, to call philosophy down from heaven to converse upon the earth; that is, to leave natural philosophy aside, and to apply knowledge only to manners and policy. But as both heaven and earth do conspire and contribute to the use and benefit of man, so the end ought to be, from both philosophies to separate and reject vain speculations, and whatsoever is empty and void, and to preserve and augment whatsoever is solid and fruitful; that knowledge may not be, as a courtezan, for pleasure and vanity only, or, as a bondwoman, to acquire and gain to her master's use; but, as a spouse, for generation, fruit, and comfort."

We fear that the *cui bono?* is a question much oftener asked, particularly in this country, than is consistent with the true dignity or end of knowledge. We seldom hear in Britain of men devoting their whole lives and powers to the improvement of the scientific departments of medicine, content to exchange the worldly reward that they might de-

rive from its practice as an art, for the happiness they derive from the pursuit itself, and from the consciousness of a fame well earned by the success of their labours. We would it were in England as in Germany; and that the place of such a man in society were estimated by his intellectual wealth, and not by the house in which he resides and the dinners which he gives. We are not without hope that such a spirit is extending. We shall augur well for science when it is universal.

It may be urged that the prevalence of such a taste as we have been endeavouring to excite will lead too many to desert the practice of their profession for the pursuit of its theory. We do not think so. It is a beautiful instance of the adaptation between the mental constitution of man and the world in which he is placed, that for every variety of occupation there are found some minds which take pleasure in it. It is not every one whose capabilities encourage the hope of success in the pursuit of the higher departments of knowledge; and yet those who are thus deficient may have a peculiar aptitude for carrying into practical operation the principles which have been attained by others. As long as there are diseases to be cured, will there be found men ready to undertake the employment, as certainly as others who feel a distaste for it. How those who devote their energies to the philosophy of medicine, and have no independent means of support, are to be maintained, is a grave and difficult question. It may be thought that either the state or the profession at large should afford assistance to such as have attained a certain degree of eminence; but whether such assistance should be direct, or whether it should not be afforded by throwing open to general competition those situations which are now more frequently disposed of by private interest, may be fairly doubted. We shall not at present enter upon this discussion; but we may take an opportunity of adverting to it when we enter more fully, as we propose to do in a succeeding article, into the *Connexion of the Medical Sciences.*

When some acquaintance with physical science is possessed by *every* medical practitioner, there will, perhaps, be less difficulty in combining attention to practice with some amount of philosophical enquiry, than at present exists. There is a prepossession in the public mind against those who have a scientific reputation, and in favour of those who are known for nothing but their devotion to practice; even though the unemployed hours of the latter may be spent in an unprofitable manner. It will often happen that the man of scientific education will, from his habits of mind, be able to see at a glance that which the mere practitioner discovers after a long and blundering examination. He will be able to perceive where principles can be applied, and where he must trust to experience alone; and he will more certainly judge of the results which may be expected from treatment, and of what may be more safely left to take its own course. In the analysis of difficult phenomena, the explanation of perplexing symptoms, it is that the scientifically-educated physician has the chief superiority over his more empirical colleague; and the advantage of the early culture of physical science, in cherishing those powers of mind which are required for this purpose, and training them to work in unison, are so obvious that it is strange they have been so long overlooked. When such an education is universal, then we

anticipate that medical men will find it more easy to combine the scientific and practical departments of their profession. At present, those who devote themselves to the former must live chiefly upon hope.

But there are, even in this country, not a few, we trust, who find in this pursuit so elevated and never-failing a source of happiness, that they would not relinquish it for any mere worldly good to be anticipated from following a different course. And it is not among these, we venture to pronounce, that those jealousies and disputes prevail, which too frequently meet our view, and which are so disgraceful to the votaries of science. All these disputes may, we think, be traced to that misapprehension of the true ends of knowledge against which we have contended. To the man who loves knowledge for its own sake, and for the sake of its beneficial influence on his race, what does it matter whether he alone has attained the elevation, or whether he shares it with others? The prospect below, around, and above him is the same. He has the same animating satisfaction in the review of difficulties overcome;—the same expansion of feeling as he surveys the extent of the domain beneath his feet;—the same delight in the glimpses he discovers of paths which may conduct him to new and yet more valuable acquirements. It is among those in whom the mere love of fame is the strongest,—who seek most eagerly for the applause, not so much of the master spirits of the age, as of the world at large, and for the substantial advantages which that brings with it,—that we observe the keenest sensitiveness to detraction in regard to the value of their attainments, the greater disappointment if it can be proved that they have been at all anticipated in them. Such a seeker after truth can bear the proximity of no other. He must stand alone. He looks at the pinnacle of knowledge, not as the commanding height from which he may take a wider survey of its glorious domain, but as the pedestal on which he may elevate himself above his fellows, that they may fall down and worship him around its base.

Which of these characters are we most inclined to respect and love?

VII.

Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London. Second Series. Vol. IV.—*London*, 1839. 8vo, pp. 334.

1. The present volume commences with some "Cases of Spasmodic Disease accompanying Affections of the Pericardium. By Richard Bright, M.D." The attention of the profession has been recently called, and we believe with advantage, to a local affection, by some regarded as of a spasmodic, by others as of a paralytic character, depending on unnatural compression or irritation of distant nerves. We refer to the pathology of Laryngismus Stridulus, as explained by the late Dr. Lee. In the present paper, by Dr. Bright, it is attempted to show that various affections of a spasmodic character may be brought on by the effects of inflammation of the pericardium.

"All the cases will be found to agree in the great extent to which certain portions of the nervous system were involved, the irritation of which might, in two of them at least, be looked upon as the cause of death, and in the third bore no

small part in the exhaustion of the patient. In each of the cases, the pericardium was importantly implicated; and there was reason to think that the phrenic nerve was the more immediate means of communicating the irritation to the spinal cord. The nervous affection has been in each case of a spasmodic character, but has presented such a variety of aspects, as, on that account, to afford interesting matter for reflection. In one case it was *chorea;* in a second it was *trismus*, terminating in *epileptic convulsion;* and in a third case, modified by the sex of the patient, it assumed more the appearance of *hysteria*." (p. 2.)

The following is an outline of the first case related by Dr. Bright: A young man, aged seventeen, suffered from general but not acute rheumatic symptoms. When these were subsiding, peculiar spasmodic symptoms occurred, which eventually resembled chorea of most unusual violence. Notwithstanding all the remedies employed, the chorea became worse, and the spasms put on the character of the most violent convulsions. There was some wandering of mind, and personal restraint became necessary. He died about the sixteenth day after the commencement of the spasmodic disease. The body was examined on the following day: the heart, pericardium, and contiguous parts of the lungs were almost exclusively the seat of disease; the heart was adhering to the free pericardium, by the most profuse effusion of firm semi-transparent gelatinous fibrine; this was particularly the case about the base of the heart; the edges of the lung which lay on the pericardium were slightly adherent to that membrane by inflammation; the brain and abdominal viscera were most carefully and minutely examined, and all found perfectly healthy.

The second case was that of a man who, after some imprudent exposure to cold, was seized with pains apparently of a rheumatic character. To these succeeded orthopnœa, quick, irregular pulse, and difficulty in swallowing. The last symptom became very distressing, and was accompanied with much difficulty in opening the mouth; the patient could swallow only with a kind of convulsive catch; the heart was acting very rapidly and spasmodically; the tetanic symptoms rapidly increased, and the teeth became completely closed; the patient could not swallow his saliva, and there were some slight indications of spasmodic action of the muscles of the back. In spite of all remedies, there was no relaxation of spasm; but to this were added severe convulsive attacks, of an epileptic character. After suffering several of these, he died within twenty hours of the first appearance of dysphagia, on which the trismus and convulsions had rapidly followed. After death, the lungs were found to be somewhat gorged with blood, but crepitant throughout almost their whole substance. The lower half of the right pleura, as it covers the ribs and extends over the whole diaphragm, was very much inflamed, and covered with a thin coating of fibrine. The inflammation was still more marked as it ran up towards the root of the lung, and covered the right side of the pericardium, where the phrenic nerve was seen winding its way down the membrane in the midst of indications of the most intense inflammation; and as it ran over the diaphragm, was covered with shreds of recent false membrane. The left pleura was also inflamed. No other morbid appearances were found. *The head and spine were not examined.* Dr. Bright especially calls attention to the combination, in the above cases, of inflammatory affections with convulsive diseases,

masking each other; and adds, that, "the great and important point is, to impress upon our minds the fact, that the most violent attacks of spasmodic disease will occasionally owe their existence to inflammation of that portion of the pleura and pericardium where inflammation is often with difficulty detected; that part more particularly where the phrenic nerve in its course or its distribution is to be found." (p. 9.) Dr. Bright has been for many years persuaded of the connexion between chorea and inflammation of the pericardium—a connexion not much attended to, although the combination of the spasmodic disease with rheumatism has long been recognized. It would appear that when disease of a spasmodic character has appeared in connexion with rheumatism, it has been commonly attributed to metastasis of inflammation to the spinal cord. Dr. Bright's attention has been long called to the subject, and he has been long convinced that inflammation of the pericardium has been one among the numerous causes of chorea. Speaking of the connexion of chorea and rheumatism, Dr. Bright says:

"The instances are numerous; and though I doubt not the coverings of the cerebro-spinal mass may be and are implicated, yet I believe that the much more frequent cause of chorea, in conjunction with rheumatism, is the inflammation of the pericardium, and that the irritation is communicated thence probably to the spine, just as the irritation of other parts, as of the bowels, the gums, or the uterus, is communicated, and produces the same diseases." (p. 15.)

We have given Dr. Bright's communication, but not without a strong feeling that there is something very unsatisfactory in its pathology. Every one is conversant with cases of pericarditis and of rheumatic pericarditis, without the accompaniment of any spasmodic disease; and every day's experience brings before our notice the effects of inflammation among parts plentifully endowed with nerves, capable, we may fairly presume, of conveying such a stimulus to the spine as might show itself by convulsion or spasmodic actions. But what is the truth? We do not make the objection with any idea of being able to satisfy it; but it is well when we are disposed to imagine that tangible morbid anatomy gives us a clear insight into pathology, to point out its present deficiencies, that we may not remain content with such partial knowledge.

II. A case of malignant disease of the tongue, removed by ligatures applied beneath the jaw, between the mylo-hyoid, genio-hyoid, and genioglossi muscles, is related by Mr. Arnott. The result of the operation was recovery. It is useless to abridge the account of the operation, and we cannot give it entire. Those who may be called on to remove diseased parts implicating a large portion of the tongue, will refer with advantage to the description of Mr. Arnott's operation.

III. The "Memoir on Tuphlo-enteritis, by Dr. Burne," will be noticed in another place.

IV. Mr. Salter has related a case of *carditis*, the form of which, as described by him, is undoubtedly rare. But it is probably wrong to conclude that inflammation of the substance of the heart is very rare,

although inflammation terminating in the formation of pus may be of unfrequent occurrence. Various reasons might probably be given why, even supposing the muscular substance of the heart to be inflamed, the action should not continue so long as to end in suppuration.

There are probably, says Dr. Hope, cases which, though not attended with effusion of pus, will come under the denomination of universal carditis. Softening and induration are the results of inflammation in other muscles, and analogy points out that they have the same origin in the heart. And further evidence is derived from the fact, that, in cases of pericarditis, the characters in question only occupy a certain depth of the exterior surface of the organ, whence the presumption is almost positive, that they originate in an extension of the inflammation from the pericardium. The morbid appearances discovered after death in Mr. Salter's case were as follows:

"The capillary and other vessels on the bag of the pericardium were distended with red blood. . . . . That portion of the membrane investing the substance of the heart showed also, by its unusual vascularity, striking evidence of its having suffered from inflammation. . . . . The part of the pericardium which had sustained the most intense muscular disturbance was that which lies on and is attached to the diaphragm. Here there was not only a distended state of the capillary and larger vessels, but numerous ecchymosed spots and blotches resembling those observed on the skin in purpura hæmorrhagica. There was no lymphatic, serous, or purulent effusion into the pericardial sac. The heart was somewhat larger than natural, its substance of moderate firmness; large coagula were found in all the cavities. . . . . There were no signs of disease in the lining membrane of the heart or valves. In the ascending aorta there were the appearances of commencing ossification. . . . . Excepting a small portion, of a few lines in thickness, on either surface, the left ventricle had entirely lost its muscular colour; it was of a lightish yellow hue, but still preserving the fibrous character of muscle. From all the cut surfaces of the various sections which were made, could be scraped purulent matter. In some parts absorption had taken place, leaving small cavities in the muscular substance, varying from the size of a pin's head to that of a small pea; these were all filled with pus." (pp. 77-8.)

As will be supposed, there were no symptoms which could be considered as pathognomonic of the above condition. It would appear to be one of unmixed carditis; for we can scarcely agree with Mr. Salter in regarding as of an inflammatory character the vascularity which he has described as affecting the pericardium. Experience has, we believe, shown, that in the absence of some of its effects, such as suppuration, the formation of lymph, &c., it is never safe to infer that inflammation has existed, however great may be the vascularity of diseased parts. And in this case there appears to have been a layer of healthy muscular substance intermediate to the pericardium and deposits of pus.

Although not strictly belonging to the present subject, we cannot forbear to notice that we have lately witnessed an instance of gangrene of the heart. The case was that of a young man, who died of gangrene of the pleura and substance of one lung. In the muscular substance of the heart was a portion of gangrenous matter about the size of a common nut. Of the nature of this morbid condition we entertain no doubt; and it is only recorded here, because the disease, if ever witnessed, appears to be one of infinite rarity. We should add, that the action ending in gangrene in this instance could scarcely be regarded as

inflammatory, as the dead portion was immediately surrounded by healthy muscular substance. Mr. Salter's case is one of considerable interest.

v. Some "Notices of the Effects of Lead upon the System; by James Alderson, M.A. and M.D.," follow. If an individual with any partial paralysis is removed away from the causes, predisposing and exciting, of such paralysis, is put upon a generous diet instead of one of an opposite quality, is subjected to a course of medicines of which opiates and quinine form a part, and is required in every way to give rest to the part paralyzed; to what would the majority be disposed to attribute the cure which might take place? And if, rejecting the idea that the cure had been brought about by the combined means, any *one* was to be selected as that to which most benefit was to be attributed, which of the above would be chosen? Dr. Alderson meets with cases of amaurosis as an effect of lead. He takes his patient away from the atmosphere of lead in which she contracted the disease, puts her upon a generous diet, soothes her system with opium and tonics, and ties a handkerchief over her eyes; and then writes a paper to attribute the cure to the effect of preserving the retina from the stimulus of light. We have no doubt of the importance of the means suggested by Dr. Alderson, and cannot but much commend its ingenuity; but we think that too much is attributed to one of the many causes of recovery. The cases are of amaurosis, contracted by lead-workers. As Dr. Alderson's remarks are interesting, we shall quote them:

"The more usual seat of paralysis from lead, which is apparent to observation, is well known to be in the nerves supplying the flexors and extensors of the wrists and arms, especially the extensors; and the plan adopted by Dr. Pemberton, of supplying splints on which to keep the hands and fingers extended, by which the continued extension of the extensor muscles is relieved, and their suspended power recovered, has often been tried with success, both alone and in combination with internal remedies. It is upon the same principle that I have adopted a plan to restore the lost power of the nerves of vision. The action of light is the proper stimulus of the retina, and its continued action, whilst the nervous expansion is paralyzed, is precisely similar to the continued extension of the extensor muscles of the arm and hand; as in this latter case, relieving the muscles from their continued extension, by means of splints, has been found to restore their suspended power, so it was, by analogy, to be supposed, that by removing the stimulus of light altogether from the eye, the power of vision would probably be regained. With this view, a bandage was directed to be applied over the eyes, and constantly worn day and night, so as to preclude the possibility of any light acting upon the nerves of vision. The practice was followed in both cases with success. We see a precisely similar effect arising from the application of the same principle in an over-distended bladder. The over-distension takes away the power of contraction; whilst, by emptying the viscus of its contents, or in some cases only by keeping it empty, the contractile power of the organ is restored." (pp. 82-4.)

The application of this treatment, combined as already mentioned, was followed by success in two cases of amaurosis in lead-workers.

vi. The valuable account of the "Occurrences at the Smallpox Hospital, London, during the year 1838; by Dr. Gregory," it would be a pity to abridge; and we regret that, unabridged, we cannot spare space for it.

VII., VIII. Mr. South has recorded a "Case of Fracture of the Coracoid Process of the Scapula, with Partial Dislocation of the Humerus forwards, and Fracture of the Acromion Process of the Clavicle." Mr. South publishes it as an accident, the occurrence of which has been doubted, and a dissection of which has not, so far at least as he is aware, been hitherto made. We must refer to the volume itself for the description of this accident, as well as for Dr. George Budd's "Statistical Account of Cholera in the Seamen's Hospital, in 1832."

IX. Mr. George Busk, Surgeon to the Dreadnought, has tied successfully the common carotid for an aneurismal tumour within the orbit. The case is related.

X. "On the Softening of Coagulated Fibrine. By George Gulliver, F.R.S." There is a great want of accuracy in the employment of the words pus and purulent; and the old means by which it was habitual to decide on the existence of pus are quite inadequate to distinguish it from other known fluids; whilst it was and probably still is doubtful what different unknown fluids are still spoken of as pus. The paper of Mr. Gulliver shows, and we think satisfactorily, that fibrine in a softened state, bearing as it does an external resemblance to pus, has been mistaken for the latter fluid. We shall give a short analysis of Mr. Gulliver's experiments and opinions. Clots of fibrine taken from the heart were carefully subjected to a somewhat elevated temperature; parts of it became softened; the softened portion was subjected to examination.

"It could not have been distinguished from pus, either by its colour and consistency or by the facility with which it was miscible with water; it was not rendered ropy by caustic volatile alkali, and presented no irridescence when pressed between plates of glass before a candle in the manner pointed out by Dr. Young. Subjected to the microscope, the fluid was found to be loaded with particles extremely variable in size and form; some appeared very dense, others were flaky and larger than pus globules, but the greater number were smaller, extremely irregular in shape, not at all globular like pus, but consisting of very minute granular masses, and a few rounded particles about half the diameter of those of the blood were observed On the addition of acetic acid, all the particles soon disappeared, except a few which seemed more compact, and required a longer time for solution. Treated with sulphurous acid, they swelled considerably, and became nearly invisible." (pp. 138-9.)

"Some of the fibrinous concretion formed, in consequence of inflammation, on the pleura of a dog, was confined in a glass tube, and subjected to a blood heat for eighty-four hours, when it was found to be throughout softened, and presenting just the colour and consistency of pus, from which, however, it was easily distinguishable by the characters noticed in the other trials." (p. 141.)

Fibrine, softened and presenting the characteristics above mentioned, was taken from the femoral and other veins. In most of these cases, Mr. Gulliver states that he has not found any traces of inflammation in the coats of the vessels, such as roughness of the internal membrane, from adherent coagulable lymph, or increased vascularity or suppuration in their cellular sheaths. On the subject of these coagula in veins, it is observed:

"Although the occasional obstruction of the veins by sanguineous coagula, whether softened or not, has become of late years well known . . . . . my obser-

vations lead me to believe that the affection is not only more frequent than is generally supposed, but more concerned in disease, a circumstance to which my attention was first directed by Dr. John Davy. Those who have not been in the habit, when examining dead bodies, of accurately attending to this point, would be surprised at the numerous examples of this condition noted in the register of dissections made under his superintendence at Chatham. The clots were in most instances partially whitened, and softened in the centre; they were found in the heart, and also adhering to the inner membrane of the aorta; in the veins of the lower limbs, as well as in the cava, portal, and renal veins; in the pulmonary vessels, and in the sinuses of the dura mater. They were chiefly observed in subjects who, having long suffered from various chronic maladies, had great prostration of the vital powers, and languor of circulation in the last days of existence..... If, therefore, a vein becomes obstructed with coagulated blood or fibrine which cannot easily be absorbed or organized, in consequence of the very feeble vitality of the patient, the clot may be expected to soften, and it is accordingly in such cases that the affection may be often found after death, although never suspected during life." (pp. 147-9.)

The softening of coagulated fibrine is regarded as distinct from suppuration, and as constituting a considerable proportion of the cases generally denominated suppurative phlebitis. Pus is sometimes observed, mixed with the softened fibrinous matter:

"In two instances, some purulent globules were observed, completely insulated, in the centre of a clot, from the neighbouring tissues. But in both these cases pus was also detected in the blood; and when it is considered how frequently this contamination exists, it will not appear surprising that the particles of pus should become entangled in coagula contained in the vessels. Nor does this circumstance at all invalidate the conclusion that the softening of coagulated fibrine is a disease of much interest and importance, quite distinct in its nature from suppuration, although the two affections have been generally regarded as identical." (p. 152.)

XI. "Contributions to the Pathology of the Spinal Cord; by William Budd, M.D.," constitute the next communication. These contributions consist of several remarkable cases, with observations thereon, illustrative of various points of the pathology of the spine, to which attention has of late been much directed. One of Dr. Budd's objects has been to show, from his cases, that the involuntary movements excited in the palsied limbs were the same in kind as those observed in decapitated animals. The cases are well deserving of attentive perusal. In the first case there was total loss of voluntary power in the lower extremities, and involuntary muscular contractions were excited by applying stimulus to the skin or mucous membranes. This case, and others to which we shall allude, are of interest, as illustrating the probable want of connexion of the excito-motory actions with sensation:

"Although sensation was never quite extinct in Larg's lower extremities, it was evidently not concerned in the production of the involuntary contractions; for when an artificial mode of stimulus was employed, the convulsions were uniformly the most vigorous in that case in which the application was not felt by the patient, namely, when the hollow of the foot was lightly touched with a feather. Here, then, we have oral evidence from the subject of experiment, that these acts are quite independent of sensation and volition. The fact that such vigorous convulsions were excited by touching the hollow of the foot with a feather is of great interest, and tends to show that the nerves which transmit these impressions to the spinal marrow are—if not the same with those of true

sensation—most actively excited by a stimulus which produces strong impressions on the sensitive nerves. Every one knows how urgent is the sensation of tickling in the hollow of the foot. Volkmann has proved that these two kinds of nerves (if there be two kinds) arrive at the spinal marrow in one bundle, namely, the posterior roots. That their distribution should be alike is obvious, for their purpose is the same—the reception of external impressions. We must not fail to remark, that, in the case of Larg, convulsions were never induced by excitement of the passions." (pp. 159-60.)

Respecting similar phenomena in another case, it is remarked of the convulsions :

"Their independence of sensation was curiously shown by the difference of effect from the application of a heated plate, and of the same plate at common temperature, although the difference of heat in the two cases was not felt by the patient, the sense of contact being all that was perceived. This continuance of the sense of contact, while that of temperature was completely lost, is a novel and interesting illustration of the speciality of sensations; a subject so ably treated by Sir C. Bell." (p. 175.)

Without being able to give it any satisfactory reply, Dr. Budd moots the question as to the cause of the susceptibility to involuntary contractions in muscles which do not commonly exhibit it, when in a paralytic state,—whether this is to be ascribed to the withdrawal of the influence of volition, or to an irritation of the cord, and consequent increase of its motor power. Dr. Budd's opinion is in favour of the cooperation of both causes in the majority of instances, especially where the involuntary contractions are powerful and extensive.

The remarks of Dr. Hall in the next communication bear upon the subject, as well as upon other facts in Dr. Budd's paper. The convulsions observed were not associated with a sense of fatigue, but the power that caused them was subject to exhaustion, and removed by rest. "Strychnia given internally increased the susceptibility to involuntary movements, and caused spontaneous convulsions that were limited to the palsied parts."

XII. The next communication is entitled "Memoirs on some Principles of Pathology in the Nervous System. By Dr. Marshall Hall." The subject of the paper is the *condition of muscular irritability in paralytic limbs.* On this subject discrepant opinions exist; some maintaining that the irritability of muscular fibre remains in paralytic limbs; others the contrary. The object of the author is to explain this difference of opinion, by showing that previous physiologists have taken a partial and incorrect view of the matter. It is essential to distinguish between cerebral paralysis, or that which removes the influence of the brain, and spinal paralysis, or that which removes the influence of the spinal marrow. We have not space to devote to the experiments and opinions quoted from Prochaska, Nysten, Legallois, &c., by Dr. Hall, to establish the statement above made respecting the supposed conditions of muscular irritability in paralytic limbs, nor indeed is it necessary ; but we feel pleasure in laying before our readers, with some minuteness, the author's mode of reconciling the differences alluded to.

If strychnia be given to a paralytic, it is *often* the paralytic limb which is first affected by it. Is the irritability of the muscular fibre thus affected increased? The affirmation of this question, says Dr.

Hall, would explain the phenomenon. The following experiments, in illustration of this point, are quoted: "A little child, aged two years, was perfectly paralytic of the left arm. The slightest shock of galvanism was directed to be applied which could produce an obvious effect. It was uniformly observed that the paralytic limb was agitated with a degree of energy which produced no effect on the healthy limb." (p. 200.) In similar cases, and in hemiplegic cases, the experiment was repeated, and, with two exceptions, the results were the same, the paralytic limb being always more shaken than any other part. Dr. Hall next experimented upon animals. The experiments were made on six frogs.

"I divided the spinal marrow immediately below the origin of the brachial plexus, and I removed a portion of the ischiatic nerve of the right posterior extremity. I had immediately, or more remotely, the following interesting phenomena: 1st. The anterior extremities alone were moved spontaneously. . . . 2d. Although perfectly paralytic, in regard to spontaneous motion, the left posterior extremity, that still in connexion with the spinal marrow, moved very energetically, when stimulated by pinching the toes with the forceps. 3d. The right posterior extremity, or that of which the ischiatic nerve was divided, was entirely paralytic, both in reference to spontaneous and excited motions. 4. After the lapse of several weeks, whilst the muscular irritability of the left posterior extremity was gradually augmented, that of the right was gradually diminished. . . . . In this interesting experiment, we have, then, first the phenomena of loss of spontaneous motion on removing the influence of the brain, the excited or reflex actions remaining, and the loss of these on removing the influence of the spinal marrow; secondly, in the case of mere cerebral paralysis, we have augmented irritability, and in that of the spinal marrow, we have the gradual diminution of this property. 5. Strychnine being now administered, the anterior extremities and the left posterior extremity, or that still in connexion with the spinal marrow, became affected with tetanus; but the right posterior extremity, or that severed from all nervous connexion with the spinal marrow, remained perfectly flaccid. 6th. Lastly, the difference in the degree of irritability, in the muscular fibre of the two limbs, was observed, when these were entirely separated from the rest of the animal. In a word, the muscles of the limb, paralyzed by its separation from both cerebrum and spinal marrow, had lost their irritability; whilst those of the limb, separated from its connexion with the cerebrum only, but left in its connexion with the spinal marrow, not only retained their irritability, but probably possessed it in an augmented degree. The next question came to be—Do these phenomena obtain in the human frame? I visited a patient affected with hemiplegia, including paralysis of the face, and I passed a slight galvanic shock through two pieces of metal, of which one was placed over each cheek. The muscles of the paralytic side were most affected. I repeated the experiment with the same result. I now compared with these, two cases of injury of the facial nerve, passing the galvanic shock in the same manner, through the fibres of the orbicularis: it was now the muscle of the healthy side which was affected by the galvanism, the eyelid of that side being closed, whilst that of the paralytic side gaped as before. I next compared the effect of galvanism in two cases of complete paralysis of the arm, one hemiplegic, the other the result of dislocation of the shoulder. The muscles of the former were more, those of the latter less irritable than those of the healthy arm, respectively, as were also those of the arm of a patient affected with the paralysis induced by lead. Lastly, I compared the cases of paralysis of the lower extremities, one arising after pertussis, and therefore cerebral, the other, I think, from disease within the lumbar vertebræ; in the former there was augmented, in the latter diminished irritability. By means of these experiments and observations, we are enabled, I believe, to explain all the apparent discrepancies between the statements of former authors and between each

of them and my own. . . . . We may conclude that, in cerebral paralysis, the irritability of the muscular fibre becomes augmented, from want of the application of the stimulus of volition; in paralysis, arising from disease of the spinal marrow and its nerves, this irritability is diminished, and at length becomes extinct, from its source being cut off." (p. 201, et seq.)

The same facts may be applied to the diagnosis between spinal and cerebral paralysis.

We subjoin the conclusions at which Dr. Hall arrives from the foregoing and other observations:

"1st. That the spinal marrow, exclusive of the cerebrum, is the source of the muscular irritability.

"2d. That the cerebrum is, in its acts of volition, an exhauster of that irritability.

"3d. That, in muscles separated from their nervous connexion with the brain, we have augmented irritability.

"4th. That, in muscles separated from their nervous connexion with the spinal marrow, we have, on the contrary, diminished irritability.

"5th. That the degree of the irritability of the muscular fibre of paralytic limbs, compared with that of the muscles of the healthy limbs, will afford us a source of diagnosis between cerebral and spinal paralysis, and especially between

"1, Hemiplegia of the face, and
"2, Paralysis of the facial nerve;

"3, Hemiplegia of the arm or leg, and
"4, Disease of the nerves of these limbs;

"5, Disease of the spinal marrow in the dorsal region, and
"6. Disease of the cauda equina in the lumbar region, &c.

"6th. That the greater influence of emotion, of certain respiratory acts, of the principle of tone, &c., on the muscles of certain paralytic limbs, than on those of healthy limbs, depends on their augmented irritability.

"7th. That the same principle explains the greater susceptibility of the muscles, in certain cases of paralytic limbs, to the influence of strychnine.

"8th. That, in the conclusions of M. Fouquier, Professor Müller, &c., a sufficient distinction was not made between the influence of the cerebrum and of the spinal marrow, which in this, as in so many other respects, have such different properties.

"9th. From these, and other experiments and observations, I conclude, too, that sleep restores the irritability of the muscular system, by arresting the acts of volition, which exhaust or diminish it; muscular efforts, on the other hand, diminish the irritability and induce fatigue."

XIII. Mr. Stafford has described a "Case of Enlargement from Melanoid Tumour of the Prostate Gland, in a Child of five years of age."

XIV. Mr. Curling gives an instance of "Congenital Absence of Pericardium," witnessed by himself, together with the few similar cases recorded in works of morbid anatomy.

XV. The next paper, communicated by Mr. Cæsar Hawkins, is "On a Peculiar Form of Congenital Tumour of the Neck." The kind of tumour here described is "composed of many cysts joined together, in which the proportion of organized matter is so considerable, as to give a more solid character to the tumour, and make it deserve the name of cystic tumour, as much as the apparently analogous cases of cystic sar-

coma, occasionally found in the breast, testis, or ovary of adults." (p. 232.) Mr. Hawkins has met with seven such tumours in the necks of young children. Two of these cases are detailed, and a minute description of the morbid anatomy in one of the cases is given. It is observed that the numerous cysts, composing the tumour, are formed in the common cellular tissue—that they occur more often in the neck than elsewhere. The diagnosis is confessedly obscure. "When [the component cysts are] numerous, and full of liquid, and small in size, they feel like enlarged glands, or other solid globular bodies; when numerous, and only half filled, they become soft and compressible; but in both these cases, the existence of fluid is difficult to be detected, in comparison with those cases in which only a few larger cysts exist. The resemblance to fatty tumours is considerable, and the deception is assisted by the quantity of fat generally situated beneath the skin, and filling up the inequalities of the surface of the tumour. They are also much like a subcutaneous nævus, which is so often developed in the same situation, when the cysts are half filled. . . . . . In each case that I have seen, however, the existence of globular bodies, in some of which fluid was perceptible, distinguished the tumour from every other kind likely to be formed in infancy." (p. 240.)

The inaccessible situation and intricate connexions of the deeper parts of these tumours, in many instances, render the endeavour to extirpate them most unjustifiable. Their treatment may be as follows: 1. Evacuating the cysts by a grooved needle from time to time. 2. Pressure, when the situation of the tumour does not forbid it. 3. Stimulant applications. Respecting the last, Mr. Hawkins observes, "judging from the few cases I have seen, their effect would appear to extend to some depth below the skin, and to act both on the cysts and their fluid contents." We have never met with any of the cases described in this paper; but if we may draw any inferences from the treatment of other forms of encysted tumours, we should be disposed to employ setons in these cases, especially where the cysts are few in number, and of any size. Mr. Hawkins does not allude to this mode of treatment, and many surgeons appear to have an objection to the employment of setons, which we believe to be groundless. Our own mode of using them in analogous cases, is what we should be disposed to try in these, i. e., to introduce one or two threads at first; if too much irritation or inflammation was induced by these, to withdraw one or both, and wait for a more convenient opportunity; if sufficient action did not follow their introduction, to add threads until our object was obtained. The objection which attaches itself to setons, we believe to depend on their being employed, when the state of the health is not such as to justify their employment—on the want of care, to produce the least degree of action, which is consistent with the end which we have in view,—and in neglect of general and local management whilst that action is in progress. Being guided by proper principles, we think that setons might be effectually employed, so as to dissipate, at least, a portion of the tumours described by Mr. Hawkins.

XVI. Two Cases of "Measles occurring oftener than once in the same

Individual" are detailed by Dr. John Webster. The cases must be regarded as authentic, and in that respect valuable.

XVII. Mr. Solly has recorded a "Remarkable Case of Dry Gangrene, occurring in a Child three years and seven months old." The age of the child, and the extensive ravages of the gangrene, render this a remarkably interesting case in many points of view, but we can but refer to it in this place.

XVIII. Mr. Lever has communicated "Statistical Notices of 120 Cases of Carcinoma Uteri," including under the term only such cases as all agree in terming "Carcinoma." Before commencing his investigation, Mr. Lever states that he held many opinions respecting the disease, which were contradicted by his investigations conducted on the numerical plan. This would appear to be a tolerably certain effect of numerical investigations, and certainly is a fact of great importance. The period of life most obnoxious to the disease, appears to be from the fortieth to the fiftieth year. Mr. Lever's tables "afford a complete refutation to the statement that celibacy favours the development of the disease.". . .

"Only 25 women, or 20·8 per cent., had enjoyed good uterine health in early life, while 95, or 79·16 per cent., had suffered either from (what is termed) functional disease, or syphilis. The most frequent malady was dysmenorrhœa, with which no less than 66 had been afflicted. The proportions stand thus:—

		per cent.
Those who had good uterine health		20·8
Those who had suffered from	Amenorrhœa	15·8
	Vicarious Menstruation	0·83
	Menorrhagia	1·66
	Dysmenorrhœa	54·16
	Syphilis	6·6"

(p. 271.)

XIX. Mr. Curling has described, under the name of "Dactylius Aculeatus," an entozoon, many of which were voided from the urethra of a little girl. The generic and specific descriptions are as follows:

"Genus DACTYLIUS (from δακτύλιος, annulus). Corpus teres elasticum annulatum et utrinque attenuatum, caput obtusum os orbiculare, anus trilabiatus.

"DACTYLIUS ACULEATUS. Capite obtuso, toto corpore aculeorum serie multiplici armato, caudâ obtusâ et annulatâ.

"Hab. in Hominis vesicâ urinariâ" (p. 282-3.)

There were no disturbances of the urinary organs corresponding with the presence of these entozoa. Mr. Curling considers that parasitic animals occur in the human body far more frequently than is generally supposed, being commonly overlooked from the minuteness of their size. He subjoins, on the same point, his opinion, that the trichocephalus dispar would probably be found in the large intestines of most bodies after death. "During the last winter," it is added, "they were detected in most of the bodies examined at the London Hospital; indeed, in nearly all the cases, in which much pains were taken in looking for them in the intestinal canal of healthy persons destroyed by severe injuries, as well as of those cut off by acute and chronic diseases." (p. 285.) These are interesting facts, but a question still remains, whether the animals, found in the intestinal canal after death, had existed there as such during the life of the individual whose corpse they are found to inhabit.

xx. Mr. Liston has made some "Remarks on the Acute Form of Anasarcous Tumour of the Scrotum."

"This distension [of the scrotum] is or is not attended by redness or erythema of the surface; but there is reason to think, from the suddenness of the accession, and from the appearances on exposing the cellular tissue, that there is no actual inflammation of its texture; there being no induration, nor any appearance of lymph or puriform fluid in the areolæ. The affection has generally supervened upon abscess or ulcer, perhaps trifling, in the perineum or groin. Its accession has been sudden, the swelling and tension becoming very great and alarming, even within a few hours. The most dependent part, generally the posterior, will be found at a very early period to present one or more deeply-seated ash or tawny-coloured spots; these extend; the integument is speedily involved; and unless active measures be adoped, the entire coverings and investments of the testicles will be destroyed, and these organs exposed. The progress of the case will depend much upon the state of the patient's system, and the nature of the infiltration. . . . . But in hospital practice, ... the surgeon will do well to look with suspicion at any sudden swelling in this region, and be prepared to treat it actively." (pp. 289-90.)

It is necessary to distinguish between these cases and cases of infiltration of urine. Free incisions are requisite in either case, but the urinary passages, in the former cases, do not require to be interfered with. Mr. Liston has related several cases of the affection above described.

xxi. Dr. Lee has given "an Account of a Fœtus of seven months, with its placenta partially adherent to a nævus occupying the scalp and dura mater."

xxii. Then follows an exceedingly interesting communication from Mr. A. Nasmyth, "On the Structure, Physiology, and Pathology of the Persistent Capsular Investments and Pulp of the Tooth." The points to which Mr. Nasmyth particularly wishes to call attention are, "the capsular investment on the surface of the enamel; secondly, the layer external to the crusta petrosa; and, thirdly, the structure and development of the 'fourth constituent substance or ossified pulp." The paper well merits a careful perusal; we should gladly, if it were practicable, do something more than briefly allude to its chief points of importance. With regard to the cellular investment on the surface of the enamel, the author states that it is easy to demonstrate its existence. It will, of course, be most easily found in those teeth which have been the least used.

"The teeth require merely to be macerated in muriatic acid, diluted to one eighth of its ordinary strength, and in the course of a few minutes generally, it will be found to be loosened from the surface; and, as soon as it is once partially separated, it may be easily altogether detached. In all cases where this covering has been removed by means of acid, it has, of course, the appearance of a simple membrane, in consequence of the earthy deposits having been dissolved, and of there being only present the animal tissue. The structure and appearance of the covering, detached in this manner from the enamel, are the same in every respect as those observed in the capsule of the unextruded tooth; consisting, like it, of two layers, fibrous externally, and having on its internal surface, the peculiar reticulated appearance common to both." (p. 313.)

Speaking of the crusta petrosa, concerning which various opinions have been held, the author states it as his opinion, that the capsular investment, above alluded to, has been spoken of as the cementum, or crusta petrosa. The same osseo-membranous covering in the human tooth is much thicker round the fang than round the enamel, and the portion

which invests the fang increases in size somewhat in relation to the age of the individual. It is by means of this capsular membrane that Mr. Nasmyth considers that the milk-teeth are absorbed. On this point it is observed:

"The portion of the capsular membrane under consideration seems to be the only agent capable of effecting the process of absorption; and we may have ocular demonstration of its performing this, by withdrawing carefully a deciduous tooth when it is near falling off. The fangs will then be found to have almost disappeared, and only a small portion of the membrane to remain; the latter is observed on the spot from which the tooth has been removed; it is in connexion with the pulp, and the whole is highly vascular, and retains an exact impression of the surface of the tooth which was opposed to it. As absorption can only be carried on by the surface in immediate contact with the part absorbed, it follows that this membrane must be so organized as to be able to effect that process." (p. 319.)

Mr. Nasmyth seems altogether to leave out of consideration the organization of the teeth themselves. Notwithstanding that there are many very high authorities who discredit their organization, there is a class of facts which cannot be explained but by admitting it, and if so the absorption may be effected by the vessels of the tooth itself.

The disease of the teeth termed exostosis is a consequence of a morbid action in this capsular membrane. It is well known that this may take place in a variety of forms. It is stated that these bony growths may sometimes be detected by a slight alteration in the colour of the tooth, and by its yielding a little more than natural in one direction, when forcibly pressed. These conditions, however, exist in so many other states of the teeth, that we cannot regard them as of any diagnostic value. It is far more important, and far too often overlooked, that "cerebral congestion, rheumatism, earach, tic douloureux, &c., are often supposed to exist" when the symptoms so designated are dependent on exostosis of the teeth. The observations on this subject in the valuable work of Mr. Bell cannot be too strongly impressed upon the mind. We hear too of physicians who never undertake the conduct of a case until the teeth have been examined, and the appropriate remedies applied to such as are defective. And although this extreme is unnecessary, it contains, as do all similar extremes, a valuable practical lesson, which we shall do well to profit by. The small notches, sometimes met with on the necks of teeth, and which are acutely painful when touched, are supposed to be effected by the vessels of the capsule covering the fang.

Illustrations are given of the ossified pulp. In some of the lower animals, the pulp appears to be normally converted into an earthy structure, intermediate in appearance between bone and ivory. In man, the conversion of the pulpy membrane into osseous matter is either partial or general. It is of frequent occurrence, often coming on after the actual or other cautery has been used to allay sensibility without actual destruction of the internal membrane. The formation of this substance is always attended with a peculiar uneasiness of the tooth itself and neighbouring parts; but the patient is often not relieved "till the malady, having crept on to the external capsular membrane, affords to the careful observer a decided demonstration of its existence." Mr. Nasmyth promises the society further communications on the structure of the teeth, which, if they equal the present, must be regarded as very valuable.

xxiii. The volume terminates by some remarks "On the Structure of the Corpus Luteum, by Robert Lee, m.d." Dr. Lee has examined several specimens of corpus luteum, and believes that its structure is different from that which has hitherto been described. With Dr. Montgomery, and all the best authorities, he considers that the *true* corpus luteum is never formed but as a consequence of impregnation. But it is well known that the same name has been given to various bodies in the ovaria which are not, in fact, corpora lutea. And it hence becomes important to determine the true characteristics of these bodies. The two prevailing opinions are: 1, that it is by the thickening of the inner membrane of the Graafian vesicle that the corpus luteum is formed; and 2, that it is formed by, and inclosed between, the two membranous coverings of the vesicle. The latter opinion is that entertained by Dr. Montgomery. Dr. Lee offers another explanation of the structure of this body. He says, having described his observations on the subject, "we may conclude that it is neither produced by a thickening of the inner layer of the Graafian vesicle, nor by a deposit of a new substance between its two coats, but that it is formed around the outer surface of both these coats of the Graafian vesicle, and that the stroma of the ovarium is in immediate contact with the external surface of the yellow matter." In a postscript of this paper, Dr. Lee states that he "examined the ovarium and corpus luteum with Sir Astley Cooper and Mr. Wharton Jones, and the result is, that the correctness of the view which has been taken of the structure of the corpus luteum in this paper, is now put wholly out of doubt."

Art. VIII.

1. *On Inflammation, Chronic Disease, and Perforative Ulceration of the Cæcum, and of the Appendix Vermiformis Cæci, with Symptomatic Peritonitis and Fæcal Abscess.* By John Burne, m.d., Physician to the Westminster Hospital, (*Med. Chir. Trans.*, Second Series, Vol. II.—*London*, 1837.)

2. *Memoir on Tuphlo-Enteritis; or Inflammation and Perforative Ulceration of the Cæcum, and of the Appendix Vermiformis Cæci.*—By John Burne, m.d., Physician to the Westminster Hospital. (*Med. Chir. Trans.*, Second Series, Vol. IV.—*London*, 1839.)

3. *Beobachtungen auf dem Gebiete der Pathologie und Pathologischen Anatomie.* Von Dr. J. F. Albers.—*Bonn*, 1838. 8vo, pp. 218.

Observations in Pathology and Pathological Anatomy. (*Chapter on Typhlitis.*) By Dr. J. F. Albers.

4. *Histoire de Tumeurs Phlegmoneuses de Fosses Iliaques.* Par le Docteur Grisolle, Ancien Chef de Clinique à l'Hôtel-Dieu, &c. (*Archives Générales de Médecine, pour Janvier, Fevrier, et Mars.—Paris*, 1839.)

History of Phlegmonous Tumours of the Iliac Fossæ. By Dr. Grisolle.

In the preceding article, in which we have given an analysis of all the more important papers in the last volume of the Medico-Chirurgical Transactions, we passed over entirely the valuable memoir of Dr. Burne

on *Tuphlo-Enteritis*, or Inflammation of the Cæcum. We did so because we wished to consider the subject of that paper at greater length than we could then conveniently do, and to comprehend in our notice not only a former memoir of Dr. Burne's, but also some publications on the same affection which have recently appeared on the continent. To this purpose, then, we devote the present article, to which we shall give as much of a practical character as possible; well knowing that the diseases under discussion are more common than is generally believed, and are but imperfectly understood by many practitioners, to whom they are not unfamiliar.

An accumulation of fæces in the bowels, sufficiently great to give rise to a perceptible tumour, and usually accompanied by constipation, and more or less of local pain, is a case well known to all practical men. This accumulation may exist anywhere in the course of the large intestine. After the patient has been costive for some days, he is seized with griping pain in some part of the abdomen; and, on examination, a well-defined tumour is discovered, which generally offers some unevenness of surface. At first there is no fever, and but little constitutional disturbance, except some degree of listlessness; but the expression is dull, the tongue is covered with a moist white coat, and indented, and the appetite is impaired. On the free action of a purgative, all these symptoms are removed, and the patient is well. If this remedy be not had recourse to, or if its action be insufficient to remove the obstruction, the collection which forms the tumour increases, and by distending the bowels, occasions the most exquisite tenderness; the patient shivers and becomes feverish; and, occasionally, vomiting and all the symptoms of ileus occur. In some cases, the obstruction is not complete, the bowels act perhaps daily, and from a superficial enquiry, the physician would be led into error as to the nature of the tumour; but, on pursuing his investigation, he will find that the evacuations, though regular, are very scanty and liquid.

Although a collection of fæces, sufficient to form a perceptible tumour, may occur in any part of the large intestine, the most frequent seat of such tumours is the cæcum. This is explained by the pouch-like form of the cæcum, by its being bound down in the right iliac fossa, and consequently admitting of less motion than other parts of the large intestine, and by the circumstance that its contents, which have acquired the excrementitial character, are obliged to move in a vertical direction, or against the force of gravity. It is to cases of this kind, that is, to cases in which the cæcum is distended by a collection of fecal matter, and its mucous membrane in consequence inflamed, that Dr. Burne has given the name of "*Tuphlo-Enteritis*."

But other tumours occasionally form in the right iliac fossa, in dependence on the cæcum, which, with reference both to prognosis and treatment, we must carefully distinguish from the other class of cases. Dupuytren, in his "Leçons Orales de Clinique Chirurgicale," speaking of abscesses in the right iliac fossa, says, "I have long taught that tumours form in the right iliac fossa, which seem intimately connected with the parietes of the cæcum. These tumours are frequently attended by remarkable disturbance in the function of the large intestine: in a great number of cases they terminate by resolution; in some circumstances

by abundant suppuration; and sometimes they are the origin of a general peritonitis." (Tom. iii., p. 331.) In this passage Dupuytren has well pointed out the different terminations of tumours that are met with in the right iliac fossa in connexion with the cæcum. He also remarked that such tumours are preceded or attended by different trains of symptoms —sometimes by diarrhœa, at other times by constipation. But, notwithstanding these differences, he supposed them to be all of the same kind, namely, all tumours formed by the engorgement of the cellular tissue in which the cæcum is imbedded. He held, in accordance with the doctrines of Broussais, that these engorgements may result alike from any cause that produces irritation of the mucous membrane of the neighbouring portion of the large intestine; and that they sometimes terminate by suppuration, but more commonly by resolution. He seems to have never once suspected that the tumour might be simply a collection of fæces.

Dr. Burne has, in the papers before us, rendered a valuable service to pathology, by showing that these tumours differ essentially in kind, and by pointing out the characters by which their different varieties may in general be distinguished. In proceeding to give some analysis of the cases detailed in these papers, we shall divide them into two classes: the 1st including those in which we imagine the tumour to have resulted from an accumulation of fæces; the 2d, those in which it arose from a collection of pus.

1. *From Fecal Distension.* By far the majority of cases of tumour in the right iliac fossa belong to the first category. As an example of them we may cite the second case given by Dr. Burne:—A man is found complaining of extreme pain and tenderness in the situation of the cæcum, where there is discovered "a hard body, like something lodged in the blind extremity of that gut." The bowels had been constipated for a week. He was freely bled and leeched, but the issue seemed doubtful, when the bowels began to act copiously, and the patient recovered. (*Med. Chir. Trans.*, vol. xx., p. 210.)

Occasionally, the obstruction is more complete; and, besides the local pain and tenderness, the patient has vomiting and other symptoms of obstacle to the passage of fæces. The first case of Dr. Burne is a fair specimen of such cases.

A boy, twelve years old, was seized one morning with aching pain in the right ilio-inguinal region. The pain increased as the day advanced; it did not remit or recur in paroxysms, but remained fixed and constant. On the morrow he began to be feverish and to vomit. On the third day leeches were applied to the seat of the tumour, and aperients were given. On the fourth day, the symptoms increasing, blood was drawn to the extent of fifteen ounces, and strong aperient medicines were continued. On the fifth day more leeches were applied. He was then seen for the first time by Dr. B., who thus describes his condition. "The boy was lying on his back quite still, careful not to move his body lest it should aggravate the pain: the whole abdomen was tumid and tense, more particularly in the right ilio-inguinal region, where there was a fixed aching pain, with tenderness so excessive that the slightest touch was reluctantly borne, while over the other regions of the abdomen the tenderness and pain were trifling: he was sick and vomited from time to time;

the tongue was foul and brownish; the pulse frequent and weak; there had been no action of the bowels since the commencement of the illness, now six days, except from the colon by an enema on the second day." (*Med. Chir. Trans.*, vol. xx., p. 209.) Dr. Burne recommended fomentations and gentle saline aperients. On the following day, the bowels had acted several times; some hard lumpy matter had been discharged; and the tenderness, pain, and tension had diminished. From this time the boy's health was quickly and perfectly reestablished.

Instances of the same kind are so frequent among the lower orders of this country, who disregard the slighter inconveniences of constipation, that all medical men, who have much experience in hospital or dispensary practice, must be very familiar with them. We could add a long list to the cases given by Dr. Burne, but the task is needless, as their general characters are very uniform; the cases become more alarming in proportion as the obstruction is more complete, or more difficult to remove. Cases 9 and 10 of Dr. B., come under the category we are now considering; and the last of them is worthy of notice, as showing how difficult it sometimes is to remove these accumulations of fæces. The patient was a gentleman of middle age, and of full habit and stature.

"He was ill in bed, with pain, great tenderness, and a deep-seated resisting circumscribed fulness, amounting almost to a tumour, in the right ilio-inguinal region; the rest of the abdomen was full and slightly tender, but free from pain; the bowels had been obstinately confined for several days, and sickness and vomiting were frequent; the tongue was very much loaded, the urine red with copious deposits of the lithates and purpurates, and the skin of a feverish heat; the pulse was frequent, rather large, but compressible. He had been bled and leeched, had used a hot-bath, and taken calomel and purgatives. The countenance was anxious, and he was much exhausted from several days' suffering, as well as from the necessary antiphlogistic treatment. The disease was manifestly an inflammation of the cæcum. Calomel and opium, with the saline effervescing aperient every four hours, were prescribed: the part to be frequently fomented. The symptoms persisted for two days, when evacuations took place, and he began to recover. The evacuations, though kept up afterwards without difficulty, were insufficient; and there remained the same deep-seated circumscribed resistance in the region of the cæcum, with fulness and flatulence of the belly, and a sensation to the patient that he wanted to be more freely purged, the bowels seeming to him to act only to a certain point, from which the flatus, instead of passing on, would roll back in the intestines. On this account he took a brisk cathartic, composed of colocynth and calomel, followed by senna and salts. These, instead of producing relief, brought on a return of sickness, an increase of the fulness, and of the pain in the cæcal region, and induced us to return to the more mild and appropriate saline aperients, under the use of which the unfavorable symptoms again subsided. His health, however, did not forthwith return; the tongue remained foul, the appetite defective, and the circumscribed fulness was still to be felt in the region of the cæcum. To resolve this, mercurials were persevered in, and it was not till after five or six weeks of strict regimen, and the daily use of a saline aperient, that all evidence of the local disease passed away, and recovery was reestablished." (*Med. Chir. Trans.*, vol. xxii., p. 38.)

In all these cases, the inflammation of the cæcum was evidently secondary, the consequence of some mechanical irritation; and the violent symptoms uniformly subsided when the bowels acted freely. In such instances, when the nature of the tumour is well ascertained, the

use of purgatives must be continued; and we agree with Dr. Burne in thinking that saline aperients, in small doses are the best.

Dr. Burne states that an idiopathic inflammation of the cæcum from the usually assigned cause, exposure to vicissitudes of weather, has never fallen under his notice. On this point, our own experience agrees completely with that of Dr. Burne. We have never met with a case of idiopathic inflammation, affecting the cæcum only. Ulcers are, indeed, often found in the cæcum in persons who have died of dysentery; but they are, we believe, never limited to the cæcum, nor do they often exist in the highest degree in that portion of the large intestine.

Dr. Albers has, however, devoted a chapter of his essay to what he calls "typhlitis acuta"—acute idiopathic inflammation of the cæcum. It appears to us, from the description which he has given of this affection, that he has confounded under this title cases of dysentery, in which the cæcum was affected together with the rest of the large intestine, and cases in which the cæcum was inflamed in consequence of its distension by a collection of fæces. As symptoms of this idiopathic inflammation of the cæcum, (which he says is not a rare affection,) Dr. Albers enumerates—a constant, uniform, burning pain in the right iliac fossa, which sometimes extends in the direction of the ascending and transverse colon; tension and *hardness* in the same part; and pain and *numbness* extending from the right side of the sacrum towards the right thigh. The *hardness* which is felt in the cæcal region Dr. Albers supposes to be caused by the inflamed portion of intestine; and the pain and *numbness*, by the *pressure* which this inflamed portion exerts upon the muscles situated beneath it. We can hardly conceive that the coats of the intestine can be so thickened and indurated, by acute inflammation, as to cause the circumscribed hardness which Dr. Albers has noticed; or that they could become so heavy as to produce, by their pressure, pain and numbness of the thigh. In a subsequent part of his paper, when speaking of inflammation of the cæcum produced by an accumulation of fæces, (typhlitis stercoralis,) Dr. Albers lays great stress on the very same symptoms, and especially on the painful numbness of the right thigh and pelvis, caused by pressure on the internal iliac muscle. We believe, then, that the cases which Dr. Albers supposed to be cases of idiopathic inflammation of the cæcum were, like those we have already cited from the papers of Dr. Burne, cases in which inflammation of the cæcum was excited by an accumulation of fecal matter; and we are led to this belief the more readily, as Dr. Albers has not substantiated his opinion by the details of the very cases recorded by him. In the chapter which he has devoted to acute inflammation of the cæcum, he has given the particulars of only two cases; and these are very unhappily chosen for the maintenance of the opinion, that acute idiopathic inflammation of the cæcum is a common disease; the first being one, in which inflammation of the cæcum was caused by its strangulation in a hernial sac; the second, quoted from the Sepulchretum of Bonetus, that of a man who had swallowed 800 pins, and at the opening of whose body "the cæcum was found in suppuration."

In all the cases to which we have as yet referred, the collection of fæces has been in some sort accidental; it has formed in persons who perhaps have experienced slighter degrees of constipation, but whose

bowels have been in other respects healthy. In all such cases, removal of the obstruction is followed by complete recovery of the patient.

Occasionally, however, the obstruction is the result of some organic disease, which impedes or entirely prevents the natural movements of the part. The cæcum, as well as the pyloric extremity of the stomach and the rectum, is liable to a chronic disease affecting chiefly the submucous cellular tissue, which becomes hypertrophied and almost cartilaginous in structure, and which causes contraction of the natural passage, and, finally, all the evils which result from its obstruction. The disease, when seated at the rectum, may be considerably relieved by the use of bougies, which produce a mechanical dilatation of the contracted part; but when the pylorus or the cæcum is thus affected, the malady admits of little palliation, except from attention to diet. When a collection of fæces forms in the cæcum from such a cause, its removal (which is extremely difficult to effect) will be followed by the relief of urgent symptoms; but, of course, we cannot hope for eventual success. The third and fourth cases of Dr. Burne offer an illustration of the preceding remarks.

The third case is that of a woman, aged thirty-one, who was admitted into Guy's Hospital in a very emaciated state, from an illness which had afflicted her many months, and of which the prominent symptoms were an obstinately constipated state of the bowels, together with sickness so frequent that the greater part of her food was cast up again. She complained of pains in the belly, like the throes of labour; and in the right ilio-inguinal region a small hard tumour was discovered, which was supposed to be the cæcum diseased; the abdomen was tumid and flatulent, and a strong vermicular motion within the belly was observable, corresponding exactly with the peristaltic action of the intestine; besides which, the distended convolutions of the small intestines could also be accurately traced by the undulating elevations of the abdominal parietes. Various means were employed without benefit, and in the course of a fortnight she died. On dissection, the cæcum was found thickened and much contracted, the ileum distended with flatus, and loaded with feculent matter, while the colon was empty and contracted. (*Med. Chir. Trans.*, vol. xx., p. 212.)

The subject of the fourth case was a girl, twelve years old, very small in stature, but not deformed. Ill health, it was said, had impeded her growth for several years. The history of her complaints, as far as could be ascertained, was that she had suffered for two or three years from large tumid abdomen, with a very irregular and difficult state of the bowels, flatulence, spasmodic pains, loss of appetite, continued emaciation, and frequent attacks of sickness and vomiting. The dejections were generally scanty, soft, and very offensive. A fortnight before her death, she was seized with unusually severe pain in the belly, followed quickly by sickness and vomiting: the abdomen was immensely distended, and very tender; and the dejections consisted of mucus only, without any trace of feculent matter. The vomiting continued till her death, prior to which, for some days, the matter thrown up was distinctly feculent. The body, which was extremely emaciated, was examined fourteen hours after death. When the abdomen was opened, the small intestines were seen distended excessively, being three or four inches in

diameter, of a mottled livid red, and agglutinated by soft albuminous matter. Tracing the intestinal canal, the seat of the obstruction was found at the cæcum, to which point the distension of the bowels continued, while beyond it the colon was empty, contracted, and sound. The cæcum when removed and examined proved to be contracted and thickened, its tunics being blended together, and transformed into a dense, opaque, white, unyielding, gristly substance; and anteriorly were discovered numerous organized bands covered with a smooth shining membrane, stretching across the channel of the gut from side to side, in various directions, forming an irregular coarse network. In this contracted cæcum and network, a complete obstruction had been formed by feculent matter, dry and friable, plugging up the channel in a manner which nothing could have removed. Leading to this part was the ileum, filled with an amazing quantity of soft, yellow, homogeneous feculent matter, the bowel itself having all its tunics confounded together, and being converted into a dense strong tissue, a line in thickness, resembling thick wet parchment; all trace of villous structure, or valvulæ conniventes, having disappeared. The colon was healthy. (*Med. Chir. Trans.*, vol. xx., p. 213.)

The forms of disease witnessed in these two cases, especially the latter in which small organized bands stretched across the channel of the gut, are of frequent occurrence in the rectum, and are mentioned by authors as the cause of those varieties of stricture of the rectum, which are benefited in the most decided manner by the use of bougies.

II. *From a Collection of Pus.* The cases included in the second category, those, namely, in which the tumour in the iliac fossa is formed by a collection of pus, differ materially from the preceding. We have already stated that Dupuytren supposed the abscess in these cases to be seated outside the peritoneum in the cellular tissue behind the cæcum, and to be the result of irritation of the mucous membrane of that portion of intestine. Dr. Burne has, and we believe with justice, rejected this opinion, and has shown that the abscess in question is a circumscribed abscess, within the peritoneum, occasioned in most cases by perforation of the vermiform appendix. Dr. Burne states that he has never met with a case in which the abscess arose from perforation of the cæcum. Such cases are, no doubt, extremely rare. In chronic dysentery, in which there is extensive ulceration of the mucous coat of the large intestine, the submucous cellular tissue is amazingly hypertrophied, so as effectually to prevent perforation. When ulceration of the intestine takes place in the course of an acute disease, as typhoid fever or acute dysentery, there is no thickening of the subjacent coats, and perforation occasionally happens. In typhoid fever, it is generally the lower portion of the ileum that is most diseased, and a perforation there is followed by general peritonitis, which proves speedily fatal: in acute dysentery, on the contrary, the large intestine is the chief seat of disease, and when its coats are destroyed in that part of the cæcum which is not covered by peritoneum, an abscess forms in the cellular tissue behind. We are not prepared to say that such cases occur frequently, even in acute dysentery, but that they do occasionally occur is proved by the following instance:

A man, aged fifty-one, previously in good health, was seized, without obvious cause, with griping, almost incessant purging, and all the symptoms of acute dysentery. At the end of ten days, when we first saw him, a tumour could be felt in the right iliac fossa, nearly as large as the palm of the hand, and exquisitely tender on pressure. He was treated by bleeding, leeches, effervescing draughts. Very little change took place in the tumour, and the dysentery continued unmitigated until five days from our first seeing him, fifteen days from the onset of the complaint, when he was seized suddenly with pain in the abdomen, which was so violent as to draw piercing cries from him, and which was speedily followed by vomiting and collapse. He died at the end of a few hours. The body was examined twenty hours after death. There was considerable sloughing of the mucous coat of the large intestine in a great portion of its extent, and at the termination of the ascending portion all the coats were destroyed, and perforation had taken place. It was this perforation that was the immediate cause of death. In the cæcum also, where it was not covered by peritoneum, all the coats had been destroyed, and there was an infiltration of pus in the cellular tissue behind.

Although, therefore, we are forced to admit that an abscess may form in the right iliac fossa in dependence on the cæcum, from perforation of this gut, yet in an immense proportion of cases it is unquestionably a consequence of perforation of the vermiform appendix. This appendix, from its open mouth terminating in the cæcum, is peculiarly liable to the intrusion of small, hard, undigested substances, which can only escape from it by a retrograde course. If these small bodies remain long in the appendix, and especially if their diameter be large compared with that of the canal, they cause ulceration of its coats, and finally perforation. In the following passage Dr. Burne explains his notion of the manner in which this gives rise to abscess:

"As long as the ulceration is limited to the mucous membrane it is of little consequence, but immediately that the peritoneum is perforated inflammation ensues, for the organic sensibility of the peritoneum will not suffer the presence of any foreign substance. Inflammation, therefore, is lighted up, and may spread with rapidity over the whole continuous surface of this membrane, constituting an universal peritonitis; or the inflammation may be limited to the vicinity of the perforation, may be circumscribed and an abscess form."

Adhesive inflammation of the peritoneum is most probably set up before perforation actually takes place, so that the appendix opens into a space limited by adhesions, and not into the general cavity of the peritoneum. The circumstance that this portion of intestine admits of little motion, and its situation in the iliac fossa, are extremely favorable to the formation of a circumscribed abscess.

The explanation which Dr. Burne has given of the production of these abscesses in the passage we have quoted, is borne out by the details of his cases. The error into which Dupuytren was led respecting their seat and its cause had its origin in the prevailing doctrines of his day, and was not corrected by morbid anatomy. Of the six cases which he has given as cases of abscess in the right iliac fossa, dependent on the cæcum, one only proved fatal: in this case, indeed, the abscess was in the cellular tissue behind the cæcum, but from the details which Dupuytren has

given respecting it, we are inclined to believe that it was a case of psoas abscess, originating in caries of the vertebra, and not in disease of the cæcum. (See Leçons Orales. Tom. iii., p. 341.)

We have said that the other cases related by Dupuytren did not prove fatal. A circumstance, however, is mentioned of one of them (obs. 5,) which tends to show that the abscess in that case resulted from perforation of the appendix. After the matter had continued to discharge externally for two or three months, a raisin-stone, which had escaped by the opening, was found in the dressings. Dr. Burne has had more frequent opportunities of making *post-mortem* examinations in such cases. Of the six cases (cases 5, 6, 8, 12, 13, 15,) which he has related, in which there was abscess, four proved fatal. Of these four cases, three presented sloughing of the vermiform appendix; the remaining case (case 12) we believe to be incorrectly classed with the others. With the exception of this case, all those given by Dr. Burne, in which there was abscess in the right iliac fossa in connexion with the cæcum, and which proved fatal, were found by dissection to be cases of circumscribed abscess within the peritoneum, apparently resulting from perforation of the vermiform appendix.

In the course of little more than a year, three cases of abscess in the right iliac fossa, in dependence on the cæcum, have occurred to ourselves. Two of these cases proved fatal; in the third, the abscess opened externally, and, after protracted suffering and the copious discharge of pus having a fecal odour,* the patient recovered. In each of the two fatal cases, there was, as in the cases by Dr. Burne, a circumscribed abscess in the peritoneal cavity, and perforation of the appendix. In one of these cases, the perforation resulted from the presence of an intestinal concretion, of the size of a cherry-stone; in the other, we could discover no such cause for the perforation, which was near the blind extremity of the appendix, and of the diameter of a split pea.

The same conditions, then, being found in all the fatal cases, it seems fair to infer that they existed also in the cases which terminated favorably, and which during life presented the same characters; in other words, that those abscesses which occur in the right iliac fossa, and which are dependent on the cæcum, are not, as Dupuytren imagined, abscesses formed in the cellular tissue, in which the cæcum is imbedded, and sympathetic of irritation of the mucous membrane of that gut, but that they are, primarily, circumscribed abscesses in the cavity of the peritoneum, resulting from perforation of the vermiform appendix.

The cause of this perforation we believe to be, in most cases, the presence of some foreign body, which has become accidentally impacted in the appendix, and which produces sloughing or ulceration of the part beyond it, by causing strangulation, and thus preventing the nutrition

* We infer from this circumstance that the abscess communicated with the cæcum. The intestinal gases do not pass through the coats of the intestines, unless when the latter are dead or strangulated. Fecal odour in the pus, evacuated *during life*, is then, we conceive, indicative of perforation, or, at least, of death or strangulation of the coats of the intestine. In this opinion we differ from M. Grisolle. He says that the pus discharged by an external opening presented a fetid, stercoral odour, in two of his cases, in which there was no reason to believe that the abscess communicated with the intestine. Both of these patients recovered; so that M. Grisolle had no opportunity of examining the condition of the intestine.

of that part. In the cases of Dr. Burne, as well as in those which have fallen under our own notice, the mucous membrane of the large intestine was perfectly sound; so that the ulceration of the appendix was not connected with any general disease of the intestine. In one of the cases that came under our own care, the perforation was *ascertained* to be owing to the presence of a concretion; in two of the cases of Dr. Burne, the extremity of the appendix had sloughed away—a circumstance more likely to result from such a cause than from a simple ulcerative process. The fact that no foreign body was found in these cases, does not form a very strong objection to this reasoning. Every one who has had experience in opening bodies, will confess how readily a small concretion—and the concretion in these cases must necessarily be small—may be overlooked in a large collection of pus, and in the midst of such extensive disease; especially, when the person making the examination does not expect to find it. As a further confirmation of the view we have taken, we may refer to two other cases of Dr. Burne (cases 7, 14), in which there was no abscess, but perforation of the appendix, followed by general peritonitis; and in each of which the perforation was ascertained to have been caused by an intestinal concretion.*

The cases which we have thus grouped together resemble each other in all their essential characters. They occurred for the most part in persons who had suffered no previous disease or derangement of the bowels. The patient was seized with fixed pain in the right iliac fossa, which was soon followed by feverishness, great thirst, a small frequent pulse, anxiety, and, in most cases, by vomiting and by constipation. Soon after the appearance of these symptoms, a deep-seated tumour was felt in the right iliac fossa, at first small, but gradually increasing in size. In some instances the bowels were readily acted upon, in others not; but in neither case did any marked relief follow their evacuation. The abscess, once formed, may either open into the cæcum, in which case the matter becomes discharged by the intestines (Dupuytren, obs. 4, 6; Dr. Burne, case 13); or it may open externally (Dupuytren, 3, 5; Dr. Burne, 5). These terminations appear to be of nearly equal frequency, but the former is the more favorable, from its offering a readier issue to the matter of the abscess, and consequently rendering the suppuration less protracted.

The tumour, when first perceived, has not always precisely the same seat; its situation varying with that of the appendix, which is far from constant. Sometimes it is found in the pelvis; at other times turned upwards, either extended or curved, under the origin of the large intestine.†

Diagnosis. The right discrimination of the two classes of cases of tumours in the right iliac fossa, according as they result from an accidental accumulation of fæces in the cæcum, or from perforation of the vermiform appendix, is of the utmost importance with reference both to prognosis and treatment. When of the former kind, the malady is

* Dr. Copland, in his Dictionary of Practical Medicine (art. Cæcum), states that he has met with four instances of the same kind.

† Manuel d'Anatomie, par J. F. Meckel, tom. iii., p. 402.

comparatively slight; and although the tumour may not be readily dispersed, and the symptoms may be of alarming nature, there is seldom much real danger. When of the latter kind, the case is always serious, and the issue often for a long time doubtful. It is from having confounded these different kinds, that some authors have been so free to promise a favorable termination to cases of tumour in the right iliac fossa.*

A correct diagnosis is equally essential as regards treatment. The means that must be perseveringly employed in the one case, will exhaust the patient, and may even be the immediate cause of death, in the other.

The situation of the tumour is, in both cases, the same; and, in either case, it may form rapidly in a person of almost any age,† of either sex, and in the previous enjoyment of good health. When, however, the tumour depends on a collection of fæces, it is the very size of the tumour, and the distension and obstruction which it occasions, which are the cause of all the symptoms; and the tumour is already of considerable magnitude when these symptoms occur, and the attention of the patient is directed to the part. When the tumour depends on perforation of the appendix, the pain and uneasiness, and even more alarming symptoms, precede its appearance; and, if the patient be observed early, the tumour, when first discovered, is small and deep-seated. Once that the tumour has formed, in either case, the bowels are in general confined; but when it results from constipation, the patient experiences great relief from their action: when caused by abscess, their free evacuation is followed by little or no amendment.

The history of the early symptoms, then, but especially the amount of relief which follows the evacuation of the bowels, and the quantity and character of the matters discharged, will generally enable us to determine if the tumour results from an accumulation of fæces. But a tumour from psoas abscess may form in the same situation, and may be difficult to distinguish from the abscess which we have described as the result of perforation of the appendix. Dupuytren says, that in psoas abscess "the pus spreads along the psoas and iliacus muscles; it is deposited in a liquid state in the iliac fossa, and the tumour to which it gives rise is soft and fluctuating from its first appearance. This remark is sufficient to distinguish these purulent collections from those which we have before described."‡ The character here mentioned will aid us much in forming our diagnosis; but if the view we have taken of one of his cases be correct, it was not sufficient to protect even Dupuytren himself from error. It is proper, however, to remark that, in the case to which we allude, the patient first came under Dupuytren's care when the abscess was on the point of breaking, and consequently when the character which he has described as so important was no longer distinctive.

* Dupuytren says, "The prognosis is not generally very unfavorable, since of sixteen cases, observed in very different circumstances, one only proved fatal."—Leçons Cliniq., tom. iii., p. 349.

† An instance has come under our own notice, in which a tumour in the right iliac fossa, from a collection of fæces, gave rise to very alarming symptoms in a little girl, between seven and eight years of age.

‡ Leçons Cliniq., tom. iii., p. 348.

Another element in diagnosis is the degree of the local pain and tenderness. In abscess from caries of the vertebra this is generally slight; while in circumscribed abscess of the peritoneum it is always very great, and is usually attended with general symptoms indicative of the affection—fever, vomiting, hiccough. In cases still doubtful, we may assist our judgment by a consideration of the habit of the patient, and of the presence or absence of pain, uneasiness, or weakness of the loins which usually precedes for some months the appearance of psoas abscess.

Dr. Albers has described another class of cases, in which an abscess forms in the right iliac fossa, from idiopathic inflammation of the cellular tissue in which the cæcum is imbedded (*perityphlitis*). We are inclined to believe that Dr. Albers has taken a wrong view of the cases which he has grouped under this head. In one case, which he has quoted from Puchelt as an instance of this kind, there was caries of the iliac bone! It seems, indeed, that in many of the cases which Dr. Albers has imagined to be cases of idiopathic inflammation of the cellular tissue, there was a similar combination, for he speaks of disease of the bones as a common consequence of suppuration of the cellular tissue. We need hardly remark, that in all probability disease of the bone was the cause and not the consequence of the abscess.

Dr. Albers also notices perforation of the vermiform appendix, as a common termination of abscesses having their origin in the cellular tissue of the iliac fossa. He seems much puzzled to explain this predilection for the appendix; since, as he justly observes, in this affection the cæcum is more exposed to the causes of perforation than the appendix. It would, indeed, be strange if the matter of an abscess, which could find its way through loose cellular tissue to the skin, or to that part of the cæcum which is not covered by peritoneum, were to open into the peritoneal cavity, and subsequently to perforate the peritoneal and other coats of a small floating body like the appendix! Our readers will agree with us that here, as in the instances in which he found a bone diseased, Dr. Albers read the cases backwards, and took for the effect of the abscess what was, in reality, its cause. Dr. Albers has, indeed, given the details of two cases, in which he found an extensive abscess in the cellular tissue of the right iliac fossa, and in which neither perforation of the appendix or cæcum, nor caries of a bone, is noticed.

But in these cases no mention is made of the vertebra, and it does not appear that they were even looked at. Until this omission be supplied, we cannot admit that in these cases the abscess in the iliac fossa was primary: it seems to us much more probable that it was the consequence of disease of the vertebra. In all such cases which have fallen under our own observation, we have been able to trace the matter to this source. The extent in which the bone is diseased is, indeed, often small, bearing no proportion to the size of the abscess; a circumstance which, together with the insidious manner in which scrofulous disease of the vertebra comes on, is sufficient to account for the real origin of the abscess being so often overlooked. We cannot, then, recognize the disease which Dr. Albers has designated *perityphlitis*. An abscess in the cellular tissue of the right iliac fossa, when it does not result from a blow or external wound, from perforation of the appendix or cæcum, or from

child-birth, is, we believe, in almost every instance, the consequence of disease of the bones either of the back or pelvis. There may be cases in which an abscess forms in the iliac fossa from idiopathic inflammation of the cellular tissue, but we have never seen one.

Treatment.—The nature of the case once ascertained, we can proceed with confidence in our treatment. When the tumour is formed by a collection of fæces, our main reliance must, of course, be on purgatives. If there be considerable pain and tenderness, leeches and fomentations will procure great relief, and may promote the action of other means; but it is only by removing the obstruction that we cure the patient. If powerful cathartics fail of effect, or if they bring on vomiting, we may have recourse to milder ones—to small and repeated doses of salts, which, if the stomach be very irritable, may be given in effervescence. These, partly perhaps from the solvent power of the liquid in which they are given, have often better effect than medicines of more drastic kind. If the obstruction be complete, and symptoms of ileus occur, we have a further resource in bloodletting, and in enemas containing tartar emetic or tobacco.

When the tumour results from perforation of the appendix, we must be careful that we do not, by rough handling or by violent purgatives, destroy the newly-formed adhesions, and thus give rise to general peritonitis. If the fever run high, or the patient be plethoric, we may take blood from the arm, with the view of lessening the inflammation; but we must not waste the patient's strength by repeated bleedings, in the hope of preventing the formation of abscess. Do what we will, the abscess will form, and will eventually discharge itself either externally or into the cæcum. The patient has, at least, a long illness before him, and will require all his strength to support it. A means which we may use with more freedom, and which is more efficacious in relieving the pain and tenderness, is the application of leeches. Besides these, we may prescribe soda-water or effervescing draughts to allay thirst and appease the irritability of the stomach, and wait with patience the issue of the abscess.

In the course of our remarks, we have often referred to the opinion of Dupuytren, that an abscess may form in the cellular tissue behind the cæcum, *sympathetic* of some irritation of the mucous membrane of the neighbouring intestine. In giving this explanation of the origin of the abscesses in question, Dupuytren merely applied to a particular instance a general proposition of Broussais's—a proposition which he considered as one of the key-stones of pathology—that, "when a local irritation reaches a certain height, it repeats itself in other systems or in other organs, and always without change of nature." In his next proposition, Broussais states that it is by means of the nerves that this repetition is effected. "The nerves are the only agents of the transmission of irritation which constitutes morbid sympathies." Since the time when Broussais first promulgated this opinion, greater attention has been paid to the vascular system, as an agent in the dissemination of disease; and every one will now confess that the proposition referred to must lose much of its generality before it will afford a rigorous explanation of facts. A local irritation may, as Broussais seems to have imagined, be transmitted to the brain or spinal cord, and be thence reflected into sensitive

or motor nerves, so as to produce pain or muscular spasm in a part remote from the original seat of disease: pain of the knee from diseased hip; of the testicle from the passage of a calculus along the ureter; of the shoulder from disease of the liver; cramps of the legs from the presence of worms in the intestinal canal; convulsions from teething,—may all be adduced as familiar examples of such *sympathetic* affections. But we question if a well-authenticated instance can be cited of an abscess produced in this manner. Abscesses that have been called *sympathetic* generally form in the course of the lymphatics leading from the part primarily affected, and result from inflammation propagated along those lymphatics, or, perhaps more frequently, from the absorption of some irritating matter. But if abscesses ever do form in the way that Broussais imagined,* we certainly have no evidence that abscesses in the cellular tissue behind the cæcum result from irritation, however great or prolonged, of the mucous membrane of the intestine. Numerous cases of dysentery, of years' standing, have fallen under our notice, in which the mucous membrane of the cæcum was destroyed in a great extent, and the submucous cellular tissue thickened and almost cartilaginous, yet we have never met with an abscess in the cellular tissue behind, which we could ascribe to irritation of the lining membrane of the gut. We are happy to find that on this point, as on many others, our opinion is borne out by the experience of Dr. Burne.

Statistics.—The view taken of these affections by M. Grisolle, in his "History of Phlegmonous Tumours in the Iliac Fossæ," is more comprehensive than that of Dr. Burne or Dr. Albers; as he includes all phlegmonous tumours which appear in either fossæ, whether they result from disease of the intestines, from child-birth, or from any other cause. The memoir is very elaborate, and consists principally of an analysis of seventy-three cases, twelve only of which came under the observation of M. Grisolle *himself:* the rest he collected from different sources. It is from these cases that, to use his own words, he attempts to solve all problems connected with the history of this disease. In fifty-three of these seventy-three cases, the abscess was in the right iliac fossa; and in twenty in the left. Seventeen of the cases occurred soon after child-birth; the rest were independent of the puerperal state. Nine of the seventy-three cases terminated in complete resolution, and in nine others resolution had commenced; but the engorgement had not entirely disappeared when the patients left the hospital; so that in eighteen of the cases the only evidence of abscess was derived from the general symptoms and from the existence of a tumour.

We are much inclined to doubt the truth of the opinion that abscesses and inflammatory engorgements of the cellular tissue are so frequently resolved; and believe that in most of the cases said to have terminated

* Broussais divided morbid sympathies into two kinds: the first manifesting itself by organic phenomena, as congestions, alterations of secretions, increase or diminution of exhalation or absorption, change of temperature, faults ot nutrition; the second by pain, by convulsive movements of the voluntary muscles, by mental aberration. The former he designated organic sympathies; the latter sympathies of relation. He supposed that both were exercised solely by means of the nervous system, but that organic sympathies might take place through the great sympathetic; whereas sympathies of relation required the intervention of the brain and spinal marrow. See prop. 86, et seq.

in this manner the tumour in the iliac fossa resulted simply from a collection of fæces, especially as M. Grisolle, in a subsequent part of his paper, tells us that resolution is very rare when the treatment is commenced later than the fifth or sixth day from the appearance of the earliest symptoms.

Notwithstanding the variety in the cause of these abscesses, and their different terminations, M. Grisolle proceeds with his analysis, as if the cases were all alike. When treating of symptoms, he tells us in how many of the cases there was pain in the iliac fossa at the commencement of the disease, in how many not; that diarrhœa preceded the appearance of the tumour in one twelfth of the cases, constipation in one tenth, alternations of diarrhœa and constipation in rather less than a twentieth, &c.; and, in like manner, when speaking of treatment, he informs us that bleeding was freely practised in twenty of the cases, and enters into a laboured investigation of the effect which this produced, without any distinction as to the kind of cases to which this treatment was applied.

Although we cannot too much commend the industry of M. Grisolle in collecting his materials, we cannot but think such an analysis of them very valueless, and a strange abuse of the numerical method. What is the use of calculations of this nature applied without discrimination to cases of abscess resulting from such a variety of causes—from perforation of the appendix; from childbirth; from blows; and, perhaps, from caries of the vertebræ? A much more profitable task would have been to have grouped together those cases which had clearly the same origin, and to have analyzed them, with the view of discovering in what characters they differed from other cases having with them some points of resemblance, but essentially different in kind; and thus affording us some guidance in arriving at a correct diagnosis of an individual case.

The very abundance of the materials which M. Grisolle has accumulated serves also to increase the disorder. He tells us, at the commencement of his paper, that a great number of the cases which he found recorded in the Archives of Science were incomplete, and wanting in important details. It would have been much wiser to have discarded these cases, which only obscure the light given by the others. The numbers analyzed would, indeed, have been smaller; and the paper would not have had the statistical air which it has at present: but this defect would have been more than compensated by greater clearness and precision.

Approving much, as we do, of the numerical method when legitimately used,—that is, in the proper cases and in the proper manner,—we cannot too strongly protest against its abuse. One of these abuses is flagrant in the memoir of M. Grisolle; we mean the neglect of carefully selecting the materials to which this method is applied. If different objects be confounded, the greater the number of observations, the greater the certainty that the results obtained from them will be incorrect. To borrow an illustration from astronomy—an astronomer, to determine the place of a star, is not satisfied with a single observation, because he knows that his lenses and instruments are not perfect enough to enable him to avoid small errors of observation: he makes a great number of observations; and on the supposition that the errors of indi-

vidual observations will be some in excess and others in defect, and that many of them will thus compensate and destroy each other, he rightly takes the *mean* place, determined by all his observations, as the place of the star. But suppose that he confounded two stars, and sometimes observed one, and sometimes the other. it is clear that the mean place resulting from his observations would be some place between the two, and would be more certainly incorrect than the place given by a single observation. Or, suppose that he always observed the same star, but that the instrument with which his observations were made had some bias, which tended, for instance, to give an error in defect: this error would be common to all the observations; it would be a constant quantity, and would, therefore, appear in the result with much greater certainty than in any single observation. If the astronomer had no means of ascertaining, and so making allowance for this bias of his instrument, all his calculations would be vitiated, and, however great the number of observations, the calculated place of the star would not be the true place, but the true place diminished by the mean error caused by the bias of the instrument.

It is the same when statistics are applied to medical subjects. If different objects be confounded, or if the observer have a particular bias, the conclusion from a great number of observations will be more certainly erroneous than that from a single one. It is this fault of confounding different objects that renders valueless a great part of the calculations of M. Grisolle; and, on account of it, we shall refrain from entering into a complete analysis of his paper, which, nevertheless, contains many judicious remarks.

M. Grisolle agrees with Dr. Burne in rejecting the notion entertained by Dupuytren and others, that abscesses form in the cellular tissue behind the cæcum, *sympathetic* of irritation of the mucous membrane of the intestine. The arguments he makes use of to support his opinion are the same as those employed by Dr. Burne.

Dupuytren states that particular occupations predispose to abscess in the right iliac fossa, and that this complaint is especially common in house-painters, colour-grinders, turners in copper, and persons continually exposed to the dust and emanations from irritating metals. Both Dr. Burne and M. Grisolle have been unable, from a review of their cases, to discover any such influence in the occupations in question. The reason of this disagreement is, that Dupuytren supposed all tumours in the right iliac fossa to be phlegmonous, and consequently reckoned as such fecal tumours, which are no doubt most common in the classes he has specified; while Dr. Burne and M. Grisolle have confined themselves more exclusively to *abscesses* which, as we have seen, rarely result from an accumulation of fæces.

The most interesting observations in the paper of M. Grisolle are those relating to abscesses which form in the iliac fossa after delivery. We are not prepared to enter into the consideration of these cases, as we have wished, in the present article, to confine our attention to abscesses which are dependent on the cæcum, referring to those which arise from other causes only so far as is necessary to illustrate our diagnosis. The circumstance of the appearance of the abscess soon after childbirth is of

itself almost sufficient to distinguish those which result from this cause. We shall, therefore, subjoin without comment some of the statements of M. Grisolle respecting these cases.

Of seventeen cases of this kind which he has collected, the abscess was seated in eleven on the left side, and in six on the right. M. Grisolle can assign no reason for the greater frequency of the abscess on the left side. The disease appeared in eleven of the cases from the third to the tenth day after delivery; in two from the tenth to the fifteenth; in one at the end of a month. In the remaining three cases, the date of the appearance of the disease was not specified.

The prognosis is more unfavorable in cases of abscess after childbirth than in others: seven of these seventeen cases, nearly one half, having proved fatal. M. Grisolle says, " A circumstance worthy of notice is, that none of the patients that have fallen under my own observation had suckled their infant; and the same seems to have been the case with almost all the women whose histories have been left us by authors."

ART. IX.

1. *The Principles and Practice of Medicine; founded on the most extensive experience in public hospitals and private practice; and developed in a course of Lectures delivered at University College, London.* By JOHN ELLIOTSON, M.D., F.R.S., &c. *With Notes and Illustrations, by* NATHANIEL ROGERS, M.D.—*London*, 1838. 8vo, pp. 1088.
2. *Lectures on the Theory and Practice of Medicine, delivered in University College, London.* By JOHN ELLIOTSON, M.D., F.R.S., &c. *Edited by* J. C. COOKE, M.D., *and* T. C. THOMPSON, M.D.—*London*, 1839. 8vo, pp. 744.
3. *Clinical Lectures, delivered during the Sessions* 1834-5 *and* 1836-7. By ROBERT J. GRAVES, M.D., &c., Dublin. (*American Medical Library—Philadelphia*, 1838.) 8vo, pp. 405.
4. *Lectures on the Theory and Practice of Medicine.* By WILLIAM STOKES, M.D., &c., Dublin. (*American Medical Library—Philadelphia*, 1838.) 8vo, pp. 214.
5. *Elements of the Practice of Medicine.* By RICHARD BRIGHT, M.D., and THOMAS ADDISON, M.D., Physicians to Guy's Hospital. Vol. I.—*London*, 1839. 8vo, pp. 613.
6. *Elements of the Practice of Medicine, designed as a Text-Book for the use of Students.* By WILLIAM REID, M.D.—*Edinburgh*, 1839. 8vo, pp. 573.
7. *Elements of the Theory and Practice of Medicine, designed for the use of Students and Junior Practitioners.* By GEORGE GREGORY, M.D., &c. Fifth Edition; revised and considerably enlarged.—*London*, 1839. 8vo, pp. 750.

THE British press has teemed, of late years, with elementary treatises and published lectures on the practice of physic. Such productions, especially the former,—being devoted to the exposition of current doc-

trines and precepts, rather than to the detail of individual opinion or the lengthy discussion of controverted points,—afford good materials for an estimate of the general state of the art in any country at a given time. Adopting this criterion, we think there is ground of congratulation on the late progress and actual state of medicine in the British dominions; and we would even venture to express a belief that there exist amongst us a juster appreciation of the relative importance of the several sources of the science, a minor subservience to any dominant hypothesis, and less of mechanical routine in the treatment of disease, than in any other country of Europe.

The works whose titles head the present article emanate from the three great centres of medical science in these islands: we shall, therefore, take advantage of the occasion to throw a glance over the actual state of British pathology and practice, indicating what we conceive to be its leading excellencies and defects, and the directions in which we may look for the most important additions to our knowledge; noticing, at the same time, such passages of our authors as bear most on the general argument, and commenting briefly on their merits. We have yet received only in part the lectures of Dr. Stokes, as published in the "American Medical Library;" we shall, however, make use of them as far as they go.

It is generally admitted that the rapid improvement of pathology within the last half century has been owing mainly to the increased cultivation of morbid anatomy. While it were vain to deny that the French have far exceeded us in the diligent accumulation of facts relating to this subject, we must claim for our countrymen the merit of having confined their application within juster limits, and brought them into more useful relation to medicine. In England pathologists have for the most part contented themselves with deducing from the inspection of bodies after death a knowledge of the changes of structure in which diseased actions terminate; of the precise seat of such actions and changes; and of the local complications of general affections. These are the true uses of pathological anatomy: it is the misapplication of it which produces a tendency to localize all disease, and to exclude the idea of all deranged action that does not lead directly to change of structure—a mode of reasoning which, if closely examined, will be found tantamount to regarding the animal body as a mere congeries of vessels; since it is by the disordered action of these alone that morbid alterations of structure are immediately produced. The tendency of a purely anatomical pathology is exactly the reverse of what it might at first appear. It professes to give certainty to medical doctrine, by bringing everything under the cognizance of the senses; whereas, in reality, each observed fact is forced to explain all the phenomena coincident with it, whether it have any rational relation to them or not; and thus a large and fertile tract of enquiry, where we have the inestimable advantage of subjecting facts to ocular demonstration, is converted into a hotbed for the growth of hypotheses. We regard this as one of the most unfortunate obliquities that has occurred in the course of medical enquiry; and without meaning to imply in the smallest degree that it is an universal error of French pathologists, or that our own have not frequently fallen into it, we cannot but consider the correct estimation and rational use of morbid ana-

tomy as characteristic and happy features of British medicine. The reader will find some good remarks on the relation of morbid anatomy to medicine in Dr. Stokes's introductory lecture, which we further recommend, as an able exposition of the manner in which our art should be studied, and withal an animated and striking discourse, well adapted to win the attention and call forth the energies of the student.

The physiological study of medicine engenders a disposition to regard the diseased no less than the healthy body as a whole, the parts of which are connected by relations in many instances clearly determinate, and in others not the less real because they elude our means of research. From such a view of the nature of disease has arisen a general principle of *pathological relation*, which is, perhaps, the most important yet established in medicine, because it has reference at once to the origin, the character, and the treatment of morbid actions. It may be thus briefly stated: Certain surfaces and organs of the body are especially exposed to the extraneous causes of disease, and these surfaces and organs are precisely such as are connected with other parts, and with the whole of the living system by the most extensive sympathies and equilibria of action: a large proportion, therefore, of the true theory of disease consists in tracing the origin of morbid actions to parts remote from those in which they are developed as secondary phenomena; in distinguishing primary from consecutive disorders; and in determining the laws according to which a diseased state of one surface or organ produces the same or a different diseased state in another. The germ of this doctrine may be found in many writers previous to Abernethy, but to him we are indebted for the first attempt to expand it. His views, though distinctly based on the principle of pathological relation, were very limited in their scope, and carried to such excess with respect to a particular set of organs, that their immediate application was productive of considerable evil as well as much good.

In the hands of Broussais the principle has been more distinctly evolved, more variously illustrated, and, in many instances, happily applied to practice; but Broussais could conceive no morbid state except that of inflammation, and accordingly his doctrine is only good for as much as inflammation will explain. It may be observed also, that the data on which his reasoning is founded are insufficient, inasmuch as some of the most important inlets of disease are not taken into account. We may instance the nervous system. The noxious agents to which this system is immediately exposed are doubtless widely different from those which affect the digestive, respiratory, or cutaneous surface; but they are not less numerous and influential. Every inordinate impression on the senses, every excessive emotion of the mind, is a more or less powerful morbific cause; and it seems not a little singular that, while we admit the influence of acrid ingesta in causing gastric irritation, and of atmospheric vicissitudes in producing catarrh, we are apt to forget that an intense blaze of light or too loud or long-continued a sound may occasion headach; that protracted mental distress may induce disease of the heart; and that any overwhelming passion may cause instantaneous death. It would be easy to point out other directions in which noxious impressions from without are received by one organ or system, and propagated to others, or to the whole economy. But it would be out of

place here to prosecute this subject further; and we will content ourselves with suggesting that, if any intelligent and well-informed enquirer would take up this doctrine of pathological relation—not with a preconceived preference for one surface, organ, or system, nor for one morbific cause or morbid action, but with an impartial reference to all that is known of the immediate and remote influence of injurious impressions on the living body—he might gain much credit, and confer great benefit on science, at an expense of labour amounting to little more than that of collecting and digesting the materials which already lie scattered over the field of medical literature.

A beneficial result of the physiological study of medicine in the present day is a growing disposition to emerge from the exclusive *solidism* which has till lately prevailed since the final explosion of the Galenic doctrines. That the blood probably possesses life, and is certainly under the immediate and extensive influence of vital agents, are considerations which must inevitably direct attention to the fluids in any system of medicine of which physiology is the basis. The superiority of a physiological over a merely anatomical pathology, as well as the importance of the fluids considered as a source of disease, is very strikingly illustrated by recent researches into the origin of morbid formations. Take for example *tubercle*. So long as we are occupied merely with looking at, handling, and incising the parts in which this morbid structure is evolved, and whose natural texture is subverted by it, we are engaged in a pursuit which bears somewhat the same relation to the study of medicine as that of simple conchology does to the natural history of the testaceous mollusca. We take up a shell, and we call it by a certain name, but we know nothing of the animal to which it belongs, and the animal is dead, or we should not now have its shell in our cabinet. In like manner, we may take up a tubercular mass, and mark minutely its texture, colour, and consistence, and we may say this is the disease called *tubercle*; but the part is destroyed, or we should not now be examining it anatomically. Now all this, though highly important as leading to better things, throws not a ray of light on the origin or cure of the scrofulous diathesis; but if we take the *blood* into account, and reflect that the essential part of the morbid formation may consist in an inorganic product deposited from it, and that the morbid mass we are examining may be composed of the natural texture of the part infiltrated with such matter, and variously incorporated with it, or obliterated by it, we then prosecute our anatomical analysis with a definite object, to ascertain whether this be actually the case; and if we find it to be so, we have made an important step in pathology, and may even entertain a hope that formidable or fatal diseases may, at some happier period of our art, be subdued or prevented by corrigents applied to the blood or to its source in the organs of assimilation. Perhaps the most interesting point in the pathology of morbid formations is their distinction into those of which the characteristic substance is organic, from those in which it is inorganic. Now, this is an essentially physiological question, and, without losing sight of the merits of Andral, Cruveilhier, and others, we think it will be conceded that the labours of Carswell have brought it out in bolder relief, and contributed more to its elucidation than those of any other enquirer.

Among the important studies which follow in the train of physiological medicine are the investigation of the laws of contagion; of epidemic and endemic influences; and of the hereditary tendency of diseases;—subjects quite out of the reach of the scalpel, and therefore neglected for the most part by mere anatomical pathologists; but which, viewed in their true light, as connecting medicine on the one hand with natural philosophy, and on the other with public hygiène, afford some of the finest pursuits to which the scientific physician can addict himself.

On the whole, we think that British medicine has always partaken largely of a physiological character, and we rejoice in the belief that it is daily becoming more firmly established on a physiological basis, which we are convinced is the only true and permanent one.

On the state of British therapeutics we may observe, that one of its peculiar excellencies resides in the conjunction of a rational empiricism with the fulfilment of scientific intentions. We employ many remedial agents simply because they are found to be beneficial, and do not object to a remedy approved by observation, because we cannot explain the mode of its action. From the absence of a like submission to experience among some of our continental brethren, several of the most efficient articles of the materia medica are hardly ever used, or altogether proscribed. Thus, colchicum is nearly excluded from French practice by objections merely theoretical; and mercury is scarcely employed except as a purgative for children;—one result of which is, that some acute inflammatory diseases are much more fatal in France than in England. It is observed by Dr. Graves, and we believe with perfect truth, that most cases of pericarditis in France die, while most of ours recover.

It must be admitted, however, that we are very apt in this country to give in to a much less laudable kind of empiricism, the result of which is an extreme degree of credulity as to the powers of particular medicines. A *reason* not being always demanded why a remedy should be serviceable, the fact that a certain number of patients get well under its use is too often esteemed a sufficient proof of its salutary effect, without due consideration of the question whether they would not have got well as fast or faster without it. We would observe also that, although the practice of British physicians is characterized in many instances by great vigour and efficiency, these advantages are seriously counterbalanced by over-activity in cases where energy is not required, and may be highly injurious; and where a due regulation of diet, regimen, and habits is all that is needful or properly admissible. In truth, we meet with daily examples in which the harmless *tisanes* of our Gallic friends, or the infinitesimal doses of the German homœopathists, might be most advantageously substituted for our own too vehement practice.

Before entering into the particular subjects of the present article, we may be expected to say something on the classification of diseases, with a view to their connected study. There can be no doubt that the high and deserved reputation of Cullen gave an undue prominence to methodical nosology, and that practical medicine has sustained little loss from the comparative neglect into which this study has fallen among the elementary writers of the present day. Of the authors before us, Dr. Elliotson thinks methodical nosology useless, and advises his pupils

never to plague themselves any more about it; he nevertheless admits the advantage of a general arrangement in which morbid actions are considered as to their *nature*, and as to their *seat*. Drs. Bright and Addison express no opinion on the subject. The first volume of their work, which is all that is yet published, embraces the consideration of fevers, of inflammation in general, and of the various diseases which are either distinctly inflammatory or usually supposed to be more or less connected with inflammation in their origin. Dr. Reid follows the classification of Cullen, with such variations as are necessary to adapt it to the actual state of knowledge. Dr. Gregory, in the present edition of his work, adopts an arrangement in which constitutional or general disorders take the precedence, and are followed in succession by those of the brain and nervous system, those of the thoracic viscera, of the abdominal viscera, and of the more superficial parts of the body. Dr. Stokes reverses the order commonly pursued; and, instead of treating first of general pathological subjects, enters at once into the consideration of particular diseases, commencing with those of the digestive system. Dr. Graves's lectures, being clinical, necessarily exclude classification. If we were to advance an opinion on this subject, we should say that methodical nosology, employed as it has been to reduce all known diseases within the limits of a natural history arrangement, is worse than useless, and that all attempts at a universal classification must fail till we are perfectly acquainted with the nature, seat, and symptoms of all morbid actions: it nevertheless appears to us that the objects of pathology may be brought into very useful relations by the formation of insulated groups, formed sometimes on one principle and sometimes on another. For example, who can doubt the advantage of considering in connexion those diseases which consist in inflammation of parts whose texture is similar? Here is a nosological group formed on the ground of *general anatomy*. Again, who can doubt the advantage of regarding, in a common point of view, the diseases of the central organs of circulation and respiration? Here we have a group associated on the ground of *special physiology*. Will any one deny the utility of treating in connexion of the several exanthemata? or of grouping together the various forms of fever distinguished by periodicity of type? In each of the two latter instances we go on the simple principle of the *analogy of phenomena*, the cause or mechanism of which we do not pretend to understand: our nosology is here destitute of any scientific foundation, but the relations it establishes lead to important points of practice. The same disease may enter advantageously into the formation of several different groups, according to the point of view under which it is contemplated; and hence the impossibility of forming a general nosology on principles either philosophically true or practically useful. If these observations be just, it follows that methodical nosologists have failed from attempting too much, and have applied their science chiefly where it could be of least use—in defining the limits of systems, or forming the groundwork of elementary treatises.

In the brief remarks we are now about to make on some points of practical medicine, it is unnecessary to follow any other order than that which convenience may suggest. It is scarcely necessary to add that, amid the multiplicity of objects which present themselves, we must be

content with selecting such as are most illustrative of important principles, or such as derive new interest from the observations, or are rendered imperfect by the omissions of our authors.

Fever. It has been very usual with writers on the practice of physic to consider the paroxysm of ague as a kind of epitome of the phenomena of fever in general, and to make it the basis of their illustration of the subject. Of the authors before us, Drs. Bright and Addison and Dr. Gregory fully admit this principle. While it is easy to recognize the affinity between ague and remittent fever, and to conceive them referrible to a common cause, variously modified in its operation and complicated in its effects, we confess it has always appeared to us inexplicable that any parallel should have been drawn between ague and continued fever. When we consider the persistent character of continued fever, and the marked periodicity of ague; the great tendency to local complication in the former, and the general absence of it in the latter; the rarity of a distinct crisis in the one disease, and the almost uniform occurrence of a particular crisis in the other; lastly, when we contemplate the difference of their morbid anatomy, and the wide diversity of their etiology and treatment, we are at a loss to discover wherein the analogy between them consists, and should almost be disposed to maintain that there is scarcely any other disease attended with pyrexia which has so little resemblance to continued fever as ague. In favour of the analogy, however, we find it familiarly asserted, that the two diseases pass into each other; that ague of a certain duration is often produced into remittent and thence into continued fever; and that the last-named disorder will subside into remittent fever and ague. We are not prepared to deny the possibility of these occurrences; all we can say is that, in a pretty extensive observation of marsh diseases we have never seen an instance of either of them. It is indeed true that ague sometimes becomes complicated with visceral inflammation, and this gives rise to severe and intractable forms of remittent fever, in which the remission can only be so called in contrast to the extreme intensity of the symptoms during the periodical exacerbations: the patient, in effect, labours under a *continuance of fever;* he has local inflammation with the attendant pyrexia, and he has moreover an ague, by the accessions of which the other evils are greatly aggravated. But such a case is widely different from what is ordinarily understood by "continued fever;" and if the local inflammation be subdued, the disease is immediately converted into simple ague. Again, we well know that the debility which attends convalescence from continued fever renders the patient extremely liable to an attack of ague in districts where the latter disease is prevalent; and this, we believe, is the real history of most cases in which continued fever is said to pass into ague. Dr. Elliotson makes a remark which may at first appear very singular, namely, that ague is very often mistaken for *typhus!* yet we are quite convinced that he is right. The truth is, ague when complicated with visceral inflammation is apt to assume an exceedingly adynamic form. There is great muscular debility, depression of spirits, and tendency to syncope; the tongue is brown, and the blood presents a loose crasis: these, however, are delusive symptoms, for a second bleeding will often be better borne than the first, and the coagulum of the blood will be firmer. Certain it is, however, that stupid practitioners sometimes call

such cases typhus, and we have heard of the great prevalence of typhus in districts where, to our own knowledge, that disease was scarcely to be met with.

Our authors abstain, judiciously perhaps, from commenting at any length on the pathology of ague; indeed the subject is scarcely alluded to. In the absence of any certain light, we think physiological considerations afford a strong presumption that the morbific cause acts principally upon the spinal cord. The morbid sensations generally set in with a feeling of coldness in the course of the spine, and disordered functions of the cord are manifested by the hebetude of common sensation which, in severe accessions, almost amounts to anæsthesia, and by the deranged action of the voluntary muscles, which is more akin to distinct clonic spasm than to ordinary rigor. It is not uncommon to see the shivering of the cold stage accompanied with movements somewhat resembling those of chorea; and the same phenomena in a more intense degree have occasionally been exhibited in the various forms of convulsive ague described by authors. It may be observed, also, that those singular cases in which the ague is confined to one half of the body seem to favour the notion that the immediate seat of the disorder is in a *duplex organ*—which is probably the spinal cord, since we have no reason to believe that the brain is particularly implicated.

On the whole, we think that ague may be rationally regarded as forming a kind of link between the *pyrexiæ* and the pure *neuroses;* and that much juster and more useful analogies would spring from such a view of it than from the common one which bases the doctrine of fever on the paroxysm of ague.

A word on the treatment of ague: A variety of remedies have gained an ephemeral reputation as febrifuges, but, being weighed in the balance, have been found wanting. This, we believe, has arisen from the very different degree of curability in the sporadic and the endemic disease. In a place where ague is rare, anything may cure it which produces a momentary change in the state of the system, because it has such a strong tendency to a spontaneous cure; thus, an emetic or an anodyne given on the approach of the cold stage, a fright, or even a charm acting only on the imagination of the patient, may remove the disease: but in a marshy district, where the morbific cause is in constant and energetic operation, the lighter remedies will generally fail altogether. Remedies for ague must be tried in an ague country, or the results of observation will be very equivocal. If the practice lately advocated, of bleeding in the cold stage, had been brought to this test, we imagine there would have been very little dispute as to its claims to confidence. The authors under review seem generally unfavorable to this practice in cases of simple ague. Our own experience has convinced us that it is not generally attended with the dangerous results attributed to it by some authors, but that it is nevertheless prejudicial, by increasing the severity of the subsequent paroxysms, and protracting the duration of the disease. Whatever debilitates the body renders it more liable to ague and less able to shake it off; and in ague districts every peasant knows that if he has been bled, for whatsoever reason, he is more than usually apt to catch the ague. We beg to be understood as speaking of the disease only in its simple form: where it is complicated with local inflammation,

bloodletting must be resorted to on ordinary principles; and it is a curious fact that in such cases venesection often subdues the ague along with the inflammation; more frequently, however, the ague continues, and requires the use of the bark.

There is another form of ague in which we have found bleeding very serviceable. It is popularly known by the name of *dead* or *dumb* ague. There are no distinct paroxysms, but frequent irregular shiverings; the pulse is feeble and intermittent; and the patient is constantly cold, languid, and oppressed. Here a moderate venesection, combined with measures to correct the abdominal secretions which are in a most disordered state, will often immediately change the aspect of the case; the ague assumes a regular form and type, and becomes curable by the ordinary means. Drs. Bright and Addison allude to this form of disease, but say nothing of the treatment: we direct attention to it, because it is by no means uncommon, and requires to be dealt with cautiously, for if the bark be rashly given it may occasion visceral inflammation; indeed we have seen this form of ague converted into a most anomalous and intractable remittent, by the premature use of febrifuge medicines.

Of the only two febrifuge remedies of established reputation, bark and arsenic, our authors generally give the preference to the former; nor can we at all agree with Drs. Bright and Addison, when they represent arsenic as "a remedy rivalling if not actually surpassing the cinchona in power." We never saw the bark fail (in the shape of sulphate of quinine) in a single instance in which it appeared to us to be properly applicable and judiciously used; but arsenic has very often disappointed us entirely, or has not succeeded till pushed to a dangerous dose; and we are bound to add that we have too frequently known the exemption from ague purchased at the price of a very serious irritation of the gastro-enteric membrane. Dr. Elliotson says he has failed with arsenic, but never with bark. The cases in which we have found arsenic most useful are those in which there is a great tendency to relapse. Quinine cures the ague when present, and will generally prevent a paroxysm when threatened, but it seems to have little power of obviating the morbid disposition: if, however, the quinine be given to arrest the disease, and the arsenic be then exhibited in moderate doses, and continued for some time, the morbid tendency will generally disappear.

There has been some discrepancy of opinion among authors as to the propriety of administering bark in ague when inflammation is present. The common precept is not to use this medicine when there are any marked symptoms of local inflammation or congestion, and we believe it to be perfectly just. Several writers of authority, however, have deviated from this rule, and Dr. Elliotson appears to do so in the following passage, which, however, is not remarkable for perspicuity:

"It was formerly thought very wrong to stop an ague until the patient had gone through a certain preparation. I have surprised many persons who, when in other countries, used to see some preparation employed before the remedies for ague were given, by stopping the ague immediately. I never saw injury arise from it; though, if there be any local affection of the head, chest, or abdomen, you must be cautious, and attend to that at the same time. Should there be any congestion of the head, lungs, or abdomen, it may be necessary to bleed, purge, and use all the other remedies for this state; for if you do not, it is possible that the mere stopping the ague at once may be useless. When you

have done everything indicated by the local affection, there will be no danger in stopping ague. I never lost a patient by employing sulphate of quinine when local inflammation was present. Arsenic may be very improper when the inflammation which is present affects the stomach; and in the case of gastritis, probably neither arsenic nor quinine can be borne; and you do not remedy the morbid condition till you adopt local or general bleeding. I have seen local disease removed the more easily after stopping ague; for every paroxysm, of course, disturbs the circulation, renders it more irregular, and is likely to throw a greater load of blood upon those organs which are in a state of congestion. At any rate, ague will make bad worse." (*Ed. Cooke and Thomson*, p. 173.)

Dr. Elliotson, if we understand him aright, believes that the ague generally *can* be stopped, notwithstanding the presence of the local affection. Here we differ from him. We have seen this happen in a few cases; but in a great majority we have found that the ague has been rendered more violent by the aggravation of the local disease: in many instances, also, the type of the ague has become confused, and the case has passed into one of severe and intractable remittent fever. The treatment of ague accompanied with inflammation or congestion requires much caution, and the life of the patient sometimes depends on the vigilance of the practitioner. If we can get the inflammation under, so as to secure a single distinct intermission, or a near approach to it, we can generally control the disease. A single dose of the sulphate of quinine should be given, and its effects carefully noted: if it produce any considerable return of local pain or febrile excitement, we must not repeat it, but wait for a better opportunity; if no bad symptom occur, the medicine should be pushed boldly; and if there be no accession of ague at the next period it may be continued with confidence of success.

It occasionally happens that ague is complicated with some slight inflammatory affection, which shows itself *only during the paroxysms.* Here also it is better to get rid of the local disease first, if we can; but if it prove obstinate, the quinine may be given in the intermissions, and the ague and inflammation will most likely cease together.

We are happy to find that none of the writers before us, in treating of *continued fever*, embrace any of those hypotheses which regard local inflammation as its essence; although we are by no means disposed to deny the frequent occurrence of this state as an incidental complication. With respect to the local derangements of vascular action occurring in the course of fever, Dr. Reid expresses a strong doubt whether they be identical in character with common inflammation, and is inclined to believe that they may more properly be regarded as peculiar results of disordered innervation. He observes, that if the nervous influence be weakened or deranged, a change may be effected in the relation of the fluids to the textures through which they circulate, whereby the ordinary stimulus of the former produces morbid effects, just as in a similar state of the nervous system the ordinary stimulus of light or sound occasions pain and disturbance in the organs of sight and hearing. He adds, that too little attention has hitherto been given to the enquiry, how far the universal derangement of one class of functions modifies subordinately the disturbed conditions of others. We think these remarks worthy of consideration. We have long been of opinion that disordered innervation is much concerned in the local congestions alluded to, and have on former occasions adverted to this topic. Many arguments

might be adduced to prove that such congestions, in many cases at least, differ materially from ordinary inflammation, but the one on which we would principally insist is the length of time which they may endure without causing change of structure. Thus a fever patient shall labour for many weeks under a state of brain supposed to be inflammatory, and yet he shall be restored to perfect health without a vestige of any organic lesion within the cranium. Another shall be affected for the same length of time with what is conceived to be gastro-enteritis, yet this patient also shall recover entirely. In each of these cases, moreover, the transition from a state of dangerous disease to that of complete convalescence may be exceedingly sudden. Now we can hardly believe that real inflammation of a pretty active character could often exist so long without committing fatal ravages, or at least leaving behind it vestiges which would long be perceptible. We dwell for a moment on this point, because, if there be any truth in the opinion advanced, an important practical corollary follows, namely, that the practitioner must not expect to get rid entirely of such complications of fever, but must content himself with keeping them under, trusting for their final removal to the cessation of the general disorder, of which they are merely the effects. The most disastrous results often arise from an obstinate determination to subdue such local affections by repeated bloodletting: the topical disorder is not subdued, and, what is worse, the vital powers of the patient are irretrievably prostrated.

We by no means contend that fever is not frequently complicated with actual inflammation, or that the early and prudent use of the lancet in such cases is not advisable; here, however, the fever is usually of a distinctly inflammatory type. The *insidious* inflammation, as it is called, which accompanies low fever is, we believe, distinct from common inflammation occurring in the same textures, and is not to be subdued by the same means, although the occasional application of a few leeches will often suffice to keep it below the point of disorganization.

Drs. Bright and Addison treat, under distinct sections, of fever with predominant cerebral, abdominal, and thoracic affection, and of the modifications of treatment in each. This is judicious, and will tend to give greater distinctness to the ideas of the student, who will find no difficulty in combining them when more complex cases present themselves. There is one point to which we would here advert, which has probably attracted the attention of most practitioners, but which is not usually mentioned in elementary books: we mean the occasional difficulty of ascertaining whether the local affection complicating fever be seated in the abdomen or in the head. We shall presently, however, have occasion to notice this subject when speaking of abdominal diseases, and have now only to remark that the observations there made are no less applicable to fever than to other acute disorders.

In the treatment of continued fever there is a question of considerable moment, on which the best authorities differ, namely, how far it is possible to *cut it short*. Dr. Elliotson speaks of arresting fever *in limine* as a frequent occurrence; Drs. Bright and Addison as an occasional one; Dr. Gregory regards it as so rare that the hope of it cannot be made a foundation of rational treatment; and Dr. Reid, in expressing a similar

opinion, suggests that as it is by no means easy, except in very well-marked cases, to pronounce on the probable duration of fever, supposed cases of fever cut short may have been merely ephemera. There may be some truth in Dr. Reid's supposition; but we think a much more frequent source of error consists in mistaking symptomatic for idiopathic fever, and imagining that we have cut short a fever when we have merely subdued an inflammation. This is especially true with respect to inflammation of the small intestine, the local indications of which are often obscure, and the symptomatic fever typhoid. Dr. Reid doubts whether it be possible to abridge the course of fever arising from specific contagion, and adduces the analogy of variola and scarlatina, which, he says, can never have their duration altered: we have, however, in one instance seen scarlatina distinctly cut short by venesection. Without denying the possibility of cutting short fever, we regard the occurrence as so rare that it may, in a practical point of view, be put nearly out of the question; neither can we help suspecting that the means employed for this purpose have frequently the opposite effect of protracting the disease. The measures most generally adopted are bleeding and emetics. Now we are convinced that the former, applied with the object of subduing fever—apart from inflammation, congestion, plethora, or other state affording a special indication—has merely the effect of somewhat debilitating the patient, and thereby somewhat retarding those processes by which nature effects the restoration of health. With respect to emetics, we have very small faith in their power of cutting short fever, while we feel well assured that they increase irritation of the gastro-enteric membrane where it is present, and sometimes determine it where it did not previously exist. It is admitted on all hands that when there is any tenderness of the abdomen emetics should not be given; but we would ask, is the absence of tenderness of the abdomen a certain criterion that the mucous membrane of the intestines is sound? We think not. In ileitis, the most frequent abdominal affection complicating fever, the tenderness on pressure is hardly ever great, and sometimes so obscure that it cannot be distinguished from the slight uneasiness caused by pressure when the intestine is merely distended with gas. We would ask also if it be prudent, when the circulation is universally deranged, and every secreting surface disordered, to adopt a measure which necessarily occasions a sudden and violent flow of blood to the intestines? From these considerations we agree with Dr. Gregory—in opposition we allow to the opinion of high practical authorities—that a foul state of the stomach affords the only proper indication for the use of emetics in fever. When this organ is loaded with excessive or acrid ingesta, as indicated by oppression, nausea, and bitterness in the mouth, an emetic may be given with advantage; and since ipecacuan evacuates the stomach as effectually, and irritates the digestive mucous membrane much less than emetic tartar, it is we think greatly to be preferred. We speak here only of the common forms of fever, for in those rarer varieties, which have obtained the name of *congestive*, and which have many points of resemblance to malignant cholera, strong emetics are among the most efficient means of rousing the circulation: nor do the objections above stated apply in such cases; for our object is at all hazards to bring out a disease that can be dealt with, and to prevent the system from collapsing into death without an effort.

We have said nothing of the cold affusion as a means of arresting fever. It is a remedy of which few practitioners of the present day can speak from large experience; but the fact that it has fallen so entirely into disuse warrants a strong presumption that it has not realized the expectations awakened at its first introduction.

Another point of great importance in the treatment of fever is the management of that state of delirious and sleepless excitement which frequently supervenes in the advanced stage, usually from the eighth to the twelfth day. Drs. Bright and Addison tell us that when active delirium comes on at this period, so that the patient becomes violent, talks incessantly, and attempts to get out of bed, the tongue being dry and brown, the skin parched, and the pulse frequent and sharp, some form of depletion is indicated, while cordials induce a state of stupor and low delirium soon terminating in death. They add that if instead of the excited state above described, there is merely confusion or stupor, depletion is still necessary. They do not recommend full bleeding, but state that the abstraction of six, eight, or ten ounces of blood is frequently followed by rallying of the vital powers, subsidence of delirium and a softer state of the pulse. We believe the applicability of these remarks will vary according to the character of the epidemy, for the cerebral affection under consideration is common to the worst forms of typhus and the synochus which is now the most prevalent form of fever in England. Taking fevers as they go, however, our own observation is not so favorable to blood-letting under the conditions alluded to as that of Drs. Bright and Addison: we have seldom found bleeding when very moderately used produce any marked effect one way or another; but when employed less cautiously we have often seen it annihilate at once all hope of recovery. When speaking of the maculated fever of Dublin, Dr. Graves expresses his conviction of the entire inefficacy of depletion in the severe cerebral affection of the advanced stage, and recommends, as a most valuable remedy, the combination of emetic tartar with opium. He gives some very striking cases in which its effects seemed almost magical. This practice Dr. Graves claims as exclusively his own, and assuredly if it prove as successful in the hands of others, he will be entitled to much gratitude for its introduction. The subject of continued fever is well handled by the authors under review. Drs. Bright and Addison have given a neat and flowing outline of it. Dr. Reid has entered into it very much in detail, and viewed it in a great variety of bearings: it is just to add that he has given many indications of a philosophical mode of thinking and of close personal observation. Some of the clinical remarks of Dr. Graves are extremely valuable; and Dr. Stokes's observations on abdominal inflammation have also an interesting relation to the subject.

For an excellent and comprehensive account of the *exanthematous fevers*, we have great pleasure in referring the reader to the work of Dr. Gregory from the thirteenth to the twentieth chapter inclusive.

Diseases of the Abdomen. No department of medicine has derived greater light from morbid anatomy than that which relates to abdominal diseases and the various remote affections dependent upon them. The discovery of the mucous form of enteritis, for which we are indebted to Pemberton, and which has unveiled the immediate cause of many serious visceral diseases and the chief source of danger in some of the

worst forms of fever, would alone constitute an era in the history of medical theory and practice.

In our comments on abdominal disease we shall chiefly follow Dr. Stokes, whose lectures on this subject are truly admirable, and evince so judicious an union of the views of Broussais with those of our own countrymen, so ample an experience, and so consummate a tact in discerning and treating disease, that we cannot recommend them too strongly to the student. In treating of *gastritis*, Dr. Stokes dissents from the opinion of Broussais that idiopathic inflammation of the stomach never occurs but in connexion with a similar state of the small intestine; he admits, however, the extreme rarity of pure gastritis, and attempts to account for it on the physiological principle that the stomach is an organ, the healthy functions of which require that it should be frequently in a state of great vascular excitement, and hence that it is adapted by nature to sustain with impunity an influx of blood, which would cause inflammation in another organ—otherwise every act of digestion would be followed by gastritis: the idea is ingenious, and as far as we know original. Dr. Gregory seems scarcely to admit the existence of gastritis, except as a peculiar affection of infants, and as the result of habitual drunkenness or poisoning. Drs. Bright and Addison consider it as a rare disease; Drs. Elliotson and Reid say nothing as to the frequency of its occurrence; the former, however, remarks that the whole of the inner surface of the stomach is seldom found inflamed, but only some particular part, and this corresponds with our own observation. We should say that partial gastritis is not at all uncommon, while universal gastritis is very rare, though not so much so as some authors would lead us to believe. We may add that we have sometimes found on dissection a limited portion of the gastric membrane inflamed in cases where there had been no symptoms during life to lead to the suspicion of such disease.

Dr. Stokes points out the error into which Abernethy and Broussais have led their followers by representing the state of the tongue as an unerring guide to that of the digestive tube. In opposition to this he cites the extensive observations of Andral and Louis on the state of the tongue in the gastritis of continued fever, from which they infer that there is no constant relation between the state of the tongue and that of the stomach. Dr. Stokes's experience has led him to a similar conclusion with respect to the idiopathic affections of the intestinal canal; and he considers the appearance of the tongue as of much more importance as an index to the condition of the general system than to that of any particular organ. On the whole we believe this opinion to be just. We are at all events quite convinced that serious disease of any part of the digestive apparatus may coexist with a natural state of the tongue. Of this truth, with reference at least to the stomach, we recently met with a striking example in a female patient, who had suffered for several years under an increasing affection of this organ, and who when she first applied to us had constant tenderness of the epigastrium, and acute pain at the cardiac orifice in the act of swallowing, with entire loss of appetite, debility, anxiety, and some emaciation—symptoms indicative of no very sound condition of the stomach: nevertheless this patient's tongue might have been selected as an example of a perfectly healthy one.

Dr. Stokes illustrates very ably the sympathetic affections which are liable to supervene on inflammation of the intestinal tube, especially in its upper portion, and directs attention to the important law that when such inflammation has subsisted for some time, and attained a certain degree of intensity, the original local symptoms may subside altogether, and the gastritis or enteritis be represented by the symptoms of disease in the organ secondarily affected, as the brain or lung, and yet the primary inflammation shall exist all the while in full force. He cites two illustrative cases. One is from Andral: the symptoms of gastritis were suspended on the supervention of tetanus which was speedily fatal; and on dissection no morbid appearances were found in the brain or spinal cord, but the inner surface of the stomach was intensely inflamed. The other case occurred in the Meath Hospital: the patient on admission was labouring under violent maniacal excitement, with bloodshot eyes, a ferocious aspect, dry, fissured tongue, quick, weak pulse, and constipated bowels; there was no tenderness of the epigastrium, no vomiting or other symptom of gastritis, though on the third day the belly was slightly tender and tympanitic; the cerebral symptoms increased, and death took place on the eighth day: on dissection extensive inflammation of the digestive tube was discovered, but none in the brain or its membranes. We well remember a case which we saw incidentally some years ago in one of the London Hospitals. The patient, a young woman, was lying in a state of profound coma, with fever and a rapid pulse. Being asked our opinion as to the nature of the case, and learning that there had been diarrhœa at the commencement, and that the cerebral symptoms had come on suddenly, and detecting obscure indications of pain in the abdomen from the uneasy movement of the patient when it was strongly pressed, we declared our belief that the intestines were the primary seat of disease. The eminent practitioner under whose care the patient was differed from us, adding that we should soon have an opportunity of ascertaining, as he had no doubt she would die. A few days after he told us that on dissection the brain had been found healthy, and the mucous membrane of the intestines highly inflamed.

If abdominal disease be sometimes masked by cerebral symptoms, it is no less true that affections of the brain may be accompanied at their commencement with extreme irritability of the stomach and bowels, as indicated by copious vomiting and purging. Of this every practitioner must have met with examples in the early stage of acute hydrocephalus. Dr. Graves notices a similar occurrence in scarlatina and typhus, and makes some excellent remarks on the diagnosis of such cases. The cerebral diarrhœa and vomiting generally set in at a very early period of the disease, as the first or second day, and are seldom attended with the red and furred tongue, bitterness of the mouth, burning thirst, and epigastric tenderness, which denote gastro-enteric inflammation; they are not relieved by leeching the abdomen; and they are particularly characterized by the discharge of bile which is much greater than ever occurs in gastro-enteritis. The last-mentioned symptom is very remarkable and, as Dr. Graves truly observes, is not confined to febrile affections, but occurs generally in vomiting from deranged cerebral circulation; thus, in sickness produced by swinging or sailing, immense quantities of bile are thrown up. From these considerations it is evident that in all fevers

which set in with vomiting and purging which there are no abdominal symptoms to account for, the practitioner ought to keep a very watchful eye on the state of the brain.

With respect to the treatment of gastritis, Dr. Reid is the only one of our authors who recommends large venesection as a general practice. He admits, however, in common with Drs. Elliotson, Bright and Addison, and Stokes, the great efficacy of leeches. Dr. Stokes advocates strongly the internal use of ice, and advises a portion the size of a walnut to be kept in the mouth till its angles are melted down, and then swallowed whole, and this to be repeated as often as the patient desires it. Drs. Bright and Addison, and Dr. Elliotson, also recommend the internal use of ice. Dr. Stokes observes that the fact of gastritis being occasionally produced by swallowing cold water or ice when the body is heated by exercise is no argument against the use of these substances when the inflammation actually exists. Dr. Elliotson remarks, too, after Currie, that the production of inflammation by the application of cold to the stomach after exercise does not arise from the person's being *hot*, but from his being *exhausted*—in which we believe there is much truth. The internal use of cold water and ice in gastric inflammation is not yet generally familiar to British practitioners, which is much to be regretted, for it is one of the most powerful means of cure, and conjoined with abstinence and rest is often sufficient, in slight cases, without any other remedy.

Dr. Stokes makes some interesting remarks on *hematemesis* and *delirium tremens* in connexion with acute gastritis. Hematemesis, he observes, may arise from various causes; it may be vicarious; it may be accidental, as from the rupture of a blood-vessel; or it may result from mechanical obstruction of the circulation: but there is a species of gastritis of which copious hemorrhage is a prominent symptom, or, in other words, an hematemesis of which gastritis is the immediate cause. In such a case, besides the vomiting of blood, there is tenderness of the epigastrium, fever, heat of skin, thirst, and longing for cold drinks. Here the use of astringents so commonly resorted to in hematemesis is highly injurious and may be fatal: the inflammation must first be allayed by leeches and cold drinks, and if the hemorrhage still continue, astringents may then be used to check it. We would particularly direct the attention of the reader to this point, because the case here described is by no means a very rare one; yet, as far as we know, Dr. Stokes is the only author by whom it is distinctly stated. Drs. Bright and Addison remark that a profuse discharge of dark blood from the stomach sometimes occurs in chronic gastritis, especially that variety of it arising from intemperance; but this is a different case, the hemorrhage here proceeding from a relaxed and congested, or, in a few instances, an eroded state of the vessels.

With respect to *delirium tremens*, Dr. Stokes lays great stress on the two opposite conditions under which it is known to occur—after a debauch, or on the sudden suspension of the habitual use of alcoholic liquors. In the first case he believes the pathological state to consist in gastritis, accompanied with high excitement of the brain and nervous system, in consequence of the absorption of alcohol, or from sympathy with the stomach, and tending strongly towards inflammation of the

brain. He considers this state as illustrative of the law just now adverted to, that inflammation of the stomach may be entirely masked under the sympathetic irritation of other organs; for the abdomen may not be tender, nor the tongue red, and all the symptoms may point to the brain as the seat of disease, and yet intense gastric inflammation may be going on all the time. The treatment is that of gastritis, and our author states that it has been used with extraordinary success in the Meath Hospital, where the most aggravated symptoms of delirium tremens have been subdued by leeches to the epigastrium and iced water. On the other hand it has been found in the same institution that in patients who died under the stimulant treatment, inflammation existed either in the stomach, or in the substance or membranes of the brain. In the second case the functions of the brain are disturbed by the abstraction of an accustomed stimulus, and the indication is to restore that stimulus: it is here, according to Dr. Stokes, that the ordinary practice of giving porter, wine, brandy, opium, &c., is appropriate and successful. From the foregoing considerations Dr. Stokes lays it down, as a general rule, that when delirium tremens arises from the want of the habitual stimulus, it is to be treated with stimulants and opiates; but that when it supervenes on an excess, the proper treatment is that of gastric inflammation. We are content to recommend these views to the careful attention of the reader without venturing on an opinion as to their general validity, for we have not had sufficient opportunities of dissection in such cases to enable us to form a decided judgment. We may remark, in passing, that admitting the pathology here laid down, we should still think considerable caution necessary in the use of depletion; for gastritis occurring in a drunkard, independently of delirium tremens, will not bear very active antiphlogistic treatment, and we are quite sure that we have seen cases of delirium tremens following a debauch destroyed by the lancet as well as others arising under the opposite condition.

Dr. Stokes's remarks on *chronic gastritis* are deserving of diligent perusal. On the whole we think he is perhaps disposed to refer too large a proportion of the cases ordinarily termed *dyspeptic* to an inflammatory state of the stomach; yet there can be no doubt that the cases to which this view justly applies are very numerous, and that incalculable mischief is continually done by the senseless use of purgatives and bitters, where a careful regulation of the diet, abstinence from all medicine by the mouth, leeches to the epigastrium, and the use of enemata to keep the bowels regular, would soon restore the patient to health. Dr. Stokes has also great reliance on counter-irritation effected by small blisters; a mild form of the tartar-emetic ointment; or the external application of croton oil, in the quantity of five or six drops, rubbed on the epigastrium, with a piece of lint or bladder interposed between the finger and the patient's skin. By the last-mentioned process an eruption of small papulæ is soon produced, which may be increased at will. Dr. Stokes observes that the action of the croton oil, as a counter-irritant, depends on its not being absorbed, for if it be absorbed, it will purge instead of irritating the skin.

In a few remarks on *cancer of the stomach*, Dr. Stokes expresses great doubt as to its true nature, and the validity of any distinction between this and common ulceration in various other parts. We entertain

no doubt that the orifices of the stomach are sometimes the seat of genuine cancer—by which we understand a morbid induration, consisting of a fibrous structure, the interstices of which contain a softer matter, the whole tending to malignant and incurable ulceration. We nevertheless believe that many cases of alleged cancer of the stomach, as well as of the rectum, have no title to that denomination, and are in effect mere instances of induration and ulceration of the parietes of a muscular canal from chronic inflammation commencing in the mucous coat, precisely analogous to the state of the urethra in cases of inveterate stricture. We cannot agree with Dr. Stokes that " in the present state of medicine we are not possessed of any data which would enable us to come to a final determination of this question." We apprehend that the case is one in which diligence of observation is the thing chiefly needed, and that any pathologist who had sufficient opportunities might confer a benefit on the science, by ascertaining in how many out of a large number of cases called cancer of the stomach, the true anatomical characters of carcinoma could be made out. We admit that it is not ascertained whether a peculiar matter be deposited in cancer as in tubercle and melanosis; but still we think the morbid formation, as a whole, has sufficiently distinctive characters. However this may be, we fully agree with Dr. Stokes, that the treatment best adapted to relieve suffering and prolong life, in the cases called cancer of the stomach, is that applicable to chronic gastritis. The case of the Emperor Napoleon, as described by Dr. Stokes, affords a painfully vivid delineation of the loose diagnosis and empirical practice too common in such diseases.

"He died with extensive ulceration of the stomach, which, of course, was called '*cancerous*,' and there were also distinct traces of disease in the liver, the mucous coat of the intestines, and the lungs. His disease was believed by himself to have originated in the stomach, and to this opinion he adhered, notwithstanding the results of some solemn consultations, at one of which his affection was declared to be an '*obstruction of the liver*' with a '*scorbutic dyscrasy*.' At another it was pronounced to be a '*chronic hepatitis*,' and a course of mercury recommended! When we reflect on this, and read in the account by Gaubert, (which you will see in the *Examen des Doctrines Médicales*,) the regimen which was used, you will not wonder at the words of this great man, when he was pressed to take more drugs, to swallow the universal nostrum mercury, to which he had the greatest aversion. 'Your disgusting preparations are good for nothing. Medicine is a collection of blind prescriptions, which destroy the poor, sometimes succeed with the rich, but whose results are more injurious than useful to humanity.' But he got mercury notwithstanding, mercury for his 'digestive organs,' to 'excite the liver,' to 'remove its obstruction,' and mercury to create bile, and purgatives to remove it; and tonics and antacids, and stimulants; and he died in torture, and his body was opened, and the stomach was found '*cancerous*.'" (p. 59.)

But we must proceed to other forms of abdominal disease. The diagnosis of *ileitis* may be cited as an example of the value of negative evidence. Independently of the general indications of abdominal inflammation, the peculiar symptoms of acute *gastritis* are vomiting and the desire for cold drinks, while inflammation of the *large intestine* is characterized by tormina, tenesmus, and copious vitiated secretions. In *ileitis*, on the other hand, though there be thirst, there is no particular preference for cold drinks; indeed the patient usually prefers them warm: diarrhœa may be present or not, but the tenesmus and

griping of colonitis are absent; in the latter disease also, the excretions are always highly vitiated, while in inflammation of the small intestine they are extremely various, and sometimes perfectly healthy. Gastritis and colonitis are attended with acute pain, though of very different character, while in ileitis the patient seldom complains of pain at all, though there is a slight tenderness on pressure over the situation of the inflamed intestine, especially between the umbilicus and crista of the ilium on the right side. In ileitis there is generally a tympanitic state of the abdomen. Ileitis is always accompanied by fever, and this is frequently of the typhoid type; indeed, as before observed, many cases called continued fever are nothing more nor less than simple inflammation of the small intestine. This point is properly insisted on by Dr. Stokes, whose remarks on ileitis are highly practical and instructive.

We are somewhat surprised to find Dr. Stokes among the number of those pathologists who consider *tabes mesenterica* as essentially connected with enteritis. We think also that his notion as to the prevalent pathology of the former disease is incorrect. He says, "the common idea formerly entertained with respect to this affection, and I believe still to a great extent is, that the disease first commences in the mucous glands, and from these extends to the lymphatic ganglia of the mesentery, which, in their turn, become enlarged, thickened, and less pervious, so that a sufficient share of nutriment cannot be absorbed, the consequence of which is that the patient dies of atrophy and exhaustion." We conceive that ever since tubercular matter has been generally recognized as a distinct morbid product, the current pathology of the disease in question has been that the mesenteric glands become tuberculated, under circumstances parallel to those in which other glands pass into the same state; and that, as in other cases, the deposition of the morbid matter may be preceded by inflammation or not, though the development of the diseased structure must necessarily, at some period of its progress, be accompanied by it. Let us examine the grounds on which Dr. Stokes embraces Broussais's pathology of tabes mesenterica.

"What is the actual state of the science with respect to this disease? It is found that the glands are certainly changed in their structure, and that they are manifestly enlarged; but this is only a link in the chain of phenomena, for it has been proved that in *the majority of cases the disease is ushered in by enteritis, and that the swelling of the glands is the result of disease, propagated along the course of the lymphatics, from the mucous surface of the intestines to the mesenteric ganglia.* This preparation, which I shall send round, will give you an idea of the actual state of the disease. Here is one of the glands which has been cut through; it exhibits the cheesy texture commonly observed in this disease, but you can perceive there are a number of lines running towards each of the glands; these are the engorged lymphatics, which you see correspond with ulcers on the mucous surface of the small intestine. That this is the true pathology of the disease will appear from the following circumstances:—First, it has been proved that the glands of the mesentery commonly become inflamed, enlarge, and suppurate in cases of inflammation of the mucous membrane of the intestinal canal in the adult. A patient gets enteric inflammation and dies; on dissection we find distinct marks of disease in the intestines, and, in addition to this, the glands are evidently diseased. Here is one fact. In the next place it has been proved that, in a great many cases of tabes mesenterica, if you retrace the history of the disease, if you go back to its first and earliest phenomena, you will find that it began with the symptoms of what has been termed remittent fever, or that

the patient had enteritis or diarrhœa, which afterwards became chronic, and that then the symptoms of tabes mesenterica began to appear. In the third place you will find that, in a vast number of cases where a fatal termination has occurred, if you pursue your dissection, and slit up the whole of the ileum, you will discover numerous old ulcerations of the mucous membrane, and find that the lymphatics which correspond with these ulcerations are in a state of manifest disease. Lastly, it has been observed that the best treatment for tabes mesenterica is that which is calculated to remove enteric inflammation, and that the old treatment, founded on the principle of removing obstruction, by the use of alkalies, absorbents, and solvents, is erroneous and false in the majority of cases. So that we have proof of the origin of this disease in intestinal inflammation, drawn from the occurrence of analogous affections in the adult, from the phenomena of the disease in its early stage, from morbid anatomy, and from treatment. I think there can be no doubt that, in most instances, it commences by intestinal inflammation. Of course a predisposition to disease of the glandular system will favour the occurrence. But is there no case in which the disease has commenced in the glands, and where the mucous membrane of the digestive tube is secondarily engaged? My answer to this question is, in a few cases we cannot prove that the disease commenced in the mucous membrane, and there is no reason why the glands of the mesentery should not be liable to primary tuberculous or scrofulous deposition, as well as those of any other part of the body; but in a vast number of instances, the enlargement of the mesenteric glands is secondary, and resembles the inflammation of the inguinal glands which results from chancre on the penis."

The opinion that disease of the mesenteric glands is an ordinary accompaniment of mucous enteritis in adults is neither sanctioned by the accounts of the best pathologists, nor, we think, by common observation. These glands, doubtless, occasionally become inflamed and suppurate in enteritis, and especially where it is a complication of continued fever, but we do not hesitate to affirm that the occurrence is comparatively rare. Moreover, admitting the universality of such lesion in enteritis, we cannot perceive the analogy between suppuration of a gland from acute inflammation, and the changes which take place from the deposition of tubercular matter in its texture; and that tubercular matter *is* deposited in the mesenteric glands in the majority of cases of tabes mesenterica, we hold to be as certain as any truth in pathology. It is spoken of as a simple matter of fact by Andral, Carswell, Sir James Clark, and many others; and if the pathologists we have named do not know tubercular matter when they see it, there is an end of the study of morbid anatomy. The mesenteric glands in the disease in question present exactly the same appearance, and their texture is infiltrated with the same matter as tuberculated bronchial glands; and we will answer for it that if two such diseased glands, of equal size, the one bronchial and the other mesenteric, were insulated, and presented to the best morbid anatomist in Europe, he could not tell one from the other. We do not deny the frequent coincidence of inflammation of the intestinal membrane with tuberculated mesenteric glands; but the observation of Andral has established that a similar state of the bronchial membrane is equally frequent when the bronchial glands are tuberculated, and in the latter case inflamed absorbents are out of the question as a cause of the glandular affection, because, of the lymphatics which enter the bronchial glands, very few come from the mucous membrane of the bronchi, since they are almost all derived from the peripheral surface of the lungs, or creep

along the outside of the bronchial tubes from their extremities. It is very probable, however, nay there is little doubt, that in both instances the occurrence of inflammation in the mucous membrane may determine the specific morbid action in the contiguous glands already predisposed to it, just as bronchial inflammation may stimulate into morbid activity the rudimentary tubercles which have long lain dormant in the substance of the lung.

We must further observe that disease of the mesenteric glands does not appear to be so frequent a sequel of the infantile remittent fever, or other forms of enteric inflammation, as Dr. Stokes's doctrine would lead us to suppose. We admit that it often occurs; but not, we think, much oftener than other strumous affections, as enlarged lymphatic glands, chronic abscesses, and diseased joints, which we agree with Drs. Bright and Addison in regarding as manifestations of a general scrofulous diathesis, occasioned by the continuance of febrile irritation, and determined as to their particular form and locality by individual predisposition. But after all, the observations of Broussais, and Henning, and Macintosh, and Stokes are not to be unceremoniously set aside, however apparently opposed to those of great pathologists; and we suspect the truth will turn out to be that, independently of the common tubercular form of tabes mesenterica, there is an inflammatory enlargement of the mesenteric glands in children consecutive on inflammation of the enteric mucous membrane, analogous to that first described by Petit and Serres, as an accompaniment of a fever occurring in Paris in the years 1811-13, and since repeatedly observed. Pemberton has described an enlargement of the mesenteric glands distinct from tubercle, arising in consequence of chronic peritonitis. "It frequently happens," says he, "that the mesenteric glands are enlarged, but I never saw suppuration in them or cheesy matter; so that I consider it a very different disease from a scrofulous affection of these glands, as it seems to be an enlargement of them, merely from irritation of the inflamed membrane with which they are surrounded."* Other writers, however, have described a tuberculated state of the mesenteric glands as frequent in chronic peritonitis.

It is evidently of much practical importance to ascertain whether there be indeed two distinct forms of disease in the mesenteric glands common among children: for assuredly the treatment of scrofula is not that adapted to enteritis, nor would strumous children derive much benefit from being treated, during a protracted state of atrophy, for an enteritis which had no existence, or was at most incidental and subordinate.

Our space will not allow us to follow Dr. Stokes through his delineation of the diseases of the large intestine. We should not, however, be justified in omitting to notice his observations on a particular form of diarrhœa connected with partial ulceration.

"It not unfrequently happens that a person, labouring under chronic diarrhœa, comes to consult a medical practitioner, and tells him that he has been suffering from this complaint for months, that he has eight or nine discharges by stool in the day, and that he has been under the care of five or six doctors in succession, without any benefit. Well, you are determined to have your trial, too, and you commence operations by putting him on full doses of acetate of lead. After a week or a fortnight, he comes back and tells you he is not a

* On Diseases of the Abdominal Viscera. Second Edition, p. 13.

bit better. You then try turpentine or balsam of capaiva—no use. Nitrate of silver—the same result. The man gets tired of you in turn, and perhaps goes to a surgeon to ask his advice. The surgeon examines the rectum carefully, and finds, at a short distance from the anus, an ulcer, which he immediately touches with a strong solution of nitrate of silver. The ulcer begins to heal, the irritation of the gut ceases, and the diarrhœa goes off. The surgeon is extolled to the skies, and the doctors disgraced for ever in the opinion of the patient. Now this is not an uncommon case. I have seen several instances of it, and I must tell you I was once mistaken in this way myself. These ulcers are situated close to the verge of the anus; they occur chiefly in persons of broken-down constitution, and those who have taken a great deal of mercury. They produce irritation in the colon, tenesmus, griping, frequent discharges by stool, and most commonly during the straining, a little blood is passed. During the course of last summer, I treated a soldier for this affection, who had been discharged from the East India Company's service (as was stated in his discharge) for incurable dysentery. I examined the rectum, and finding some ulcers close to the anus, had them touched with the nitrate of silver. Under this treatment a rapid amendment took place, and in the space of three weeks the man was discharged, quite cured. Now, are you to make this examination in every case? I believe you will act rightly in doing so in every case of chronic diarrhœa in the male, but the examination is absolutely necessary in all cases under the following circumstances: first, when the diarrhœa has been of long standing; secondly, when it has resisted a great variety of treatment; thirdly, when it has been combined with tenesmus and a desire for sitting on the night-chair after a stool has been passed, showing irritability of the lower part of the great intestine; and, lastly, when the patient's health does not appear to be so much affected as it naturally should be, where there was long-continued disease of a large portion of the great intestine. A patient will come to consult you, who will inform you that he has had eight or ten alvine evacuations every day for the last six months, and yet he eats heartily and looks quite well. Under these circumstances, the cause of the diarrhœa will generally be found to be ulceration of limited extent low down the tube, and capable of being quickly and effectually removed by a strong solution of the nitrate of silver." (pp. 87-8.)

Dr. Stokes's remarks on jaundice may be read even by the most accomplished practitioner with pleasure and instruction, though we cannot say we agree with him in regarding inflammation of the stomach and duodenum as the most frequent cause of that affection—an opinion derived from Broussais. It is not to be denied, however, that jaundice, in a greater or less degree, is one of the most prominent symptoms of duodenitis, and there is every reason to believe that inflammation of the stomach and duodenum is the most characteristic feature of the malignant yellow fever of the tropics. The epidemic which occurred in Dublin in 1826-7 bore the closest resemblance to yellow fever, indeed there can be little doubt of their virtual identity, and the same appearances of abdominal disease were observed on dissection. The cases which occurred in the Meath Hospital were described in a monograph by Drs. Graves and Stokes, and we need not here dwell on those portions of their lectures in which they are alluded to.

Some of the more obscure points in the pathology and treatment of hepatic diseases are ably commented on by Dr. Stokes, and illustrated by some very remarkable cases, among which is one of jaundice, arising from aneurism of the hepatic artery—another in which a distended gall-bladder was mistaken for an hepatic abscess pointing externally, and punctured accordingly. There are also some important surgical remarks on the mode of opening hepatic abscesses, in which it is shown that the

presence of a distinct fluctuating tumour, even with discoloration of the integuments, affords no certain indication that adhesion has taken place between the layers of the peritoneum, so as to render a direct opening into the abscess safe. Dr. Graves has invented an operation by which all danger of effusion is obviated in such cases. He makes an incision through the integuments over the most prominent part of the tumour, carries it nearly down to the peritoneum, but not quite, and keeps the wound open by plugging it with lint. The irritation of the wound causes the formation of adhesions internally, and after some time the abscess bursts with perfect safety to the patient. Such a proceeding has the advantage of being applicable to abscesses which have not yet distinctly pointed, and has a great effect in determining that occurrence by removing external resistance. We regard this as a most scientific and masterly operation, and would particularly recommend it to the attention of those (we hope they are now few) who regard a knowledge of surgery as unnecessary or derogatory to a physician. The cases in which the operation has been practised, as well as most of the other remarkable ones cited by Dr. Stokes in relation to diseases of the liver, have been published in the reports of the Meath Hospital.

Dr. Stokes notices a form of disease which is liable to be confounded with acute inflammation and abscess of the liver.

"There is one disease more which may be, and I believe has been, confounded with acute hepatitis and abscess of the liver. This affection, which has not been sufficiently noticed by authors, is inflammation and abscess of the abdominal parietes over the hepatic region; and this is a very singular disease. It is sometimes trifling, but I have seen a patient die of it. With the original nature of this disease I confess that I am not at all well acquainted; nor can I say whether the inflammation first attacks merely external parts, or whether it is a primary affection of the liver, and that the external parts take on diseased action from sympathetic irritation. In such cases we frequently observe many of the symptoms of inflammation of the liver, as pain, tenderness, biliary derangement, foul tongue, and morbid stools, with a tumefied state of the integuments. After these symptoms have continued for some time, the tumour increases in size, becomes softer, and matter forms. You give exit to the pus by opening the abscess with a lancet, and the patient gets well. This occurrence I have frequently witnessed. From a consideration of all the circumstances, it strikes me that in this disease the first morbid action in all probability commences in the liver itself, and that the external inflammation is an example of the strong sympathy which subsists between disease of deep-seated parts and integuments which cover them. Of this fact you have several illustrative instances. In pleuritis we frequently find the integuments of the chest remarkably tender on pressure; and in cases of inflammation of the brain the integuments of the scalp have their sensibility much increased. The same thing occurs in hepatitis; and in this disease one of the first distinct symptoms is this tenderness of the superincumbent skin. Now, you can conceive that, if this morbid sensibility of the investing parts should increase, in place of having some pain and tenderness, accompanied by swelling, we may have suppurative inflammation set up in these parts; and that, under such circumstances, the inflammation may leave the internal organ where it first existed, and be thrown upon the external parts in its vicinity. It strikes me that this is not unfrequently the case in this curious affection. In the case of this disease which I have seen prove fatal, the following circumstances were observed:—evident symptoms of inflammatory fever; pain and tenderness in the region of the liver, followed by the appearance of a tumour; which became fluctuating, was opened, and a quantity of matter discharged with considerable relief to the patient. She left the hospital, but returned again in

about a fortnight or three weeks, with an enormous tumour in the same place, which was again opened, and a vast quantity of purulent matter evacuated. Though the matter continued to flow out freely, she did not recover strength; and on enquiry it was found that before her second admission she had spit up some blood. One day, while dressing the abscess, the gentleman who attended her observed that when she coughed air passed out through the wound, proving the existence of a fistulous communication with the lung. On examination after death we found an abscess, the base of which rested upon the peritoneal surface of the liver, without engaging its substance. From this the matter had made for itself a double passage, one externally, the other through the diaphragm and pleura into the substance of the lung. This was the only case in which I have seen this disease prove fatal; and in it death appears to have been caused by the extent of the disease, and by the abscess opening into the pleura and lung. (*Lecture* xvi., pp. 138-9.)

Such affections bear an evident analogy to that in which the abdominal integuments over the cæcum become inflamed in consequence of a corresponding state of that intestine. A description of the last-named affection has been very properly introduced by Drs. Bright and Addison. We have noticed it fully in a preceding article.

But we must here bring our remarks on abdominal diseases to a close. We have been tempted to carry them to a considerable length by the excellent materials which Dr. Stokes's lectures afford. Independently of the original thought and observation which these lectures display, they would be of the highest value if regarded only as an admirable critical exposition of the most important part of Broussais's doctrine.

Diseases of the Circulating and Respiratory Organs. The pathology of the thorax is perhaps more advanced than that of any other region: yet our knowledge in this department is quite of recent growth; for less than a century ago the real nature of phthisis was unknown, the diseases of the heart scarcely recognized, pulmonary hemorrhage referred exclusively to the rupture of vessels, hydrothorax treated of as an idiopathic disease, and bronchitis known only as a peculiar affection of old age!

The rapid improvement in this department of pathology appears to be chiefly owing to the favorable and united relation which physiology and morbid anatomy have preserved towards it for a series of years. While the footsteps of disease in the thoracic viscera have been followed with unwearied assiduity both in France and England, the physiology of the circulating and respiratory systems has been cultivated with zeal and success by enquirers of all nations: thus the two great sources of pathological knowledge have blended in a powerful stream. Yet even here we have but too much reason to acknowledge the imperfection of medicine and the absence of any uniform relation between the science and the art; for despite our scientific knowledge of thoracic diseases, they are much less under the control of therapeutical agents than many others of whose nature we are comparatively ignorant.

In the few remarks we are about to make on the pathology of the *heart*, we shall not dwell on its particular organic lesions. A minute description of these is not to be expected in elementary treatises, and our authors in general are not diffuse upon them. Dr. Elliotson's lectures contain a good deal of information relating to them, but it is delivered in too off-hand a manner for so intricate and controverted a subject, and difficulties are sometimes too unceremoniously put aside.

Among the inflammatory affections of the heart, *pericarditis* has received its due share of attention from our authors; and if hardly anything is said of *endocarditis*, it must be admitted that there is not much to say, since our knowledge of the subject is very incommensurate with its importance, and the diagnosis of endocarditis from inflammation of the pericardium cannot be formed with any degree of certainty. Our limits will not allow us to enter on these topics, but we cannot pass by the subject of the heart without briefly noticing a very universal yet unaccountable oversight, which at length seems to have been perceived all of a sudden; for several recent writers have alluded to it in one shape or another almost simultaneously. It is this: while the pulse has long been regarded as an index to the state of every system and organ of the body, it has been in a great measure neglected as an index to the state of that very organ of whose action it is the immediate result—namely, the *heart.* Hence great practical errors have arisen in regarding certain states of the pulse as denoting certain conditions of the general system, when they are in reality principally or solely referrible to the condition of the central organ of the circulation. This truth has lately been placed in a prominent point of view by Dr. Hope, who appears to have entered on the investigation with his usual assiduity, and from whose enquiries we anticipate some valuable results.

In a late review of Dr. Holland's "Medical Notes and Reflections," we took occasion to point out the great importance, in cases of apoplexy, of discriminating the hard pulse, which denotes an entonic condition of the system from the jarring pulse of cardiac disease. We would here extend the principle of this remark to another malady, to which its application may at first appear less obvious—we mean *acute rheumatism.* If we consult practical writers as to the state of the pulse in this disease we shall find an extraordinary discrepancy. To go no further than the authors before us—Dr. Elliotson tells us that the pulse is soft and full (p. 738); Dr. Gregory that it is full, soft, and as it were *round* (p. 673); Dr. Reid says it is large, full, and hard (p. 413-15); and Drs. Bright and Addison that it is full, hard, and jerking (p. 562-83). All of course agree that it is frequent. The truth is, the pulse is various: nevertheless we think that there is a pulse generally characteristic of rheumatic fever, at least in its early stage; but we cannot regard any of the above descriptions as conveying an accurate idea of it. We should say that it was full, *tense*, and occasionally somewhat thrilling. It does, however, present a like variety with the pulse of nervous palpitation, and, we imagine, is influenced by the same cause—a morbidly irritable state of the heart. The opinion of Bouillaud that pericarditis or endocarditis exist in the majority of cases of acute articular rheumatism we hold to be altogether extravagant. If this were true, rheumatism would be a very fatal disease, which every one knows it is not, and patients would not recover under the most opposite modes of treatment as we daily see them doing. Nevertheless, the supervention of inflammatory affections of the heart is frequent enough to show that this organ is peculiarly liable to become implicated in the rheumatic state. But to the best of our observation the excitement of the circulation in rheumatic fever is usually more than commensurate with the other signs of inflammatory action throughout the system, and liable to variation from circumstances that would have little

effect if it resulted solely from an inflammatory cause; and we have occasionally found that the full and tense state of the pulse has been increased by large bloodletting: from all which we infer that independently of the inflammatory condition of the membranes which often supervenes, an irritable state of the muscular fibre of the heart is a prominent feature of rheumatic fever.

Much misconception with regard to metastasis has arisen from the neglect of auscultation. A patient is affected with acute articular rheumatism; the disease in the external parts subsides, but instead of the hoped-for recovery it is found that there is disease of the heart: in these circumstances it is said that metastasis has taken place, while, in reality, the pericardium or endocardium has been affected from an early stage of the disorder, if not from its commencement, but this important fact has been overlooked; the rheumatism, having run its course, has disappeared altogether in the external parts, because it has there no tendency to produce organic change, but it has left a fatal record of its presence in the very centre of life. We have no doubt that all this is familiar to many practitioners, but it has not hitherto been taught in the schools, and we have therefore great pleasure in directing the attention of the student to the following extract from the work of Drs. Bright and Addison, which we believe to contain a very correct view of the subject:

"The organs and parts which have been observed to be affected with inflammation in connexion with rheumatitis, are the heart, the brain, the lungs, the stomach, the bowels, and the sclerotic coat of the eye. Indeed, a morbid condition of the heart is so very familiar a complication in rheumatitis, that it appears to be more correct to regard it as constituting a part of the rheumatic disease than as an accidental and only occasional occurrence from metastasis. The morbid condition of the heart, nevertheless, is by no means uniform in its kind. In almost every case of acute rheumatism, we find the organ remarkably irritable, as indicated by the violence of its action, and by the facility with which it is thrown into a state of considerable excitement; a degree of excitement sufficient in some instances, especially after copious depletion or in chlorotic subjects, to produce a distinct *bruit de soufflet*, calculated to mislead to a belief in the existence of some more serious lesion. This morbid irritability is, however, of little moment, and usually subsides as the disease is subdued by appropriate remedies. In other cases, but much more rarely, the heart is liable to be seized with a severe crampy or neuralgic pain, which proves exceedingly distressing to the patient, and is compared to a grasping or squeezing of the organ: this is for the most part of short duration, and generally yields speedily to moderate depletion. A much more common, and unfortunately a much more serious affection, is that which is regarded as the ordinary metastasis to the heart. This consists in acute inflammation of the external or internal membrane of the heart, or of both of these structures at the same time, constituting accordingly pericarditis or endocarditis or both. These complications are by far more common than is generally supposed, and are very frequently altogether overlooked, in consequence of the obscurity of the symptoms to which they give rise in many cases. The student ought at all times, therefore, to be attentively on the watch; and by taking alarm at the slightest indication of disturbance of the heart, and by a judicious application of auscultation, endeavour to steer clear of that which must at all times prove a most serious, and not unfrequently a fatal oversight; always bearing in mind also, that in proportion to the youth of the patient, is the complication more likely to take place." (pp. 564-5.)

Not only is it true that certain pathological conditions of the heart itself may so affect the pulse as to lead to erroneous impressions as to the

general state of the vital powers, but it is true also that the condition of the heart, as remotely influenced by disease of the brain, may lead to similar errors. This is well illustrated by Dr. Graves, who gives a striking case of acute disease of the brain consisting, as appeared on dissection, in sanguineous effusion, ramolissement, and extensive suppuration, attended with great arterial excitement and a hard pulse, in which, nevertheless, the abstraction of fourteen ounces of blood was followed by sudden sinking of the pulse and death in about two hours. (p. 53.)

We must notice yet another point relating to the heart which is neglected by the majority even of good practitioners, to wit, the importance of attending to the *natural condition of the organ*, which varies considerably in different individuals. Bouillaud, Bizot, Clendinning, and others have been at much pains to ascertain the mean size and weight of hearts, the mean thickness of their parietes, &c., and the observations of Bizot are particularly valuable as tending to establish a difference of dimension at different ages. But in this, as in many other instances in which arithmetic is applied to medicine, we arrive after all at a mere abstraction: thus the mean thickness of fifty hearts may be represented by a number which does not indicate the actual thickness of any one of them; and in our anxiety to establish this mean, we lose sight of the *diversity of the individual hearts*, which is a matter of much more practical consequence. This remark will not appear frivolous if we reflect how small a difference in the thickness of the ventricular parietes will make an appreciable difference in the sound and impulse, and how small in truth is the actual difference of thickness between a healthy ventricle and one in a state of incipient hypertrophy. Now, in examining a patient's heart, we may find a feeble impulse and a somewhat greater extent of sound than usual, and may hence be led to suppose that there is a degree of passive dilatation resulting from disease; but in many individuals the parietes of the heart are naturally attenuated and its muscular power comparatively small—conditions which will often be found in conjunction with a general laxity of fibre and a weak and flabby state of the muscles throughout the body. On the other hand, the walls of the heart may be naturally strong and thick, the impulse powerful, and the sound limited. In such cases as these we are not to infer from slight deviations of the impulse or sound from the ordinary standard, that there is disease of the heart or even a tendency to it. Yet we affirm with submission, but at the same time with confidence, that we have known celebrated auscultators led into gross mistakes, and much needless anxiety occasioned to patients and their friends, by want of attention to these congenital peculiarities in the structure of the heart. A patient has palpitation from some sympathetic and transient cause; the auscultator detects a somewhat greater or smaller strength of impulse or extent of sound than is normal, and hence infers incipient organic disease; whereas if he had been previously in the habit of examining the action of the heart in this patient, he would know that matters had never been otherwise. Shall we be forgiven for insinuating that under such circumstances the fancy sometimes gains an ascendant over the hearing; divers *bruits* which have no existence are recorded, and the patient is frightened out of his wits because his heart is—as nature made it. Every practitioner, therefore, who is in the habit of attending a patient, should

make himself minutely acquainted with the natural peculiarities of the heart as well as those of the pulse.

For a correct and comprehensive general view of the diseases of the *respiratory organs* we may refer the student particularly to the work of Drs. Bright and Addison, who appropriately introduce the subject by some observations on the different forms of catarrh. It is curious with respect to the treatment of the ordinary acute form of this affection, that while the copious use of diluents is commonly recommended, Dr. B. C. Williams has found a remedy in a total abstinence from liquids for thirty-six or forty-eight hours. Dr. Reid states that he has adopted the latter plan with success, and we have heard the same statement from several intelligent practitioners.

In treating of *acute bronchitis*, it is usual to represent the extent of membrane affected as the principal measure of the danger of the case. We think, however, that this is not quite a correct view of the subject, and that much more depends on the situation and physiological relations than on the extent of the inflamed surface. The mucous membrane may be inflamed throughout the larger bronchial tubes, and yet the patient shall not be seriously ill; but the moment the disease extends to the smaller ramifications it becomes dangerous: in the former instance we have merely inflammation of a certain extent of membrane which is not implicated in the performance of any function immediately essential to life; but in the latter instance respiration is impeded by the tumefaction of the membrane in the minute tubes, and the effusion from its surface. In a great majority of fatal cases of bronchitis, the real cause of death is asphyxia; the patient is said to *sink*, but he is in fact suffocated. Want of attention to this important difference in the anatomical relations of the respiratory membrane in different parts of its course not only blinds us to the chief source of danger, but leads to discrepant and erroneous views of practice. It is currently said that bronchitis will not bear full bleeding; Dr. Elliotson, however, tells us, with reference to common acute bronchitis, that we "have only to bleed the patient well and follow it up by local bleeding." Dr. Reid also recommends the free use of the lancet. Dr. Gregory remarks that "the urgency of the symptoms appears to demand the copious abstraction of blood, but the constitution seldom bears it well." Drs. Bright and Addison, considering that the inflammation is in a mucous membrane, and that inflammation in this tissue is little under the control of depleting measures, generally running a course of indefinite duration in spite of remedies, though it be subdued in degree—that the morbid secretion obstructs the bronchi, and seriously interferes with the functions of the lungs—and that the patient has, therefore, to pass through a longer or shorter period of exertion, distress, and restlessness—arrive at the conclusion "that all remedies calculated greatly to impair the patient's strength, should be employed with the greatest reserve, and that even in the most acute attacks, the period of active depletion is likely to be of very short duration." According to our own observation when the inflammation is situated in the larger tubes, and the patient's constitution is firm, bronchitis bears bleeding perfectly well, and this remedy may be applied under the same regulations as in any other disease; but when effusion is going on rapidly in the minute bronchial tubes, the case

is widely different; every organ and tissue of the body is suffering from deficient aëration of the blood, and all the powers of life require to be supported and stimulated: in addition to this we may remark that although bloodletting be a powerful means of arresting effusion from the surface of serous membranes when acutely inflamed, there are many cases in which it increases the disposition to morbid secretion—as, for example, where suppuration has commenced in the parenchyma of the lungs or liver; and in this very case of increased mucous secretion into the minute bronchial tubes.

It is not our intention to dilate on those numerous affections of the air-tubes in which spasmodic action and vascular derangement are liable to be so blended or alternated, that the opinions of pathologists have long been, and probably will long continue to be, divided as to their essential nature, and as to the legitimacy of specific distinctions among their various forms. We may, however, remark, that if we consider on the one hand that the pneumogastric nerves appear to have a powerful influence on the action of the vessels throughout the lining membrane of the air-passages, and on the other hand that the whole respiratory system of nerves are engaged in various and constant reflex actions, so that irritation of the membranous surface readily produces spasm or other morbid condition of the muscular parts of the apparatus, it is easy to understand that there are numerous cases of disease in which it is nearly impossible in a pathological, and not very important in a practical, point of view to determine whether nervous or vascular derangement have generally the precedence—seeing that in a case of any duration both must almost inevitably be present, and the treatment must be regulated according to the actual predominance of one or the other. We may cite as examples *croup and asthma.* With respect to spasmodic croup, or *laryngismus stridulus*, we think that the opinion of the late Dr. Ley, as to its peculiar pathology, has made sufficient impression to entitle it at least to notice in elementary works on the practice of physic. None of our authors, however, make any allusion to it except Dr. Reid, who says that the malady "is more generally believed to arise from a state of irritation of the eighth pair of nerves, depending on a diseased state of the contiguous glands, the cervical and bronchial." But here too much influence appears to be assigned to Dr. Ley's doctrine, which we do not think is generally received, and which, in its full extent, we are certainly not disposed to admit, because, although scrofulous children may in general be most subject to the disease in question, we frequently meet with it in children who present no indications of this diathesis, in whom there is no disease in the cervical glands, and no reason to suspect it in the bronchial. But we do not consider that part of Dr. Ley's hypothesis which relates to the glands as the most important; it is the notion of the paralyzed state of the muscles of the glottis as opposed to that of spasm which, in our opinion, constitutes the point most worthy of consideration.

The diagnosis of *pneumonia* is ably commented on by Drs. Bright and Addison, who very truly observe that it is by no means so easy as some persons have imagined. They dwell especially on pungent heat of the surface as the most invariable and conclusive diagnostic, and think that where inflammation *is confined to the chest*, however various may be the textures involved, the presence of this symptom may be taken as a certain

indication of pneumonia in nineteen cases out of twenty. We believe this opinion to be just with reference to common pneumonia, and it is doubtful whether the disease called *typhoid pneumonia*—in which the skin is cool—forms any real exception, since there is some reason to believe that the pneumonia in this case is merely a complication of a malignant fever, of which enteric inflammation also forms a prominent feature. Their account of the physical signs of pneumonia is excellent: the following description of the mucous rattle, as heard through the medium of a hepatized portion of lung, though the similies be anything but poetical, will strike the reader accustomed to auscultation as particularly true to nature.

"The mucous rattle attendant on pneumonic consolidation becomes, in some instances from the latter circumstance, remarkably striking and characteristic, each inspiration conveying to the ear of the auscultator a sound resembling that produced by raising a flat fish from a moist and unctuous slab; or perhaps a sound so coarse, as rather to resemble the whizzing and gurgling occasioned by a cow suddenly raising its flat foot out of a mass of mud." (pp. 239-40.)

Dr. Graves makes a remark on percussion as diagnostic of solidification of the lung, which we believe is new—namely, that although the positive evidence of condensation of the lung afforded by percussion may be depended on, the absence of such evidence is no absolute proof that the lung is not solidified. He states that he has met with several instances in which percussion elicited a very clear sound from the parietes of the chest, where there was considerable solidification of the lung within. The following case he thinks affords a solution of the phenomenon:

"An old man, named Foy, died lately at Sir P. Dun's Hospital, of hepatization of the inferior lobe of the right lung, with numerous tubercular depositions in the upper lobes of both lungs. During his illness, I pointed out the existence of extensive hepatization of the lower lobe of the right lung, in which perfect and decided dulness marked out accurately the space occupied internally by the solidified pulmonary tissue. But anteriorly and above, the parietes of the chest returned a clear sound on percussion, nor could a vestige of dulness be anywhere detected. Yet the whole of the upper lobes of this patient's lungs were occupied to such an extent by crude tubercles, that no portion of the upper lobes could be selected, equal to half the size of a fist, which would not sink in water. This was owing to tubercular matter, which occupied the pulmonary tissue in detached infiltrated masses, or in single crude tubercles. How, then, did it happen that such extensive solidification of the upper lobes existed without any corresponding dulness on percussion? A careful examination of the pathological condition of these lobes satisfactorily explained the anomaly. On accurate inspection, we found that although the solidified masses of the pulmonary tissue were extremely numerous, and predominated over the parts which still retained their natural vesicular texture, so that an extensive portion of the upper lobes seemed to be quite solid, yet the solidified portions were insulated and divided from each other, throughout the interior of the lobe, by intervening laminæ of healthy pulmonary tissue, and on their surface were, for the most part, covered by a stratum of healthy vesicular lung, from a quarter to half an inch in thickness. Indeed, although the solidified masses (to use a geological expression) sometimes cropped up, and came to the surface, yet this was comparatively a rare occurrence; and by far the greater portion of that surface was composed of a thin stratum of pervious vesicular tissue. To this was owing the clear sound elicited by percussion. You will recollect, therefore, that in certain (I will admit rare) cases of tubercular deposition in the lungs, the tubercular development may have proceeded to the extent of rendering the greater portion

of the upper lobes impervious to the air, and may have solidified those lobes considerably, and yet the solidified portions may be so divided from each other by laminæ of healthy lung, and may be so covered by a stratum of vesicular tissue, that the general result of percussion is to elicit a clear sound over the whole of the parietes of the chest corresponding to the affected lobes."(pp. 355-6.)

On the treatment of pneumonia we quote the following from Dr. Elliotson, because it actually contains all he says about the matter, and presents a characteristic specimen of the summary style in which he sometimes disposes of a subject:

"It is only the treatment of any inflammation. Patients have sometimes borne the loss of an immense quantity of blood; perhaps more in this disease than in most others. It is in this affection that those enormous bleedings are reported to have taken place which I mentioned when speaking of inflammation in general—a few gallons in the course of a few days. Mercury is of the same use in this affection as in bronchitis, and in bronchitis as in other inflammatory diseases." (*Ed. Cooke and Thompson*, p. 503.)

We have been accustomed to consider pneumonia as perhaps the one of all purely inflammatory diseases which most severely exercises the judgment of the practitioner as to the precise point to which bloodletting ought to be carried; for there is assuredly, in many cases, a period at which the inflammation, though diminished by active depletion, continues in sufficient force to threaten a fatal result, but where, nevertheless, the further use of bloodletting would only hasten disorganization. Under these circumstances, we conceive it is a matter of some importance to determine whether there be any means which may take the place of bloodletting, when this ceases to be applicable. It is somewhat remarkable that neither Dr. Elliotson nor Dr. Gregory so much as mention the contra-stimulant treatment. Dr. Reid observes that this practice has been so much less successful in England than in France as to lead to the supposition that there must be something different in the character of pneumonic disease in the natives of the two countries. We apprehend, however, that the comparison of this with the ordinary antiphlogistic treatment has been made from data somewhat different in England and on the continent. We think it may be said, without prejudice, that the lancet is used more vigorously, and at the same time more cautiously, with a stricter regard to scientific principles, and consequently with greater success, in England than in most other countries. We can, therefore, easily conceive that a comparative estimate of the effects of bloodletting and emetic tartar might lead to conclusions more favorable to the latter in France or Italy than in this country. Dr. Reid states that he should, "from considerable experience of both modes of treatment, be undoubtedly inclined to avoid it [the contra-stimulant] altogether, except in those cases where the use of the lancet could not be practised." (p. 328.) This is rather ambiguously expressed. If it be meant that tartar-emetic is useful in cases to which the lancet would from the first be inapplicable, we do not think the opinion just, for we believe that this medicine does good exactly in the same cases as the lancet; that is to say, where the state of the system is entonic and the crasis of the blood firm: if it be meant that the tartar-emetic may be serviceable where the lancet has been used as far as it could be with safety, we admit this to be true.

We must not omit to notice a new practice recommended by Dr. Graves, in the *scrofulous* form of bronchitis and pneumonia. From the success of Dr. O'Beirne's treatment of scrofulous inflammation of the joints, Dr. Graves was led analogically to try if similar inflammation of the lung might not be arrested by the bold use of mercury. The result of the experiment has been satisfactory; and he believes that he has often succeeded, by this practice, in checking incipient phthisis. We must confess we should have supposed this treatment of all others the most likely to precipitate the patient into phthisis. We admit, however, the insufficiency of *à priori* reasoning in the practice of medicine.

Diseases of the Nervous System. On these much valuable information is diffused through the volumes before us. The best connected view of them, we think, is that comprised in the second part of Dr. Gregory's work; and to this we must content ourselves with merely referring, since to enter with any effect into the details of such subjects would require a number of this journal instead of an article. Two very important general points have recently occupied a large share of the attention of philosophical physicians: the one is the existence of the same morbid state of the cerebral functions in connexion with the most opposite conditions of the vascular system and of the vital force; the other is the reflection of nervous actions through the medium of certain portions of this system. The illustration of the former of these subjects, and of the latter also in its pathological relations, is almost exclusively due to English writers.* Their further prosecution will probably lead to material changes both in the theory and treatment of nervous disease.

We regret much that the portion of Dr. Stokes's lectures which we have received breaks off soon after he commences the subject of nervous pathology. His introductory observations are admirable. On the nature of the pure *neuroses*, or those diseases of the nervous system in which the most careful examination after death can detect no structural change in any of its parts, two opposite opinions have been held: the one ascribes them to some peculiar modification of nervous influence, or, in other words, some alteration in the manifestation of the vital power, independent of any molecular change; the other refers them to molecular changes which are so minute as to elude our research. The following remarks of Dr. Stokes contain an analogy so new and ingenious, that we cannot do better than lay them before the reader in his own words:

"You must have been already convinced that it is difficult to form any clear or definite notion of the nature of neuroses; indeed, the only thing we can say of them is, what they are not. When we reflect on nervous phenomena, and consider how occult, how mysterious, the properties of those organs which give rise to them are, we are struck with astonishment at the discrepancy between cause and effect. No medical man has ever witnessed a case of confirmed tetanus or hydrophobia, without being oppressed with a conviction of the imperfect and limited state of our knowledge of nervous disease. It may be very possible, that in these neuroses the change, though so slight as to escape our means of detection, does absolutely occur; and yet such is the nature of nervous phenomena, that we must admit that great and extraordinary effects are produced by very slight causes. Do we see anything like this in nature?—any remark-

* Principally Whytt and Marshall Hall.

able alterations in properties depending upon apparently slight causes? We do—we see extraordinary changes taking place in the characters of various inorganic substances, (to which I need not particularly allude,) and there is no reason why the same thing should not occur in organic structures. On considering the doctrine of Isomerism, I should be inclined to think that it throws some light on this obscure subject. In chemistry, it is a well-known though singular law, that the properties of two bodies may be essentially different at the same time that their respective component elements are, as far as our knowledge goes, identically the same; and the change, whatever it may be, appears to result, not from the abstraction or removal of any of the component atoms, but from their peculiar juxtaposition. Now, it being admitted in chemistry that many bodies having the same constitution possess totally different properties, and this difference being explained by the different position of their elements, it does not seem strange if the same thing should take place in the phenomena of organized beings; and, if this be the case, we have a key towards elucidating the nature of these neuroses, and can conceive how an analogous change—a difference in the arrangement of the molecules of the component parts of the nerves, or their centres—may produce new modifications of their properties, without making any distinct change in their nature, or adding or abstracting a single organic molecule. I am much inclined to adopt the opinion of those who think that, in the neuroses, a peculiar organic change actually takes place, though we cannot demonstrate its existence; because, to reason on the phenomena of animal life, independently of organization, is to plunge blindly into hypothesis, and retrace the errors of an antiquated and exploded school." (pp. 198-9.)

But we must not indulge further in theory: we proceed, therefore, to the last subject which it is our intention to notice, namely, *Diseases of the Spinal Cord.* These we advert to chiefly to express our regret at the extraordinary neglect they have met with in this country. While treatises on spinal distortion are appearing every day, and the somewhat intangible subject of "spinal irritation" has received of late more attention than is perhaps due to it, the inflammatory and organic affections of the spinal cord and its membranes are almost entirely overlooked. Except the valuable contributions of Dr. Abercrombie, and a good article by Dr. Todd, in the Cyclopædia of Practical Medicine, there is, we believe, nothing like a treatise on the pathology of the spinal cord in the English language, and the greater part of our elementary writers on the practice of physic seem to have excluded it as by common consent. The diseases of this organ are not indeed very frequent, but they are quite as much so as some others on which it is usual to comment. Among the writers before us, Dr. Reid has devoted a page and a half to "arachnitis spinalis," but it is not very clear which of his remarks apply to the acute and which to the chronic form of the disease; nor do we think they can convey to the mind of the student any very distinct notion of either. Dr. Graves gives the details of two remarkable cases of ramolissement of the cord connected with the retrocession of gout from the extremities. He intimates his belief that gouty inflammation may seize on the nerves and neurilemma, and be propagated along their course to the spinal cord and its sheath. This he conceives to have occurred in the cases alluded to, though we are not sure that he has succeeded in making out the point distinctly. In favour of the existence of a gouty affection of the spinal cord, he adduces some cases recorded by Dr. Copland and Dr. Prichard; he has, however, fallen into a mistake with respect to Dr.

Prichard's cases, which were not rheumatism but *chorea*, or an affection resembling it.*

It will be thought incumbent on us, in taking leave of our authors, to pass some opinion on the general merit of their respective works; and it is very gratifying to us to be able to speak with high commendation of them all. It is but justice to Dr. Elliotson to say, that we know not how far the rival editions of his lectures contain a faithful transcript of the originals. Dr. Rogers's book may be considered as a work of his own, founded on Dr. Elliotson's lectures, rather than as an edition of the latter. There is no division into separate discourses, and the editor states in his preface that he has altered the phraseology throughout: he has prefixed some introductory remarks, likewise altered, from the author's Lumleyan lectures; and he has added an appendix, derived from heterogeneous sources, which we think might as well have been omitted. We consider this parasitical species of editing as extremely objectionable, except in the case of standard works which have become more or less obsolete, and require remodelling to adapt them to the actual state of knowledge. No one can suppose that Dr. Elliotson's lectures stand in this predicament. In the edition by Drs. Cooke and Thompson, the lectures are preserved in their distinct form; and we presume that they do not differ materially from the originals, since we trace in them the terse and facetious mode of expression which characterized those lately delivered by the author, and which, together with their fulness of information, rendered them deservedly popular. The work of Drs. Bright and Addison, as far as it has yet proceeded, fulfils admirably the intention with which it was undertaken, and we know of none which we can more strongly recommend as a text-book for students. The subjects flow easily into each other, and the information is accurate and copious, without too much minuteness of detail. Dr. Reid's book, though not tastefully written, evinces much pathological and practical knowledge, and contains many sagacious remarks. We have seldom met with a work which condensed so much valuable matter in the same space. Dr. Gregory's "Elements," though rather too concise on some points of pathology, are very uniformly good: the practice recommended is for the most part highly judicious, and the style of writing is correct and perspicuous. The lectures of Drs. Graves and Stokes, though now presented for the first time to the public in a connected form, are already too well known to need any commendation from us. Our frequent reference to them in the course of this article is a sufficient proof of the estimation in which we hold them.

On comparing the works which we have been examining with each other, and with the majority of those which have lately issued from the press, it is encouraging to perceive a great increase of unanimity in the view of authors, especially with respect to the more important points of practice. This may be regarded as a clear indication of an improved method of observing, and as an earnest that something like certainty may yet be hoped for in medicine, despite those who sneer at its fallacious evidence, its changeful theories, and its meager resources.

* See Medical Repository, vol. xxi.

ART. X.

Mikroskopische Untersuchungen über die Uebereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen. Von Dr. TH. SCHWANN. Mit 4 Kupfertafeln.—*Berlin*, 1839. 8vo, pp. 270.
Microscopical Researches respecting the Identity of Structure and mode of Growth in Animals and Plants. By Dr. THOMAS SCHWANN.—*Berlin*, 1839.

THE ardour and success with which the study of anatomy and physiology has been prosecuted at Berlin during the last few years are well known. Of the zealous enquirers who have contributed thus to raise the character of the university of the Prussian capital in the departments of science which we have mentioned, the author of the work before us is one of the most industrious, and, judging from the quality of his labours, one of the most ingenious and scientific.

M. Schwann has already gained a reputation in England by his important observations on digestion, published in Müller's Archiv for 1836; but as early as 1834 he had distinguished himself by the performance of well-directed experiments, which seemed to demonstrate in a conclusive manner the necessity of the access of atmospheric air for the development of the embryo of the bird.* Since that time, he has made known the results of several investigations into the microscopic structure of the muscular and other tissues, and very recently published some highly ingenious experiments,† which go far to establish the fallacy of the hypothesis of equivocal generation as hitherto maintained. The work before us is, however, of a very different character from any of those productions, since its object is not to demonstrate an isolated fact, or to determine the accuracy or inaccuracy of a doubtful hypothesis, but to establish the existence of a hitherto unknown but important law, of general application to all organized beings.

"The object of the present treatise," says M. Schwann in his preface, "is to demonstrate the intimate relation in which the two kingdoms of organic nature stand to each other, by showing the identity of the laws of development of the elementary parts of animals and plants. The main result of the enquiry is, that the law of development of the elementary parts of all organisms is the same, just as all crystals, however different their forms, are developed in accordance with the same laws." (p. 4.)

Analogies between the elementary composition of animals and that of plants have been frequently pointed out; but the more recent advances in minute anatomy have tended to prove that no real similarity exists between the elementary parts of these two great classes of organized bodies. Thus, it had been said that cellular tissue formed the groundwork of animal as of vegetable textures; but, on more accurate investigation, it appeared that the cellular tissue of animals was a very different structure from that of plants,—that while the latter was composed of vesicles or cells closed on all sides, the former consisted essentially of fibres aggregated into bundles and laminæ, and interwoven so as to

* De necessitate aeris atmosphærici ad evolutionem pulli in ovo incubito. Dissertatio Inauguralis, 1834. Berol. iv.

† Poggendorf's Annal., 1837. Bd. xli., p. 184.

inclose cavities which communicated freely with each other. Again, it had been asserted that the great bulk of animal tissues was made up of the same elements as those constituting the whole vegetable organism, namely, membrane and fibre; but the microscope has shown that the most abundant fibres of plants, the woody fibres, are really membranous tubes, and on the other hand that the membranes of animals which were compared to the simple vegetable membranes, are compound structures formed by the interlacement of fibres. More recently it has been affirmed that the ultimate elements of all the solid textures of animals, as well as of plants, are globules,—that the fibres of animal cellular tissue, for example, are mere strings of globules; but the appearances on which this statement was founded are now known to be in many instances illusory, and dependent on the imperfection of the microscope employed, or on inattention to certain necessary precautions in its use.

In fact, in proportion as the microscope has been improved, and the knowledge obtained by its means consequently rendered more accurate, the positive characters of the elementary tissues of animals and plants have appeared more and more dissimilar. The simplicity of vegetable structures, as M. Schwann remarks, has become more manifest. The plants of one entire class have received their designation of "cellulares," from the circumstance of their being entirely formed of cells. And although the perfectly-developed "vascular" plants contain other elements, namely, woody fibre, vessels, and ducts, yet these are now known to be cells altered in form, either by simple elongation or by the coalition of several to constitute one tube. In animals, on the contrary, a more intimate acquaintance with their minute structure has seemed only to reveal a greater and greater multiplicity of component elements, perfectly distinct from each other, and still more unlike the closed cells of vegetables. It is true that, before M. Schwann commenced his researches, some animal tissues had been observed to resemble, very accurately, the cells of vegetable parenchyma in form. Thus, the similarity of the closed cells composing the adipose substance to the vegetable cells, could not escape notice; some forms of epithelium and of cartilage (that of the branchiæ of the tadpole) had been compared by Purkinje and Valentin* to vegetable cellular tissue; and the perfectly plant-like form of the cells of the chorda dorsalis of young embryos and fishes had been pointed out by Müller.† These facts, however, appeared of little importance, and no general principle was deduced from them. It was not, indeed, in their perfect forms but in their mode of development that the great points of resemblance between the tissues of plants and those of animals were to be found. This was perceived by M. Schwann, and suggested to his mind the idea of comparing the growth of animal and vegetable cells, with the view of demonstrating a general law of development for all organic elementary tissues.

"When I entered upon my enquiry," he remarks, "there appeared to be no unity in the mode in which the organic molecules were aggregated together to form the different tissues. Here they met to form a fibre, there to form a cell, and here again to constitute a globule. The principle of development appeared to be totally different for all elementary parts of which the physiological en-

* Entwickelungs-geschichte, pp. 209-10. † Anatomie der Myxinoiden, p. 74.

dowments were not the same. And while the laws regulating the development of a cell and a fibre were admitted to be different, it was necessarily supposed that those to which the development of the different kinds of cells or of the different kinds of fibres was subject, were also different, though not in the same degree." (p. 12.)

The observations of M. Schleiden,* however, on the development of vegetable cells, revealed the existence of certain definite steps in the process, and afforded to Schwann adequate data for comparing the mode of growth of cells in the animal and vegetable organisms. He was thus led to the discovery, "that all the manifold forms of animal tissue are produced by the transformation of cells, wholly analogous to those of plants in structure, and for the most part agreeing with them most remarkably in the phenomena of independent vegetative life, which they present." (p. 2.)

Before proceeding to the consideration of M. Schwann's observations on the different animal tissues, we shall lay before our readers some account of those of M. Schleiden, on which they are based, and to their accordance with which they owe much of their interest and importance.

All newly-forming tissues of plants appear to take their origin from *gum.* This substance is in some instances directly supplied by the operation of the formative processes upon the nutritive materials derived from without. More frequently, however, it has passed through the intermediate condition of *fecula* (starch), or one analogous to it. Starch, in the plant, appears to take the place of animal fat. It is superfluous nutritive matter, deposited for future use; and it is found most abundantly in those parts of the structure in which, after a short repose, a new formative process is to commence; whilst it is absent in those which are in a state of active growth. Much diversity has existed between the accounts recently given of the nature of this substance. According to Raspail, it consists of membranous vesicles containing a gummy fluid; these vesicles being formed within the cells of the plant, and taking their origin from them. The later researches of M. Payen, however, leave but little room for doubt that the supposed membrane cannot be regarded as having an organic structure; but that it is rather the concrete exterior of the gummy matter deposited in the cell, often thickened by a coating of other substances. The increase of the granule takes place by absorption into its interior; so that there is a series of concentric layers, in each granule of fecula, gradually approaching the fluid character as we advance from without inwards. Starch only presents this definite aspect, however, when slowly deposited in the internal parts of plants, where it is to remain unchanged for some time. In cells more actively concerned in the formative processes, such as those of the leaf, the pollen, and the albumen of the seed, we find it existing in smaller granules, which float in the fluid contents of the cell. The form of these granules is extremely various; but they are distinguished by the colour they take when brought into contact with iodine. Simple and unimportant as the fact appeared that iodine produces this change, it has had a most important influence on the subsequent progress of

* Müller's Archiv, 1838, p. 137.

vegetable physiology, by enabling the microscopic observer to ascertain the real character of particles of extreme minuteness, whose form alone would have left him in doubt.

Under whichever of these conditions fecula exists in the plant, it is converted into gum before it can be concerned in the production of new tissues. This conversion appears to be a purely chemical change; and it may be brought about in the laboratory under conditions similar to those afforded by the vegetable organism. In the state which immediately precedes the commencement of organization, gum appears as a consistent fluid, slightly wanting in transparency; and it is coagulated granularly by the tincture of iodine, with a pale yellow permanent colour. In the further process of organization, a quantity of extremely minute granules appear in the gummy fluid; most of which, on account of their minuteness, are seen merely as black points. The fluid then seems to take from iodine a somewhat darker yellow; but the granules, when their size enables their colour to be distinguished, seem to become by this process of a dark brownish yellow.

It is probable that, in all plants, the chemical state of the materials which form cells is nearly the same; but there is not the same uniformity in the mode in which cells are generated. The researches of Schleiden refer almost entirely to the phanerogamia; and, as we shall hereafter see, they are not to be regarded as even generally applicable to the lower tribes of plants, without much further enquiry. We are not prepared to admit that the formation of new cells in flowering plants always takes place in this manner; since the observations of Schleiden have been as yet chiefly confined to parts in which we can easily imagine some peculiarities to exist. He specifies the large cell or embryonal sac of the unimpregnated ovule (in which the albumen is afterwards formed), and the end of the pollen-tube (from which the cells of the embryo itself are developed), as the points in which the process of organization may be observed most easily and clearly.

At both these places, the fluid is at first homogeneous and transparent; but the minute granules before mentioned soon originate in it; and it then becomes opalescent, or, through the presence of a larger mass of granules, even opaque. Single, larger, more sharply defined granules are now evident in this mass (fig. 1, plate ii.); and very soon afterwards these present a regular form, and increase considerably in size, apparently from the coagulation of the minuter granules around the larger ones. From these bodies the cells take their origin in the mode to be presently described; and hence Schleiden proposes for them the name of *cytoblast* (κυτος, cell, βλαστος, germ). These bodies had been previously observed in the newly-formed cells of some plants, and in the adult tissue of a few others, by Robert Brown and other observers; but none had enquired into their relation with the original construction of the cell. Their form varies from oval to circular; sometimes they are flattened, or lenticular; sometimes almost spherical. Their colour is in general yellowish, yet sometimes passing almost into a silver white, and occasionally almost transparent. The internal structure of the cytoblast is mostly granular; without, however, the granules of which it consists being clearly distinct from one another. Its consistence is very various; from such a softness that it almost dissolves into water, to that degree

of firmness that it bears even the pressure of the compressorium without losing its form. The nearer it is to its origin, the softer it is; and it has less consistence where it ultimately disappears, as it usually does, than where it remains throughout the life of the plant.

Even the cytoblast, however, does not appear to be the real elementary organ. On the larger and best developed forms of these there is observed a small sharply-defined body, which seems to have the form of a thick ring, or a thick-walled hollow globule (fig. 2). In less developed cytoblasts, only the outer sharply-defined circle of this ring can be observed; and in its centre a dark point (fig. 3). In still smaller cytoblasts it appears merely as a sharply circumscribed spot; or, lastly, there is observed only a remarkably small dark point. In the very smallest and most transitory cytoblasts (for instance in the leaves of dicotyledons), Schleiden has not hitherto been able to discover it. In a few rare cases, two or even three of these nucleoli have been seen in one cytoblast (fig. 3). From his observations on all plants which admitted of a complete watching of the entire process of formation, Schleiden concludes that these small bodies are formed earlier than the cytoblast; and he suggests that they may have some relation with the nucleoli of the starch-granules. The size of this corpuscle varies considerably, from the extent of half the diameter of the cytoblast to the most minute point whose size did not allow of measurement. Sometimes it appears darker and sometimes brighter than the remaining mass of the cytoblast. In general, it has more consistency than the latter, and still continues sharply defined when this has been changed by pressure into an amorphous mucus.

When the cytoblasts, increasing separately in the gummy fluid, have attained their full size, the formation of the cells commences. A delicate transparent vesicle is seen on the surface of each, in the form of a flat segment of a sphere, whose plane side is composed of the cytoblast, and the convex side of the membrane of the young cell, which is situated on the cytoblast somewhat like a watch-glass on a watch. The space between the two is perfectly clear, and appears filled with aqueous fluid Externally the membrane is in contact with the mucous granules of the surrounding fluid, which are pressed back by its expansion. The appearance at this period is represented in fig. 4; and, on a smaller scale, in fig. 5, *a*. If the vesicles are isolated, the mucous granules may be almost entirely removed by shaking the stage; but the vesicles themselves also soon disappear, dissolving entirely in distilled water, and leaving only the cytoblasts behind. As the process of formation advances, the vesicle gradually extends, and becomes more consistent (fig. 5, *b*); it increases beyond the margin of the cytoblast, and quickly becomes so large, that at last the latter merely appears like a small body inclosed in one of its side walls. At the same time the young cell frequently exhibits irregular indentations (fig. 5, *c*); a proof that the expansion by no means proceeds uniformly from one point. After further growth of the cells, their form becomes more regular; and when their sides begin to press against one another, they are flattened. If the pressure be equal in all directions, the form assumed by the cell will be that of a rhomboidal dodecahedron; and this is often observed in the pith and other parts in which the cellular tissue is not too much compressed. The cytoblast is still inclosed in the wall of the cell; but at a

later period it is usually absorbed, especially when the cell is destined to undergo still further development. In the *orchideæ* and *cacteæ*, a considerable part of the cellular tissue exhibits the cytoblasts during the whole of life. It was in the former that this body was first observed by Brown, who denominated the opaque spot which he there perceived the *areola* or nucleus. It remains persistent in many hairs, especially those which are articulated, and exhibit circulation of fluid in their cells. It is a curious fact, which proves the relation between the presence of the cytoblast and the vital activity of the cell, that the currents which frequently cover the whole wall in a network of streams, always proceed from it and return to it. In fig. 6 is seen a portion of the hair of a potato, as represented by Schleiden, with the persistent cytoblasts and currents. And in fig. 7 is shown the course of the currents in one cell of the beaded hair of the tradescantia virginica, as observed some years since by Brown, and subsequently by Slack.* Sometimes, on the contrary, not only the cytoblast is reabsorbed, but the cell itself. This it is not difficult to conceive, when it is known that, up to the period when the sides of the adjacent cells become flattened against each other, the want of consistency in their walls is such that the membrane may be easily caused to disappear, or, as it were, to dissolve away, by agitation. Reabsorption never takes place at a more advanced period of development, when the original membrane has not only become more perfectly organized, but has been strengthened by secondary deposits within its cavity, in the manner to be hereafter described. It would seem that the membrane of the fully-formed cell consists of two laminæ, between which the cytoblast is inclosed (fig. 8). The inner one is the most delicate, and frequently only gelatinous; and this is reabsorbed at the same time with the cytoblast.

In connexion with these observations, we shall now give a brief analysis of the researches of Schleiden on the generative process in the phanerogamia. It will be seen that, according to his view, the early condition of the vegetable embryo precisely resembles the permanent state of the simplest cellular plants; and that its production takes place in accordance with the principles which have just been expounded. In a former article on the state of vegetable physiology (Vol. IV., p. 29), we mentioned the well-ascertained fact that prolongations of the membrane of the pollen-grains which fall upon the stigma find their way down the style, and apply themselves to the foramen of the ovule; and we stated it is probable that, from the contents of the pollen-grain thus conveyed into the ovule, the embryo, which subsequently appears, takes its origin. The researches of Schleiden have confirmed this view, and have given it a much greater degree of precision. According to his statements, the cavity of the ovule before impregnation is occupied by a large vesicle containing a turbid fluid ready to become organized in the mode already mentioned. The pollen-tube which enters the cavity, through the foramen left by the imperfect closure of the coats of the seed, pushes the membrane before it (fig. 9); and it thus becomes gradually imbedded in the embryo-sac, as, according to the Hunterian doctrine, the ovum acquires a double coating from the membrana deci-

* Transactions of the Society of Arts, vol. xlix.

dua. By absorption through the membranes, the nutritious materials contained in the embryo-sac appear to find their way into the pollen-tube; and they thus contribute to the development of the cell-germs contained in the latter. The starch originally contained in the pollen-grain usually changes its character in its descent along the tube; so that the fluid at the embryonal extremity of the latter is transparent and colourless. Here the development of cells takes place in the mode already described; and it is often observed to occur not only in that part of the pollen-tube contained within the ovule, but in the free portion also. The appearance presented by the pollen-tube of *orchis morio* is shown in fig. 10, where the cells are seen to be formed from cytoblasts within the general tubular envelope. In the part of the tube which is imbedded in the ovule, the development of the cells does not go on in a single row merely; but the tube expands in such a manner as to contain a cluster, as is shown in fig. 11. Schleiden states that, even after the development of cells within the pollen-tube had commenced, he was able to withdraw the tube from its bed in the embryo-sac, especially in orchideous plants. After a time, however, the newly-forming cells increase so much, that the enveloping membrane of the pollen-tube is no longer recognizable. Within the cells first produced many new ones are generated, and the membranes of the parent cells disappear in their turn. The embryo is nourished by absorption through the embryonal sac. In the higher dicotyledonous plants, its increase continues until it occupies the whole seed, having entirely exhausted the contents of the embryo-sac. In monocotyledons and some dicotyledons, however, the development of the embryo does not advance so far before the maturation of the seed; and the contents of the embryo-sac, not being absorbed into the embryo, develope themselves into cells externally to it, forming what is known to botanists as the *albumen* of the seed. It is in this situation that the process of the formation of cells has been chiefly observed by Schleiden.

We shall now proceed to enquire into the principal changes which the cells, from which all vegetable structures take their origin, subsequently undergo.

It has been justly remarked by Schleiden, that every cell leads a double life; an entirely independent one, belonging to its own development alone; and an incidental one, so far as it has become the constituent part of a plant. In the lowest tribes of algæ and fungi, we find these two conditions, as it were, coincident; single cells possessing in themselves the power of maintaining the life of the species, as well as their own independent vitality; or, in other words, the plant consisting of but a single cell, or being at least capable of existing as such. This is the case, for example, in the red snow (*protococcus nivalis*), in which every one of the crimson vesicles is an independent plant, having no necessary connexion with others, and capable of itself of performing all the changes required for the maintenance of its existence, and the continuance of its race. This we take to be what is expressed by the term *individuality*. The separate cells of the protococcus, however, are brought into some degree of connexion, by the mass of gelatinous matter on which they rest. The presence of this may be perceived, as a kind of slime, before any distinct traces of organization appear; and it may be

regarded as standing in the same relation with the subsequently formed cells, as the organizable gummy fluid, of which we have already spoken, in the higher plants. Now this slimy substance (which has been called *phycomater*) continues to exist, in the simpler algæ, during the whole life of the cells; and, when new ones are formed by the rupture of the parent vesicle, it appears to supply them in part with their means of nutriment. On the degree in which the isolated cells are imbedded in it and connected by it, generic distinctions have been founded. Now, if we ascend to the higher forms of algæ, we shall find that the individual plants gradually become more complex in structure, and that they are composed of an association of cells having different offices in the vital economy. Still, we do not lose sight of this *organic mucus*, which, in many species, is very abundant, and which is almost always to be found between the cells. As yet, their independent life predominates over the compound life of the general structure. They live more for themselves than for each other; and there is no occasion for great intimacy in their union. In the higher plants, on the other hand, we find that the process of organization has advanced, so as to exhaust nearly all the organic mucus, and to bring the individual cells into closer contact with each other. Still, however, some traces of it may often be found; and it thus constitutes an *intercellular substance*, which may sometimes be perceived filling up interstices between the walls of the cells, and serving to unite them together. The most remarkable instance of its presence, however, is the delicate pellicle which covers the true cuticle probably of all plants, and which is even found on submerged surfaces on which no true cuticle exists. This pellicle is a very thin homogeneous membrane, in which no traces of structure can be detected. We are inclined to regard the *intercellular substance* of plants, therefore, as the remainder (so to speak) of the organic mucus out of which their structures have been organized; and to believe that this substance, lying externally to the cells, may undergo a slight degree of organization, by contact with living tissues, as we see in the case of extravasated blood, &c. We have dwelt the longer on this point, because we shall hereafter see the application of the view we have suggested to the explanation of the *intercellular substance* found in the cartilaginous and other tissues of animals. That this presents a degree of organization, can scarcely be questioned; and Schwann is perplexed to account for any structure being developed in it, without its having first passed through the form of cells.

We shall now give a short account of a few of the more important changes which vegetable cells undergo, where their ultimate function requires their form and character to be modified. One of the simplest of these modifications, which may occur in almost all the forms into which cells are changed, consists in the deposition within their walls, of certain products secreted from the vegetable juices. Sometimes these are deposited in concentric layers, exhibiting a series of rings when the cell is cut across (fig. 13). Very commonly, however, there is not this kind of regularity; but the deposits project more into the cavity. In these cases, passages are often left (as seen in fig. 14), by which the cavity of the cell is extended at some points almost to its membranous wall; and thus the different cells retain some power of communication with each

other. Sometimes there is great regularity in this respect; the deposits in two contiguous cells being absent at corresponding points, so that the membranous septum is the only obstacle to the communication of their cavities (fig. 15). The object of these deposits appears to be in all instances the same—to import mechanical firmness to the tissues. They are not found to any great extent in cells which are actively contributing to the general vital processes; whilst in those which are inert, the cavity is frequently almost obliterated. Thus in the gritty tissue of the pear (fig. 16) and the stone of the plum (fig. 14), we find this *sclerogenous* deposit very abundant; and the same is the case in the woody tubes of the heart-wood of the stem; whilst those of the alburnum, through which the sap ascends, are comparatively free.

In other instances, however, the matter deposited within the cells appears to undergo organization; and it then presents the form of a spiral fibre, of which the coils are sometimes in close apposition, and sometimes widely separated. It is the opinion of Meyen that the membrane of the cell is itself composed of fibres spirally disposed; but we rather incline to agree with Schleiden that, in the cases which appear to support this hypothesis, the membrane of the cell has been originally homogeneous, and that the spiral fibres subsequently formed have become incorporated with it. The latter physiologist has seen the spiral fibre in various stages of development; and he states that its manifestation always commences at a period subsequent to that of the full development of the cell membrane. Not unfrequently the spiral fibre is afterwards broken into isolated portions by the extension of the cell; and these portions either grow irregularly together, or become the nuclei for deposits which unite them; so that an inner coat is thus formed, which is incomplete at some points, and these appear externally as dots very similar to those produced by the passages formerly described. The *dotted cells* and *dotted ducts*, therefore, may be regarded as cells or tubes within the cavities of which a secondary deposit has taken place; this deposit being sometimes entirely unorganized; whilst in other instances it seems to present some traces of the spiral structure (fig. 17).

We have not yet spoken of the alterations in *form* which the originally spheroidal or dodecahedral cells undergo at subsequent periods. In the early condition of the superior plants, as in the permanent state of the lower cryptogamia, no very great alterations of this kind ensue. At a more advanced stage of the growth of the former, however, we find several remarkable modifications. Among these are the continuous tubes or *ducts*, which seem to be the special channels for the ascent of the sap through the stem. That these are formed from large cells placed end to end, the partitions between which have been obliterated, has long been the opinion of Dr. Lindley, in opposition to the ideas of their formation entertained by the German and French anatomists. It is confirmed in a very remarkable manner by a specimen of fossil wood in our possession; the section of which, taken in a line with the ducts, exhibits most distinctly the remains of the partitions between the cells of which these tubes were originally formed. As the whole structure of these fossil woods—cavities as well as solid parts—has been infiltrated with silex, any minutiæ of this kind may be better seen in them than in the recent structures, which are liable to be torn and displaced when laid open even

with the sharpest knife. This mode of formation of continuous tubes has a very interesting relation with the opinions of Dr. Hodgkin as to the origin of the absorbent vessels in animals, and the development of their valvular folds.* It is seldom that *ducts* are found composed of membrane simply; their cavities are generally kept pervious either by a spiral fibre more or less continuous, or by a wall of secondary deposit such as we have described. In fig. 17 is seen a duct, in one part of which the spiral is nearly complete, whilst in another portion it is much broken; the portions of the fibre, irregularly anastomosing together, constitute the *reticulated* duct; and when the spaces left between these have been still further closed by secondary deposits, the *dotted* duct is the result. In fig. 18 a dotted duct is seen, which seems to result from the union of cells like those represented in section at fig. 15; the inner wall, with its passages, being formed by inorganic deposit alone.

The form of those cells which remain isolated often undergoes very remarkable changes. Thus the *fibres* of woody structure, and the *spiral vessels*, are neither more nor less than very elongated cells. A class of vessels has lately been described, which bears so close a relation with the capillaries of animals that it will be worth while to enquire into their origin. These are the *vessels of the latex* or *vital vessels* of Schultz; the *cinenchyma* of Dr. Lindley. The ducts of which we have previously spoken, are all straight unanastomosing tubes. These vessels, on the contrary, present a reticulated character; and a plexus of them resembles very closely the distribution of capillaries in a frog's foot. We formerly alluded to the circulation of nutritious or elaborated sap which takes place through them;† but at that time the prevalent opinion among botanists was, that this movement takes place, not in distinct vessels, but in *intercellular spaces*. We find, however, that Dr. Lindley now avows himself a convert to the doctrine of Schultz, that there is a regular series of vessels for the performance of this function.‡ We had ourselves been previously led to the same opinion; and if the doctrines given by Schultz, for separating these vessels from the rest of the tissue, be followed, we cannot perceive any ground on which their existence can be denied. Their walls are generally very thin, and their diameter irregular. The appearance of a plexus of them in the stipules of ficus elastica is shown in fig. 19 after Schultz.

The question which we shall subsequently notice, as to the origin of the capillary vessels of animals, may not improbably be elucidated by enquiries into the mode in which these vital vessels are formed. We have ourselves no doubt that they result from the partial obliteration of the partitions between cells of irregular form, which thus open into one another in several points, and not, as when regular ducts are produced, by their extremities alone. In fig. 20 is seen a portion of cellular tissue from the stipule of ficus elastica, which seems just about to undergo this change.

We have dwelt longer on this subject than its intrinsic importance may seem to warrant, because we feel sure that the more closely the early de-

* We much regret that the valuable report on this subject, presented to the British Association at its meeting at Bristol, has not yet been published.

† Vol. IV., p. 27. ‡ Introduction to Botany. Third Edition, p. 36.

velopment of the animal and vegetable tissues is *concurrently* studied, the more will they be found to illustrate one another. Most of the details which we have now given will be found to have an important bearing on the doctrines which have been propounded by Schwann regarding the formation of the animal organism; to the exposition of which we shall now proceed. We would first remark, however, that the *increase* in the number of cells does not always seem to follow the law stated by Schleiden. In the chara, as well as in the simpler plants we have described, it is very evident that the new cells *bud out* from the sides of the parent ones, which themselves undergo no change. In the confervæ it has long been remarked that the elongation of the filaments is produced by the subdivision of the terminal cell into two, by a septum which forms across it; and the last of these in its turn undergoes a similar division. This apparent division of the cavity of cells, by the extension of septa inwards, was supposed by Schleiden to be owing to the development of two or more young cells within a parent cell and the meeting of their walls; but M. Meyen very recently,* by numerous observations, seems to have proved that such a partitioning of cells by the growth of septa really takes place even in the higher plants.

Growth of Animal Cells. The first animal structures, whose mode of growth Schwann investigated when he became acquainted with the discoveries of Schleiden, were the cells of the chorda dorsalis and those of cartilage. The chorda dorsalis (the gelatiniform cord which in the embryo of all mammalia occupies the axis of the spinal column in the place of the future bodies of the vertebræ, and which exists in the larvæ of amphibia and in some cartilaginous fishes throughout life,) is particularly interesting on account of the exact resemblance which its internal conformation presents to one of the most common varieties of vegetable cellular parenchyma. M. Schwann thus sums up his observations on its structure and mode of growth.

"The *chorda dorsalis* is formed of polyhedral cells. In the substance or at the inner surface of the walls of these cells lies a body which, in form, as well as position, is wholly similar to the nucleus or cytoblast of vegetable cells; since it is an oval flattened disc containing one, two, or three smaller bodies, its nucleoli. The cells lie in close contact with each other, but sometimes, at points where three or more meet, a small intercellular space or a small quantity of intercellular substance is observed. Within some of the cells young cells are formed; these are at first round and float freely in the parent cells; they do not present the characteristic nucleus, though sometimes a very minute spherical body is seen attached to the inner surface of their wall. In some instances (for example in the remains of the chorda dorsalis, which form the intervertebral bodies of osseous fishes), the cells undergo a further development. The simple membranous wall of the cell disappears, and the substance which separates the cavities of the different cells comes to be formed in chief part of longitudinal fibres." (p. 16.)

The form of the cells and of their nuclei in the chorda dorsalis of cyprinus erythrophthalmus is represented by fig. 21, plate iii. With reference to the apparent absence of the characteristic nuclei in the young cells of the chorda dorsalis (fig. 21, *b*), M. Schwann remarks (p. 17), that "it must remain uncertain whether the nucleus is really deficient

* Müller's Archiv, 1839, p. 255.

here, as at present it appears to be in many acotyledonous plants; or whether the small globule (*d*) seen on the wall of some of the young cells is a nucleus, which afterwards grows with the cell as the nucleus of some other animal cells is observed to do; or, lastly, whether the nucleus is invisible in these young cells only on account of its transparency. The last explanation is rendered probable by the circumstance that in some fully developed cells the nucleus, though certainly present, is so exceedingly translucent as to be distinguished with great difficulty." The change to a fibrous structure which the chorda dorsalis undergoes at a later period of its growth is not explained by M. Schwann.

That the corpuscules of *cartilage* discovered by Purkinje are hollow, and that they contain nuclei and sometimes globules of fat, had been noticed by several observers, namely, by Müller, Valentin, Meckauer, and Arnold. Müller and Meckauer had also shown that they might be separated as distinct bodies from the substance of the cartilage, and were not mere cavities hollowed within it, and Gurlt and Berres had even called them vesicles. Being acquainted with these facts, Schwann at once directed his attention more particularly to the phenomena of growth of the cartilages and of their corpuscules or cells.

The cartilaginous rays of the branchiæ of fishes (cyprinus erythrophthalmus), he found to be composed at their apex entirely of nucleated polyhedral cells with very thin walls (fig. 22). The nucleus was sometimes not distinct until after the action of water upon the cartilage. Towards the middle of the branchial ray the walls of the cells became evidently thicker and their cavities smaller, and it was now more manifest that each cell had distinct parietes. Moreover, at points where three or four cells met, the intervening space which was left was seen to be occupied by a new intercellular substance (fig. 23). At the base of the ray the cavities of the cells had diminished still more in size, while their walls were of yet greater thickness. The intercellular substance, too, had increased in quantity, and with it the walls of the cells had partly coalesced, so that the cartilage appeared to consist of one homogeneous mass with small excavated hollows (fig. 24). Here, then, we have an example of the walls of cells gradually increasing in thickness, a process which is not infrequent in vegetables.

This, however, is the least common form of cartilage. More usually, and in all the true cartilages of the higher vertebrata, the intercellular substance constitutes by far the greater part of the mass; while the walls of the cells retain their original tenuity, and apparently do not coalesce with the surrounding substance. The cells or corpuscules of these cartilages frequently lie in groups of three or four. M. Schwann was at first disposed to attribute this disposition of the cells to the development of several young cells within one parent cell; for in the cartilage of the branchial arches of tadpoles, it appears to him that the substance immediately around some of the groups of cells presented a concentric line comparable to the outer contour of the thickened wall of a parent cell. Further observations, however, convinced him that this view was not correct, and that the cells are arranged in groups merely from the circumstance of three or four being developed in one cavity of the intercellular substance. The figure which M. Schwann gives of a portion of cartilage taken from the margin of a branchial arch of a tadpole, illus-

trates extremely well the growth of cells around preexisting nuclei (fig. 25). The greater part of the mass is seen to be formed of cells with distinct parietes and with nuclei, which contain one or two small bodies (nucleoli). In the interstices of these fully-developed cells and at the free edge of the cartilage (*a*), are other cells of smaller size, in which, however, the nuclei have nearly their full dimensions (fig. 8, *c*); and between these are nuclei as yet wholly destitute of surrounding cell (*b*); so that here, as in plants, the cell seems to be gradually formed around the nucleus. Schwann also perceived here and there in the cartilage small bodies surrounded by granular matter (*e*). These were apparently nuclei in the process of development by the aggregation of granules around nucleoli, which, according to Schleiden, is the mode of formation of the nuclei in plants. Whilst the new cells are being developed by the process just described, the intercellular substance gradually increases in quantity in the interstices of those already formed, and at length constitutes the chief mass of the tissue. The solid portion of cartilage appears, therefore, to be a distinct substance, and not to be formed, even in part, by the thickening of the walls of old cells during the development of a succession of younger cartilaginous cells within them, as Schwann at first supposed. It appears certain, however, that within the proper cells of the cartilage young cells, most probably of a different physiological character, are sometimes developed from nuclei in the same manner as the cells of the cartilage were formed in cavities of the intercellular substance. This development of young cells within the proper cells of the cartilage, which is also strongly insisted on by Valentin,* has been very rarely observed by ourselves in frequent examinations of the cartilages of the human subject. But of the accuracy of M. Schwann's statement relative to the formation of the cells around their nuclei, and even of the nuclei around nucleoli, in cavities of the intercellular substance, we have had ample opportunities of satisfying ourselves.

Such are the principal facts which revealed themselves to Schwann in his investigation of the structure of the chorda dorsalis and cartilages, and which led him to infer the existence of a law regulating the development of all organic tissues, animal and vegetable. We shall now follow him in his observations on the structure and growth of animal tissues generally; and first in his account of the ovum and germinal membrane, from which all parts of the future animal are developed.

The Ovum. It will be recollected that the ovum of mammiferous animals, inclosed within the Graafian vesicle of the ovary, consists of three principal parts: 1, the vitelline membrane or yolk-sac; 2, the vitelline fluid; and 3, the germinal vesicle, which lies close to the inner surface of the vitelline membrane, and itself contains a smaller body, the germinal spot of Wagner. The vitelline membrane,† or external proper investment of the ovum, is, as Krause has shown, structureless, and in this character agrees with the external wall of the formative cells of plants and cartilage. The vitelline fluid also may, with very great probability of correctness, be compared to the contents of a cell. But in what light

* Repertor. für Anat. u. Physiol., i., p. 286; and Wagner's Physiologie, p. 136.

† We here leave out of consideration the question respecting the nature of the zona pellucida, since it was fully discussed in the last Number of this Journal at p. 13.

are we to regard the germinal vesicle? Is it a young cell developed within the vitelline sac, as its parent cell? Or is it the nucleus of this sac or cell? M. Schwann was at first unable to decide this question; but M. R. Wagner's observations on the development of the ova of an insect, the agrion virgo,* seemed to supply the requisite data. M. Wagner observed that the germinal vesicle was first formed, and that the vitelline sac, in its earliest condition, invested the germinal vesicle very closely, and only gradually expanded as it became filled with its contents. This fact appears to Schwann sufficient proof that the germinal vesicle is the nucleus or "cytoblast" of the ovum; with which view the position of the germinal vesicle at the inner surface of the vitelline membrane, its greater comparative size in small imperfectly developed ova, and its disappearance at a subsequent period, likewise accord. Before we were acquainted with Wagner's observation, we had ourselves noticed the same phenomena in the process of development of the ova of daphnia pulex; but a more important confirmation of Wagner's statement and of M. Schwann's deductions from it, is afforded by the discovery of Dr. Barry, that, both in birds and mammalia, the formation of the germinal vesicle precedes that of the vitelline sac. Since it appears from Wagner's drawings that the germinal spot exists previously to the vesicle which afterwards includes it, Schwann regards this spot as the nucleolus of the ovum. (*Appendix*, p. 259.)

After thus viewing the ovum as a whole, our author proceeds to examine its contents, taking for this purpose, however, the egg of the hen. We cannot follow him in his detailed description of the different kinds of globules which he found within the yolk-sac, and of the process of development of the different parts of the ovum, but must restrict ourselves to noticing the following most important facts. The yellow globules of the yolk, those of the mucous layer of the germinal membrane, and those again of the more colourless and transparent centre of the yolk (dotter-höhle), want the characteristic nucleus or cytoblast, though they contain granules. The cells of the serous layer of the germinal membrane and those investing the inner surface of the structureless vitelline membrane, on the contrary, are shown by M. Schwann to have each a nucleus of the usual form with one or two nuclei contained within it. So that in these cells formed within the ovum we have an unequivocal example of the development of nucleated cells within a parent cell. (pp. 55-66.)

The first step in the formation of the embryo consists, as is well known, in a portion of the germinal membrane becoming separated by a constriction from the rest of the vitelline sac. Both the mucous and the serous lamina contribute to the formation of the young embryo; and, in accordance with this, it is composed, according to Schwann, "of cells destitute of nucleus, and of others in which the characteristic nucleus with its nucleolus is visible. But besides these it also contains numerous nuclei free from surrounding cell." The development of the cells composing the vascular layer of the germinal membrane will come under

* Prodromus Historiæ Generationis, Lips., 1836; and Beiträge zur Geschichte der Zeugung und Entwickelung. Erster Beitrag aus der Mathematisch-physikalischen Klasse der K. Baiersch. Acad. der Wissensch. in München., 1838.

consideration at a future page, as affording an instance of the formation of new vessels.

Having thus shown that the ovum may be regarded as a nucleated cell, and that the germinal membrane and the embryo itself (the groundwork of the future animal), are composed of cells, Schwann proceeds to demonstrate that the animal tissues not only are in this general sense produced from cells, but are individually either composed of cells or developed from them by various processes of transformation. "This being true, it is evident," he remarks, "that the most scientific classification of tissues would be one based upon the different degrees of development which the cells undergo in their formation." The following is the arrangement which he proposes:

"1st Class.—Cells, of which the walls are distinct, and which remain isolated from each other. This class consists principally of the cells which float in fluids, namely, the lymph-globules, the blood-corpuscules, the globules of pus, mucus, &c.

"2d Class.—Cells, of which the walls are distinct, but which are united into coherent structures. Such are the horny tissues and the crystalline lens.

"3d Class.—Cells whose walls have coalesced. The cartilages, bone, and the ivory or proper substance of the teeth, belong to this class.

"4th Class.—Cells which have split into fibres, forming cellular tissue, tendon, and elastic tissue.

"5th Class—Cells, of which both the walls and cavities have coalesced. Muscles, nerves, and capillary vessels." (p. 74.)

1. M. Schwann has himself added no new fact to our knowledge of the structures included in his *first class*. That the *lymph-globules* are nucleated cells was proved by the observation of Vogel,* who found that, when the globule had been rendered transparent by the action of acetic acid, a darker and more opaque nucleus was visible in its interior. Of the accuracy of this observation we have convinced ourselves by the examination of the lymph-globules of the frog, in which, indeed, the nucleus can in some instances be seen before the acetic acid is added. The existence in the lymph of the frog of nuclei destitute of surrounding cell, and of small lymph-globules with nuclei, equal in size to those of the largest globules, seems to prove also that the nucleus is first formed, and that in its development, as well as in its structure, the lymph-globule accords with the true organic cells. The *blood-corpuscules* have been long regarded as cells or vesicles, and in the large elliptical corpuscles of the cold-blooded vertebrata, the existence of a nucleus has been recognized since the time of Hewson. Their mode of development, however, has not yet been satisfactorily elucidated.

The *globules of pus* and the very similar bodies poured out by the follicles of mucous membranes, have been recently shown by Vogel, Güterbock, and Valentin to contain a nucleus of different chemical properties from the rest of their substance. Vogel has observed, moreover, that in the pus-globule the nucleus is the first formed part; and that around this a semitransparent vesicle, which afterwards acquires the size and character of the perfect pus globule, is gradually developed.

We observe that Valentin† regards the blood-corpuscules, lymph-glo-

* Ueber Eiter, Eiterung und die verwandte Vorgänge. Erlangen, 1838.

† Wagner's Physiologie, p. 133.

bules and pus-globules not as nucleated cells, but as mere nuclei with nucleoli; but his grounds for this opinion appear to us by no means convincing. In the protococcus nivalis we have already seen an instance of the existence of organic cells in an isolated state in the vegetable kingdom.

2. M. Schwann's *second class of tissues* presents us with some of the most remarkable instances of the similarity of animal to vegetable structures.

Of the horny tissues, M. Schwann examines particularly the epithelium and pigment membranes, nails, hoofs, and feathers. The fact that the *epidermis and epithelium* are composed of particles of definite form, each of which contains a nucleus, is known to all anatomists; it having been recently established by the investigations of Henle and Purkinje. Henle* observed, moreover, that the particles composing the deepest layer of the epidermis, or the rete mucosum, are globular and so small in size as to be nearly filled by their nucleus; and that, whilst the nuclei retain nearly their original size, the cells constituting the more superficial layer are larger as well as flattened. These facts are confirmed by Schwann, who points out their complete accordance with his theory. He remarks, also, that when the cells of epithelium have the globular or polyhedral form, as on the branchial rays of fishes, the nucleus can be seen to be attached to the wall of the cell. In the tadpole, Schwann has seen young nucleated cells within cells of the epithelium. (pp. 82-7.)

The *pigment membranes*, for example, that of the choroid coat of the eye, are known to be composed of polygonal cells, the centre of each of which is occupied by a colourless nucleus, and the surrounding space by granules of pigment. Schwann has not any observations upon the mode of growth of these cells; but we find Valentin stating† that the nucleus is first formed, that a cell is developed around it, enlarges and becomes polyhedral, and that subsequently the deposition of pigment molecules within the cell commences around the nucleus.

The transformation from the globular to the radiate form, which pigment cells sometimes undergo, displays to us, as we shall presently see, the type upon which capillary vessels appear to be developed.

The investigation of the structure of the *nails* in the fœtus and newborn infant afforded to Schwann a fresh confirmation of his theory. He found that the thin horizontal laminæ, into which the nail may be split or torn, were not structureless, but were composed of scales like those of epithelium, and in some of the scales he could distinguish a nucleus. The root of the nail, particularly at its under surface, presented no laminated structure, but consisted wholly of small polyhedral cells, very many of which had a distinct nucleus. Hence, the growth of the nail would seem to consist in the formation of cells, which subsequently become flattened and extended. But since, if this process went on only at its root, the nail, while it would be pushed forwards, would become thinner from behind forwards, in proportion to the flattening of the cells, it appears necessary, as M. Schwann remarks, that the formation of new cells

* Symbolæ ad Anatomiam villorum intestinalium, &c., p. 5. Berol., 1837.

† Wagner's Physiologie, p. 135.

should take place likewise at the under surface of the nail, at least to a certain extent from its root. (pp. 90-2.)

The horny tissues of the cloven *hoofs* of ruminantia was found by M. Schwann to be entirely composed of polyhedral cells; and in some of those most recently formed he was able to recognize a nucleus attached to the inner surface of their wall.

The medullary substance of *feathers* (described by Hooke,* and both described and figured by Leeuwenhoek† as consisting of globules or vesicles), presents the most exact resemblance to the cellular parenchyma of vegetables; and its mode of growth also, according to Schwann, agrees very closely with the process described by Schleiden as obtaining in plants. The development of animal cells is, indeed, nowhere more easily observed than in the young feather, at the period when the medullary substance of the shaft is in the process of formation. The surface of the matrix is then covered with a finely granular matter containing numerous small nuclei, some of which have a nucleolus (fig. 26). At a little distance from the surface of the matrix each nucleus is surrounded by a cell which is at first not much larger than the nucleus, and has a granular aspect (fig. 27, *c*, *b*). The further advanced the medullary substance is in its formation, the larger are the cells; and the nucleus also enlarges to a certain extent, but remains attached to one wall of the cell (fig. 27, *a*). While the cells increase in size, their walls become thinner and acquire a darker contour. At length they adhere together, and assume a polyhedral form, at the same time that the nuclei disappear (fig. 28). (*Schwann*, pp. 94-7.)

We shall reserve M. Schwann's account of the development of the cortical part of feathers, which is a fibrous structure, until we come to speak of his fourth class of tissues.

In the foregoing review of the growth of the horny structures, we have met with only two instances of the cells undergoing a remarkable transformation, and these we have reserved for future consideration, while generally the cells have preserved a resemblance to the simplest forms of vegetable tissue. But in the *crystalline lens*, which M. Schwann places in the same class with those structures, we can at first perceive nothing which tends to confirm the correctness of that classification, or which even indicates in any way the origin of the tissue from cells. Yet M. Schwann's observations lead him to conclude that the crystalline lens, so far from affording a contradiction, presents appearances remarkably favorable to the general theory.

The lens, as is well known, is composed of concentric laminæ, each of which is constituted of fibres united to each other by tooth-like processes of their margins. It was some time since, however, observed by Werneck,‡ and confirmed by Ahrens,§ that a layer of transparent cells intervenes between the lens and its capsule; and from such cells the fibres of the lens, according to Schwann, are formed. In a chick, after eight days' incubation of the egg, he found the lens to consist entirely of round, extremely pale, transparent, and smooth cells, some of which were furnished with a nucleus, mixed with nuclei destitute of surrounding cells

* Micrographia, p. 165. † Arcana Naturæ. Epistola 74.

‡ Ammon's Zeitschrift. Bd. v. p. 414. § Müller's Archiv, 1838, p. 259.

(figs. 29 and 30). In the eye of a fetal pig three inches and a half long, the lens, as yet only partly formed, was imbedded in numerous cells similar to those above described, while its circumference was formed by a thick and broad zone of imperfectly developed fibres evidently in the stage of transformation from cells. These imperfect fibres, like the perfect ones, formed arches from the anterior to the posterior surface of the lens, but neither in front nor behind did they reach its axis, while their extremities were either abruptly truncated, or slightly dilated, or expanded into a vesicle or cell, which resembled exactly the free cells on their exterior (fig. 31). These appearances seemed to show that the fibres of the lens are formed by the elongation of pre-existing cells—a view confirmed by the fact that nuclei were often seen by M. Schwann within the fibres of the lens of the fetal pig. The cells which have thus assumed the form of fibres seemed to acquire gradually the dentated margin; for they had that character in a more marked degree in proportion as they were examined nearer to the centre of the lens. The dentations of the fibres of the lens are compared by M. Schwann to the sinuosities of a not uncommon form of vegetable cells. (pp. 99-102.)

Valentin had as early as 1833 suggested that the cells discovered by Werneck were converted into the fibres of the lens. The process, however, by which he now describes that conversion to take place, is different from the one observed by Schwann. Several cells, he says, coalesce in a longitudinal series, and then split into fibres.* Schwann several times observed a linear arrangement of nuclei, but saw nothing which seemed to indicate that one of the fibres of the lens was formed by the coalescence of several cells.

3. Schwann's *third class* of tissues, to which he refers the cartilages, bones, and teeth, appears to us to be the least natural of all, or rather, perhaps, we should say that Schwann's investigation of those structures was too imperfect to justify his including them under one definition. He designates them as tissues in which the cells have coalesced either with each other or with an intercellular substance. But we have already seen that in the cartilages of the higher vertebrata the walls of the cells remain distinct. Schwann perceived this, and remarks that here the presence of a solid intercellular substance must be taken as the characteristic of the tissue. In the teeth, however, we shall find that the intercellular substance appears to be wanting.

We have previously described the development of the cells of *cartilage*, which accords so entirely with Schwann's theory: it, therefore, merely remains for us here to mention a circumstance in the history of this tissue, which to Schwann appeared anomalous, but of which we shall presently offer an explanation. It is the gradual conversion, as age advances, of the intercellular substance, which in the fœtus and young child is perfectly homogeneous, into a structure composed of distinct parallel fibres. This change is not universal, nor does it take place to the same extent in all parts of the same mass of cartilage; but yet it is frequently very marked. Moreover in one kind of cartilage, that composing the external ear and epiglottis, the intercellular substance is at all periods composed of minute fibres.

* Wagner's Physiologie, p. 138.

The osseous corpuscules and the intervening substance of *bone* seem to be identical with the corresponding parts of cartilage, which in the process of ossification become impregnated with calcareous salts, while the animal matter undergoes, as Müller has shown, a chemical change. We have, therefore, only to notice Schwann's mode of explaining the formation of the calcigerous canaliculi which radiate from the osseous corpuscules. There are two processes, he says (p. 35), by which they might be developed. They might arise from the walls of the cells undergoing thickening by deposition on their internal surface, except at certain points, where pore-like canals would be left, a process which is observed to take place in vegetables (*see* figs. 14, 15, and 16, plate i.); or they might be produced by the elongation and branching of the cells in the manner in which we shall see that certain ramified pigment cells, and perhaps also the capillary vessels, are formed. M. Schwann prefers the latter explanation, and probably with reason; but when he states that there is no instance in the animal organism of pores being left during the thickening of the walls of cells, as in the formation of the dotted cells and ducts, and other similar structures of plants, he is at variance with Valentin,* who describes such a process as taking place in the organization of the external skeleton of the astacus fluviatilis.

In the growth of the *teeth*, and even in that of their enamel, M. Schwann finds some confirmation of his theory. He has ascertained that the hexagonal fibres of the enamel membrane of Raschkow, described at p. 167 of the Eighth Volume of this Journal, are elongated cells (fig. 33). Each of them contains a distinct nucleus, while the membrane upon which they rest is also formed in great part by round nucleated cells, apparently the primary form of the above-mentioned fibre-like prismatic cells. Moreover, the mass of organic matter left, when the enamel of the young tooth of a child or pig was submitted to the action of dilute muriatic acid, was found by Schwann to consist of closely-aggregated prisms (fig. 32), exactly similar to the fibres of the enamel membrane. Accordingly he supposes that, by a process of transformation constantly going on at the surface of the enamel membrane, the globular cells of that membrane are changed into prismatic fibres or cells, which immediately separate from it, and, being at the same time filled or impregnated with calcareous salts, become part of the growing enamel. (pp. 118-21.)

The ivory of the tooth, or at least its intertubular substance would seem to be formed by a somewhat similar process. The globules spoken of by Purkinje and Raschkow as composing the formative pulp of the tooth, are, according to Schwann, round nucleated cells; while the elongated globules, described by those anatomists as being more regularly arranged at the surface of the pulp with their long axis directed outwards, are cylindrical cells, which, like the prismatic cells of the enamel membrane, contain nuclei with their nucleoli. When the pulp is drawn from the cavity of a young tooth, a layer of these cylindrical cells remains attached, at all events in some parts, to its inner surface. These facts at once led Schwann to imagine that there was a direct conversion of the cells of the pulp into the dental substance—in other words, that "the tooth was the ossified pulp." (p. 124.) But it appeared at first doubtful,

* Repertorium für Anat., Bd. i., p. 124; and Wagner's Physiologie, p. 135.

in what relation the ossifying cells stood to the different parts of the perfect ivory. Purkinje and Raschkow, however, had stated that the ivory of the growing tooth consisted of fibres directed from the pulp to the outer surface; and Schwann, examining these fibres (fig. 34), found that, while they corresponded in size to the elongated cells above mentioned, they were much larger than the now well-known dental tubuli. Hence he concludes that the cells of the pulp, becoming filled with animal matter and then ossified, form the fibres of Purkinje and Raschkow, and that these coalesce so as to constitute the intertubular substance of the tooth leaving, however, spaces in which the dental tubuli are at a subsequent period developed (pp. 122-8). This view of the mode of growth of the ivory accords very well with the observation of Mr. Owen* and Mr. Nasmyth,† that, even in the perfectly ossified tooth, cells can be distinguished in the intertubular substance. The ivory, therefore, would appear to contain no element corresponding to the intercellular substance of cartilage.

4. We now pass to the consideration of the tissues of the *fourth class* —those produced by the elongation of cells.

In these fibrous structures, the cellular tissue, tendon and elastic tissue, all trace of the original cellular form is lost; and yet it is here that the testimony of other observers has afforded perhaps the most abundant confirmation of M. Schwann's researches.

The essential elements of *cellular tissue* are now generally admitted to be wavy bundles of cylindrical fibres, between the interlacements of which are deposited in some parts the cells or vesicles of fat. When, however, cellular tissue is examined in the young embryo, it is found to be a gelatiniform substance, composed of a transparent base containing more or less numerous corpuscules of various kinds. The most constant of these corpuscules are the true primary cells of the fibres of cellular tissue. In their earliest state, these cells are round, and possess the characteristic nucleus, with one or two nucleoli (fig. 35). It seems probable, indeed, that the nucleus exists prior to the cell, and that the latter is gradually formed around it; for M. Schwann observed many nuclei without investing cell, but no cell without a nucleus. When further developed, the nucleated cells become acuminated at two opposite points (fig. 35, *b*); then they appear drawn out at these points into fibres, which are sometimes branched (as at *a*); and at a more advanced stage these fibres resolve themselves into bundles of fibrils (fig. 36). The division into fibrils commences, according to Schwann, at the extremities of the fibres, and gradually advances towards the body of the cell, which at length becomes involved in the change. The nucleus remains visible for a time, lying upon the fasciculus of fibrils, but is afterwards absorbed (pp. 135-9). The accuracy of these observations of M. Schwann seems to be placed beyond a doubt, by the circumstance to which we have before alluded, that similar facts have been described, though in different language, both by earlier and by contemporary writers. R. Froriep had in 1837‡ remarked that the exudations of pericarditis contained

* Report of the British Association for the Advancement of Science, 1838, p. 140.
† Meeting of British Association, 1839. Athenæum, No. dcxx.
‡ Klin. Kupfertafeln, 11te Lief., Th. lxi.—Weimar, 1837.

fibres similar to those of cellular tissue, and also irregular granules, which appeared to be elongated into thin threads in one or two directions. "These elongated fibrinous granules," he adds, "seem to be the first form of the nascent cylindrical fibres of the true pseudo-membrane or cicatrix." Güterbock,* and Valentin† likewise, had observed globules and spindle-shaped bodies, mixed with fibres, in the exudations of inflamed serous membranes and in granulations. Appearances confirmatory of Schwann's doctrine of the mode of development of cellular and fibrous tissue generally, have since been witnessed by Henle, Müller, Breschet, and Simon. In condylomata, in which Simon‡ observed the growth of the cylindrical fibres, we have also had the opportunity of seeing the various forms intermediate between the round nucleated cell and the cylindrical fibre. There is some discrepancy, though none affecting the validity of the general theory, between Valentin's more recent account§ of the development of the fibres of cellular tissue and that given by Schwann. According to Valentin, the primary cells arrange themselves in a linear order, become elongated, and then coalesce, so as to produce at first knotted fibres, the enlargements of which are due to the nuclei of the original cells. By the gradual elongation of these fibres, and the absorption of the nuclei, uniform cylindrical fibres are produced, which subsequently resolve themselves into smaller fibrils. Neither Schwann nor any other minute anatomist appears to have observed this coalescence of cells in a linear series in the process of development of the cellular and allied tissues. There is, however, a tissue investing the minute parts of glands and other organs, which, in the knotted or nucleated form of its fibres, seems to represent in a permanent condition the transitory stage pointed out by Valentin in the growth of the cylindrical fibres of cellular tissue.

Mingled with the primary cells of the cellular tissue, in the embryo, Schwann found also *cells of adipose matter*. They were round vesicles, of different sizes, containing generally one globule of fat which completely filled them. In their wall, which was finely granular or structureless, Schwann recognized a distinct nucleus (p. 140). This nucleus disappears at a later period; but, according to Valentin,‖ is not absorbed, for he was able to distinguish it in emaciated persons, in whom the adipose cells contained only a few granules of fat.

Schwann observed a third kind of cell in the cellular tissue of the fœtus, the nature of which, however, he could not determine.

The fibres of *tendon* are developed by the same process as those of cellular tissue, which they so closely resemble (p. 147).

M. Schwann's investigation into the mode of growth of *elastic tissue*, though it satisfied him that there was here no exception to the general law of development from cells, yet did not reveal the exact steps by which the cells are transformed into the elastic fibres. The appearances, however, which he observed (fig. 37), tended to show that in this tissue the fibres are produced not so much by the elongation as by the

* De pure et granulatione.—Berol., 1837. † Repertorium, 1837; p. 259.
‡ Müller's Archiv, 1839; p. 26.
§ Repertorium für Anatom. u. Physiol., 1838, p. 309; and Wagner's Physiologie, p. 137.
‖ Wagner's Physiologie, p. 135.

expansion and subsequent splitting of the body of the cell (pp. 148-51). Should this prove to be the case, the fibres of elastic tissue would, Schwann observes, hold a middle position, as to their mode of development between the fibres of cellular tissue and tendon, and those composing the cortical structure of feathers.

The cortical substance of the shaft of feathers belongs to the horny structures; but it appeared to us that M. Schwann's observations on its mode of development would be read with greater interest, if placed in connexion with his remarks on the other fibrous structures.

Examined in the imperfectly formed feather, the cortical substance of the shaft is found to be composed of large, flat, somewhat granular laminæ or cells, each of which contains a distinct nucleus, with one or two nucleoli (fig. 38, *a*). An indistinct appearance of longitudinal fibres is observed at the borders of the laminæ in parts more advanced in development. These fibres gradually become more defined, and may be traced through the whole extent of the cell (fig. 38, *b*). At the same time the nucleus begins to be absorbed, and last of all the nucleolus disappears (fig. 38, *c*, *d*). When perfectly developed, the fibres produced by the resolution of the different laminæ or cells become continuous with each other (pp. 97-8). Here, then, no elongation of the cells takes place, and we have an example of the transformation of cells into fibres by mere division.

It seems very probable that the fibres composing all the tissues of this class are really hollow cylinders; for in a form of pigment tissue, which we shall presently describe, the threadlike prolongations of the cells are certainly tubular, the dark colour of the matter contained in them rendering it visible; and Purkinje and Räuschel have seen indications of a cavity in the fibres of elastic tissue. If such is the case,—if the fibres of the cellular, elastic, tendinous, and horny tissues are hollow,—then the first step in the division of their primary cell must consist in the growth inwards of linear septa, and, though the form of tissue resulting is very different, an essential analogy must be recognized as subsisting between the process of the production of these animal fibres and that of the multiplication of cells in vegetables by the growth inwards of septa from the walls of the primary nucleated cells, to which we have already referred.

5. The fifth class of tissues—those which result from the coalescence of the cavities, as well as of the walls of cells—includes the most important and highly-endowed elements of the animal organism.

There are two principal types according to which these tissues are formed. In the one case, the primary cells arrange themselves in a linear series, and then coalesce, so as to form a cylindrical tube, denominated by M. Schwann the secondary cell; in the other, the cells not arranged in a regular order first assume a stellate form, and being at considerable distances from each other, coalesce only through the medium of their radiate processes. The latter is the mode of formation of the reticulated capillaries; the former is the type after which muscles and nerves are developed. We have already described the formation of ducts and other vessels in plants by the same process,—the coalescence of cells and the subsequent absorption of the septa which at first separate their cavities.

The first steps in the development of *muscular fibre* have not been observed by Schwann. Long since, however, Valentin had remarked that, before fibres can be distinguished in the place of the future muscle, globules (probably primary cells) are seen, arranged in longitudinal lines; and that these globules gradually coalesce, so as to form first beaded and then cylindrical threads. The principal facts ascertained by Schwann, relative to the subsequent steps in the process, are as follows: In the fibres which have acquired the cylindrical form, nuclei are still visible. While the fibres are irregular on their surface, the nuclei within them lie very close to each other, and, on account of the granular matter surrounding them, are indistinct (fig. 39, *a*, *b*, *c*). They are rendered very evident, however, by the action of acetic acid (fig. 40). Afterwards, as the fibres become elongated, and somewhat narrower, and lose their granular aspect, the nuclei begin to be more widely separated, and themselves assume a lengthened form. The fibres are now distinctly hollow, and the nuclei lie in the substance of their walls (fig. 41, *a*, *b*, *c*). The cavities of the primary cells have at this period coalesced, so as to form the secondary cell or tubular fibre. A deposition of new matter takes place on the inner surface of the secondary cell, and continues until the cavity of the cylinder is quite filled; and, subsequently, this new matter is converted into the longitudinal muscular fibrillæ which compose the striated muscular fibre. The transverse striæ become visible in the human embryo, according to Valentin, about the sixth month. The nuclei of the primary cells gradually disappear; but the membranous wall of the cylinder is believed by M. Schwann to exist throughout life (p. 156-66).

The Nerves. Schwann has not satisfactorily seen the primary cells from which the nervous fibres are developed. The nerves, in the earliest condition in which he examined them (in the fetal pig), "appeared as pale, finely-granulated cords, in which indistinctly-shaded lines gave some indications of a coarse fibrous structure. Through the whole thickness of these cords, and principally in the course of the shaded lines just mentioned, nuclei, containing one or two nucleoli, were thickly scattered (fig. 42). Occasionally, a single fibre separated itself from the nerve in a part of its extent (fig. 42, *a*), and it then became evident that the nuclei belonged to the fibres, and that several were contained in one fibre" (p. 171). These fibres, which Schwann supposes to be hollow, and to be produced by the coalescence of several primary cells, have a pale, granular aspect, and want the dark outline of the perfectly-formed elements of the nerves (figs. 42, *a*, and 43, *a*). But their identity with the nervous fibres seems certain; for, in nerves further developed than those above described, Schwann found, mixed with the pellucid granular substance, distinct nervous fibres similar to those of the adult animal, and, beside these, other fibres in which the transition of the imperfect nervous fibres into the perfect ones was evident (fig. 43, *b*). The mode in which the transformation is here effected is not so clear. The dark outline of the nervous fibre is due to the same substance which gives them their white colour; and Schwann inclines to the opinion that this substance is deposited, in the same manner as the matter which forms the ultimate fibrils of muscular fibre, on the inner surface of the tube re-

sulting from the coalescence of the primary cells. The only observation, however, which he adduces in support of his opinion is, that in nervous fibres of the adult animal a delicate, structureless, transparent membrane exists on the exterior of the white substance. Once or twice, indeed, he observed appearances (fig. 44) which led him to suppose that the nuclei of the primary cells sometimes remain for a considerable time unabsorbed in this transparent sheath of the nervous fibre (pp. 171-5). But, on this as well as on many other points relating to the development of the nerves, M. Schwann's observations are less satisfactory than his account of the process by which the muscular tissue is formed.

The globules of the nervous ganglia and gray substance of the brain and spinal cord are evidently nucleated cells within which a new substance has been deposited. Schwann has no observations on their mode of growth; but Valentin* has observed that the nucleus is formed first, and that the cell is afterwards gradually developed around it.

The growth of *the capillary vessels*, though a process of such paramount importance in the animal economy, and one which is almost constantly offering itself to our observation amongst the sequels of injuries or disease, has nevertheless baffled, hitherto, the endeavours of the numerous physiologists and pathologists who have laboured to elucidate its nature. Most of the hypotheses which have been framed to explain it have been far too fanciful and mechanical in their character. Schwann's observations on this subject are, confessedly, imperfect; but yet they appear to us so interesting, that we shall give a tolerably copious abstract of them.

Schwann prefaces his remarks on the capillaries themselves by a reference to a particular form of pigment cell which is observed in the skin of the batrachian amphibia. The dark spots in the skin of these animals are dependent on the presence of bodies, chiefly of a stellate and ramified figure, containing pigment granules (fig. 46). Schwann shows that these are really cells; that every stage of the transition can be observed by which ordinary round or polygonal pigment cells with nuclei acquire the radiate form (fig. 45, *a*, *b*, *c*, *d*, *e*, *f*); and that even in their most developed condition each of the ramified cells contains a nucleus. Such are the cells of which Schwann speaks in the following passage:

"At fig. 46 is a representation of two stellate pigment cells which have coalesced at *a*........... At the point where the arms of the two cells unite, their cavities seem to communicate; at all events, there is no apparent interruption to the line of pigment which they contain. Now, if we imagine several such stellate cells to be developed in a surface of considerable extent at similar distances from each other, and if we further suppose the several prolongations sent out by each cell to coalesce with those sent out by the other cells, we shall have an extended network of continuous canals. The size of the meshes in such a network would be determined by the distance of the original cells from each other, and by the number of the prolongations proceeding from each cell. Such appears to be the process by which capillary vessels are formed." (pp. 182-3.)

The observations which led Schwann to believe that the capillaries

* Wagner's Physiologie, p. 135.

are developed by the process described in the above extract, are the following:

"1. The walls of the capillaries in the tail of the tadpole are formed by a distinct though delicate membrane. . . . . .

"2. Nuclei similar to those of primary cells can be distinguished in the walls of the capillaries of the tadpole. . . . These nuclei are not sufficiently numerous to belong to scales of epithelium investing the inner surface of the vessels; and it is, therefore, more probable that they are the nuclei of primary cells, by the coalescence of which the capillaries have been formed.

"3. In the tail of very young tadpoles the capillary network presents, besides the ordinary cylindrical canals in which the blood flows in a regular current, other vessels of an irregular form. . . . . These vessels agree in essential characters with those which I shall presently describe, as found in the germinal membrane of the egg, where, however, the meshes are not nearly so large. The irregularly-formed capillaries to which I refer are usually widest where branches are given off (as at the points *a* and *b* in the diagram, fig. 47), but taper very rapidly as they leave those parts, and then again widen until they form a second dilated part. They present every possible degree of narrowing, from that which would scarcely be remarked, to that where the vessel is reduced to a thread not much thicker than a fibre of cellular tissue (as at *c*). Moreover, from the wide parts of these varicose vessels branches sometimes proceed, which gradually diminish in size, and are lost without reaching another broader part of the network (as at *d* and *e*). According to the view of the development of capillaries which I adopt, these appearances may be explained in the following manner: The wide parts of the vessels represent the bodies of the primary cells. These have sent out in a stellate form hollow processes, which have in most parts met and coalesced with similar processes from other cells. These processes, however, being hollow, are able to dilate during their growth, and consequently can assume the form of the ordinary cylindrical capillaries (as at *f* and *g*). . . . That the vessels here described are blood-vessels, is proved by their direct continuity with the ordinary capillaries, and also by the wider of them being actually entered by blood corpuscules. . . . . To prove, however, that they are capillaries in the stage of development by the process we have imagined, it is necessary to show the existence of the primary cells before their union with the actual blood-vessels. . . . . Stellate cells certainly exist in the same stratum with the capillary network in the tail of the tadpole, and processes from these cells sometimes appear to unite with prolongations of the capillaries. The reality of these anastomoses, however, is not quite certain; and the great number of the stellate cells, and their existence at all ages of the tadpole, are circumstances unfavorable to the supposition that they are primary cells of capillaries. . . . . The uncertainty which surrounds this point appears, however, to be removed, when our observation is transferred to the incubated egg.

"4. The germinal membrane of an egg which has been subjected to thirty-six hours' incubation being examined with a microscopic power of 450 diameters, the capillary vessels are readily distinguished in it by their yellowish-red colour. . . . . In some parts the vessels are perfect; in other parts they have the imperfect form described above as it was seen in the tadpole. In addition to the vessels, however, there are seen here and there in the germinal membrane bodies of irregular form which are not connected with the vascular network. These bodies give off blind processes in different directions, and hence have the appearance of star-shaped cells (fig. 29, *h*, *i*). Their colour is yellowish-red, like that of the true capillaries; which fact is alone sufficient to suggest the suspicion that they are an early condition of those vessels. And this suspicion is strengthened by the fact that some of these bodies (such as *k*) seem to be actually connected to branches of the capillaries. We may, therefore, at least, with a high degree of probability, regard them as primary cells in the act of becoming transformed into capillaries by the process above indicated. . . . . The fluid portion of the blood constitutes the contents of the primary cells as well as of

the vessels produced by their coalescence; and the blood corpuscules are probably young cells developed within the cells which have united to form the network." (pp. 183-8.)

Having thus followed our author in the detail of his anatomical researches, let us examine how far they justify the expectation with which he set out, namely, that one general law would be found to regulate the development of all organic elements, animal as well as vegetable, how different soever their form and physiological endowments. The observations of Schleiden had demonstrated the process of growth of the elementary parts of plants: they had shown that in all cases a body of peculiar conformation is first produced by the aggregation of molecules of a preexisting structureless substance, that a cell is gradually developed around this body, and that the cell subsequently undergoes various modifications, according to the kind of tissue which it is destined to form. The task which Schwann undertook was to ascertain whether this process obtained also in the growth of animal tissues. To prove to demonstration that the same law of development prevailed *universally* amongst organic structures was scarcely possible; but Schwann perceived that the principle would be sufficiently established, if *the greater number* of animal tissues essentially distinct in their physiological properties could be shown to agree with each other and with the vegetable tissues in the process of their formation, and if, on the other hand, no tissue should appear to be developed by a distinctly different process. Viewed in this light, the facts adduced by M. Schwann afford an adequate basis for the theory which he has raised upon them. He shows, 1, that, in all the tissues which he has examined, the presence of a body wholly similar to the cytoblast of vegetable cells may at an early period of their existence be demonstrated; 2, that structures the most various—the ovum and a part of its contents, the globules of the animal fluids, the epithelium and other horny tissues, the crytalline lens, cartilage, fibrous tissues and muscle,—either are produced by the transformation of cells containing each a nucleus (which, like the cytoblast in plants, lies in the substance of the wall of the cell), or permanently retain the form of nucleated cells; and 3, that, in the growth of many structures differing entirely in their form and functional properties,—namely, the ovum, pus-globule, horny tissues, lens, cartilage, and cellular tissue,—the nucleus or cytoblast exists before the cell, just as in the process demonstrated by Schleiden in plants.

Schwann acknowledges that there are one or two apparent exceptions to the law that all tissues are developed from cells. One of these, and the most striking one, we have already noticed, namely, the conversion of the intercellular substance of cartilages into a fibrous structure. But though this instance proves that fibres may be developed in other ways than by the elongation and splitting of cells, yet far from regarding it as a formidable objection to the general theory, we are led by the following reasons to refer the formation of the fibres even here to the agency of cells: In the first place, from the history of the development of cartilage it may be inferred, that its corpuscules or cells are the essential agents in the formation and maintenance of the tissue, and that the solid intercellular substance is a secondary and dependent part. Secondly, in the fibrillæ of muscular fibres, and in the spiral fibres of the spiral vessels of

plants, we have examples of a deposit within cells assuming the form of fibres; and thirdly, since, as we shall presently show, the walls of the cells can certainly produce chemical changes in the matter around them, it appears to us possible that, in the case of the cartilage, they may also diffuse such a plastic influence on their exterior, as in muscle and the vegetable tissues they seem to exert on the depositions within their cavities.

We have already mentioned, incidentally, several observations by other microscopic enquirers which aid in confirming M. Schwann's theory. The labours of Müller, however, being much more comprehensive and important, demand a more especial reference. In his late work on the microscopic structure of morbid growths,* he not only shows the presence in the various diseased structures of nucleated cells wholly analogous to those of the healthy animal and vegetable organisms, but he demonstrates the growth of these cells from nuclei and the development of young cells within parent ones, and points out that the caudate or spindle-shaped bodies, previously noticed by him and others in morbid tumours, are nucleated cells in the stage of transformation into fibres. Müller, indeed, states that there are no microscopic elements of diseased growths which may not be referred to one or other of the various forms which cells assume in the progress of their development into the different natural tissues. Many facts have been observed by Valentin,† also, in the field of morbid anatomy, which accord with Schwann's proposed law. Again, the bodies described by Dr. Hake‡ as constituting the basis of a peculiar disease, met with hitherto only in the liver of the rabbit, are a striking example of morbid nucleated cells, which, as we have ascertained, present in their mode of development no exception to Schwann's law; their nucleus being first formed, and the cell gradually produced around it. The operations of disease, therefore, in the building up of abnormal structures, obey the same general laws which regulate the nutritive processes of the frame in its healthy state.

In the preceding pages our principal aim has been to lay before our readers a faithful analysis of the first and second sections of M. Schwann's admirable work. We have shown the steps by which he was led to infer the existence of an important law, and have given a succinct, but we hope intelligible, statement of the facts which convinced him of the accuracy of his previous reasonings. Another object, however, has been kept in mind, namely, by bringing together the observations of other recent observers, to present a general view of the existing state of knowledge respecting the development of the elementary tissues.

The third and last section of M. Schwann's treatise is occupied partly by a review of the process by which the observations of Schleiden and himself had shown the organic tissues to be developed, and partly by an enquiry concerning the proximate cause or force upon which that process, and vital phenomena generally, depend. It will not be necessary for us to analyze closely the former part of this section. There are, however,

* Ueber den feineren Bau und die Formen der krankhaften Geschwülste. 1ste Lief.—Berlin, 1838.

† Repertorium, 1837 and 1838. ‡ On carcinoma of the hepatic ducts, &c., 1839.

one or two particulars connected with the morphological history of the organic cells, which we have as yet scarcely touched upon, but which, since they are of interest and importance, we must briefly notice. We shall then enter upon the consideration of the active properties of the cells, and of their relation to the vital phenomena of organized beings.

One of the points in the morphology of the cells to which we have alluded respects the situation in which new cells are developed in relation to those already existing. In plants, according to Schleiden, the young cells are always formed within fully-developed parent cells, and never on the exterior of the old cells. In the animal organism the contrary appears to be the rule; the formation of the new cells generally takes place on the exterior and in the interstices of the old ones, though here also there are instances of the growth of cells upon nuclei within parent cells. Thus, Schwann has observed it to occur occasionally in cartilage and in the chorda dorsalis; and the ovum, he remarks, affords a distinct instance of the formation of numerous cells within a parent cell. The observations of Dumortier, also, upon the development of the ovum of gasteropoda (published previous to M. Schwann's researches),* seem to show that in those animals the liver is formed by the successive development of cells within cells. In diseased growths, too, Valentin and Müller, as we have before mentioned, have observed a similar process.

Another circumstance worthy of remark is that of the situation in which the new cells are developed in relation to the mass of the tissue. The formation of the new cells takes place in a fluid, gelatinous, or even solid substance, which lies either in the interior or, more usually, in the interstices of the existing cells. This substance, called by Schwann the "cytoblastoma," affords the matter for the formation of the cells; but, to enable it to perform this important office, it requires to receive constantly fresh supplies of nutritive matter from the blood-vessels. Hence arises the difference between the mode of growth of vascular and that of non-vascular tissues. In vascular tissues the nutritive fluid—the liquor sanguinis—permeates every part, and new cells, which are afterwards transformed into the elements of the tissue, can consequently be developed throughout their substance. But non-vascular tissues, such as the epidermis, being accessible to the nutritive fluid at that surface only where they are in contact with a vascular tissue, the addition to their substance by the formation of new cells can take place only in that situation. This constitutes the sole essential difference between vascular and non-vascular tissues. Both are organized; in both the elements or cells grow by spontaneous assimilation. We now proceed to speak of the active properties of the cells, of which assimilation is only one of the results.

The phenomena presented by the cells during their life within the organism may be referred to two natural groups, denominated by Schwann the *plastic* and the *metabolic* (from τὸ μεταβολικον, that which is prone to cause or suffer change.)

The *plastic* phenomena are those which we have already studied in so extended a manner, as displayed in the development of the different tis-

* Annal. d. Sc. Nat., t. viii., p. 129.

sues, namely, the aggregation of molecules of organic matter in accordance with a definite plan, so as to form successively the nucleolus, nucleus, and cell; the transformation of the cells into the elements of the various tissues; and the deposition of a new substance within the simple or compound cells. These phenomena may be dependent on a force of attraction resident in the nucleoli and nuclei of the cells, as well as in the cells themselves; but it is evident, as M. Schwann remarks, that with this some other power must be combined. For although the cytoblastema contains all the elements from which the component matter of the cells is formed, they exist in it in different combinations. The cells, therefore, must have the power not only of attracting, but also of chemically changing the substances brought into contact with them (p. 234). The phenomena to which this power gives rise are the *metabolic* phenomena; and the power itself is called by Schwann the *metabolic force.*

An interesting example of the action of this force, and one which proves its seat to be in the cells and not in the surrounding matter, is pointed out by Schwann in the process of vinous fermentation. The recent discoveries respecting the nature of fermentation are described in another part of this volume (see p. 579); but the organic cells, on the agency of which the process depends, are represented by fig. 12, *a, b, c.*

A remarkable instance of the metabolic action of cells in the animal organism is presented to us in the change of composition which the substance of cellular tissue undergoes during its development. In its imperfect condition, when the cells are in process of formation, this tissue has been found by Güterbock,* Schwann, and Simon,† to contain pyine, or a substance nearly allied to that principle, and no gelatine, which is well known to be its chief component in its perfect state. During the process of ossification, a parallel change takes place in the animal matter of cartilage. The principal animal constituent of unossified cartilage is chondrine; that of bone is true gelatine. An anatomical fact also of great interest in relation to the chemical action of the cells has been pointed out by Schwann,‡ Purkinje,§ and Henle.‖ They have shown that the essential component elements of all glands are nucleated cells, sometimes disposed as an epithelium-like layer on the interior of tubular canals, at other times united in masses, so as to form nearly solid acini. It cannot be doubted that these cells are in some way connected with the action of the gland, and it is most probable that they are the efficient agents in the secreting process.

We have hitherto spoken of the metabolic phenomena merely as the results of an action of the cells by which they decompose the organic matters in contact with them. But such an action will not explain all the metabolic phenomena of the cells. "The great difference which frequently exists between their contents and the surrounding cytoblastema seems to prove," Schwann remarks, "that the walls of the cells have the power, not merely of chemically changing, but also of sepa-

* De pure et granulatione.—Berol., 1837.
† Müller's Archiv, 1839, p. 26.
‡ Froriep's Notiz, Bd. v., p. 225.
§ Isis, 1838, No. 7.
‖ Hufeland's Journal, May, 1838.

rating the matters on which they act, causing some to collect on their interior, and others on their exterior; just as substances separated by the action of galvanism collect at the opposite poles of the battery." (p. 237.)

Such seem to be the principal organic properties manifested by the cells during their growth and subsequent existence within the organism. Now, when it is considered that the entire organism is composed of elements thus endowed, some other important consequences following from M. Schwann's discovery will be readily apprehended. The similarity of the elementary parts of vegetables, in their mode of growth, and in their vital properties, some time since led botanists to regard the cells as the true individuals, and the entire plants as aggregates of these individuals, associated in accordance with a definite law; and vegetables then appeared to differ, in a most important respect, from animals, of which each was viewed as a single individual, and not as an aggregate of many. Since, however, M. Schwann's researches have shown the organism, even of the highest animals, to consist of similar elements, all developed in accordance with the same law, and each endowed with an apparently independent power of growth and self-nutrition, the idea naturally suggests itself, that here, also, the organism is an aggregate of parts endowed with independent vitality; and that its growth and subsistence depend not on any power residing in itself as a whole, but on the independent self-nutrition and reciprocal reaction of its component elements. As a fact strongly confirmatory of this view, Schwann adduces the persistence of vitality and of growth, under favorable circumstances, in single cells or elementary parts separated from the rest of the organism:

"The ova of animals are examples of cells thus manifesting an independent power of growth when separated from the organism to which they belonged. In the case of the ovum of the higher animals, it may certainly be objected that, when fecundated, it can no longer be compared with the other cells of the organism; that fecundation involves something more important than a mere external vital condition, something more than mere nutriment; that, in fact, the ovum may by that act become endowed with its peculiar vitality, and consequently that no conclusions drawn from the ovum can be applied to the other cells of the organism. But, in the case of those genera of animals in which only the female sex exists, and in that of the lower plants which propagate by sporules, there is no ground for this objection. Moreover, amongst the lowest tribes of vegetables, a single cell may separate itself from the plant to which it belonged, and continue an independent growth. In this instance, then, we have demonstrative evidence of the independent vitality of the elementary cells of plants. Now, since all organic cells grow in accordance with the same laws, and consequently cannot owe their growth in one instance to a power of the whole organism, and in another instance to a property resident in themselves; and since, moreover, some of these cells have evidently an independent power of growth, it necessarily follows that we must ascribe to the cells generally the same independent vitality; in other words, we must admit that the particular combination of molecules existing in a cell is sufficient for the development of the power which enables the cell to attract new molecules. Nutrition and growth depend not on a power residing in the organism as a whole, but on the endowments of the individual cells. The circumstance that *every* cell will not continue to grow, when separated from the organism, cannot be urged as an argument against this theory with any more reason than the vitality of a bee can be denied, because it is unable to live separate from the swarm to which it

belongs. It proves only that the manifestation of the force inherent in the cell is dependent on certain conditions, which it finds only in its connexion with the entire organism." (p. 228.)

The theory of the independent vitality of the component elements of the organism calls to mind Mayer's hypothesis, that organized bodies are composed of "monads," or "biospheres," which in their active state appear as the globules of the circulating fluids, and in other conditions form the more fixed organic structures. Not merely, however, was Mayer's hypothesis based on few and, in part, erroneous observations, but the absurdity of its details bears testimony rather to the lively imagination than to the soundness of the reasoning powers of its author. Besides ascribing to the individual monads of the organism an independent vitality, it endows them with instinctive impulses, a will, reason, and "a knowledge of the laws of chemistry and mechanics which strikes the mind with astonishment!"

M. Schwann's theory is of an entirely different character. The result of careful induction from numerous and well-ascertained facts, it goes not a step further than those facts and that reasoning warrant. M. Schwann attributes to his cells those properties which constitute what is termed organic life, but not those higher functions which are possessed only by the entire animal organism.

It will have been perceived that M. Schwann rejects the hypothesis of a vital principle, the *anima* of Stahl, by which the various nutritive processes of each animal and vegetable organism have been supposed to be regulated in accordance with rational laws, and that he adopts the opposite view, which seems to be rapidly gaining ground amongst physiologists, namely, that the forces on which the phenomena of life depend obey blindly the laws of necessity, and are, like the forces of inorganic nature, dependent on the existence of matter, though manifested only by a peculiar combination of elements. The adaptation displayed in the processes of organic bodies, M. Schwann remarks, may, like that so remarkable in the planetary system, be due to properties given to matter at its creation; in virtue of which, though obeying blindly the laws of necessity, it concurs in the production of a wisely-ordered whole (p. 222). In addition to other arguments in favour of this view, M. Schwann urges the difficulty of reconciling the hypothesis of a ruling vital principle with the independent vitality and power of growth of the elementary cells of the organism.

Having, as we have already shown, arrived at the conclusion that the force on which the vital or organic phenomena depend resides not in the entire organism, but in its elementary parts or cells, Schwann enters, lastly, upon an enquiry into the nature of that force. Here we shall not follow him, but shall content ourselves with stating the conclusion at which he arrives, namely, that the process by which the cells composing organic beings are formed may be compared to crystallization, and that those organized beings themselves may be regarded as the forms in which matters capable of imbibition crystallize. We should, however, be doing M. Schwann an injustice, if we did not state that he himself views this hypothesis as containing very much that is uncertain and paradoxical, and puts it forth only as a guide to further researches (p. 257).

The length and tenor of the foregoing review of M. Schwann's work

evince sufficiently our high estimation of its merits. We shall, therefore, not be suspected of a desire to detract from the high honours due to its author, when we direct attention to a theory of the development of the tissues contained in the writings of Heusinger, which at first view appears like an entire anticipation of the discoveries we have been considering. The following is an extract from the first part of Heusinger's "System der Histologie," published in 1822:

"Matter is the result of the preponderance of the force of contraction over that of expansion. . . . . The equilibrium of these two forces is expressed in the globular form. . . . Hence all organisms and all the elementary parts of organic bodies have at first the globular form. The contest, however, which is expressed in the forces, displays itself also in the matter; hence increased tension of the forces causes the globule (often only apparently homogeneous) to assume the form of a vesicle: and hence all organisms in their development pass from the globular to the vesicular structure. But, whenever in the organism globules and the structureless mass exist, they are led by chemical laws to arrange themselves in a linear order, so as to form fibres. Canals or vessels are produced when vesicles dispose themselves in a similar manner. In accordance with these principles, I refer the tissues of the bodies to three chief formations: 1, that of the structureless matter; 2, that of globules, which includes two groups, the true globules and the fibres; and 3, that of vesicles, in which likewise there are two groups, namely, the true vesicles and the vessels."

On following Heusinger further in his definition of the different groups of organic structures thus marked out, we find other instances of remarkable coincidence between his views and those of Schwann. Thus, his "structureless matter," or "formative tissue," corresponds almost entirely with Schwann's "cytoblastema;" and he describes the process of the formation of vessels in the following terms: "Vessels are vesicles which have arranged themselves in regular series, and have opened into each other. In the formative tissue drops of lymph or globules arise, surround themselves with a polarizing membrane, and become vesicles. These vesicles attract each other, and then their cavities coalesce." . . . .

In the application of his theory to almost all the other tissues, however, Heusinger betrays his ignorance not only of their mode of growth, but also of their structure in the perfect state. In fact, neither he nor any writer at that time was acquainted with the facts which form the basis of M. Schwann's theory. The principle of Heusinger's hypothesis was evidently derived from the speculations of philosophers respecting the forces of inorganic nature, and the statements by which he illustrated its application to organic bodies were founded on imperfect observations, or, in many instances, perhaps, on fancy alone. An hypothesis so ill supported deservedly attracted but little attention, and contributed in no way to the advancement of science. The fate of Schwann's theory has been very different. Deduced by cautious reasoning from well-ascertained facts, and verified by extended investigations, it has already been recognized as the accurate expression of an important law of nature; has found a more extensive application than its discoverer perhaps anticipated; and promises, when followed into its remote consequences, to throw light on many phenomena at present involved in obscurity.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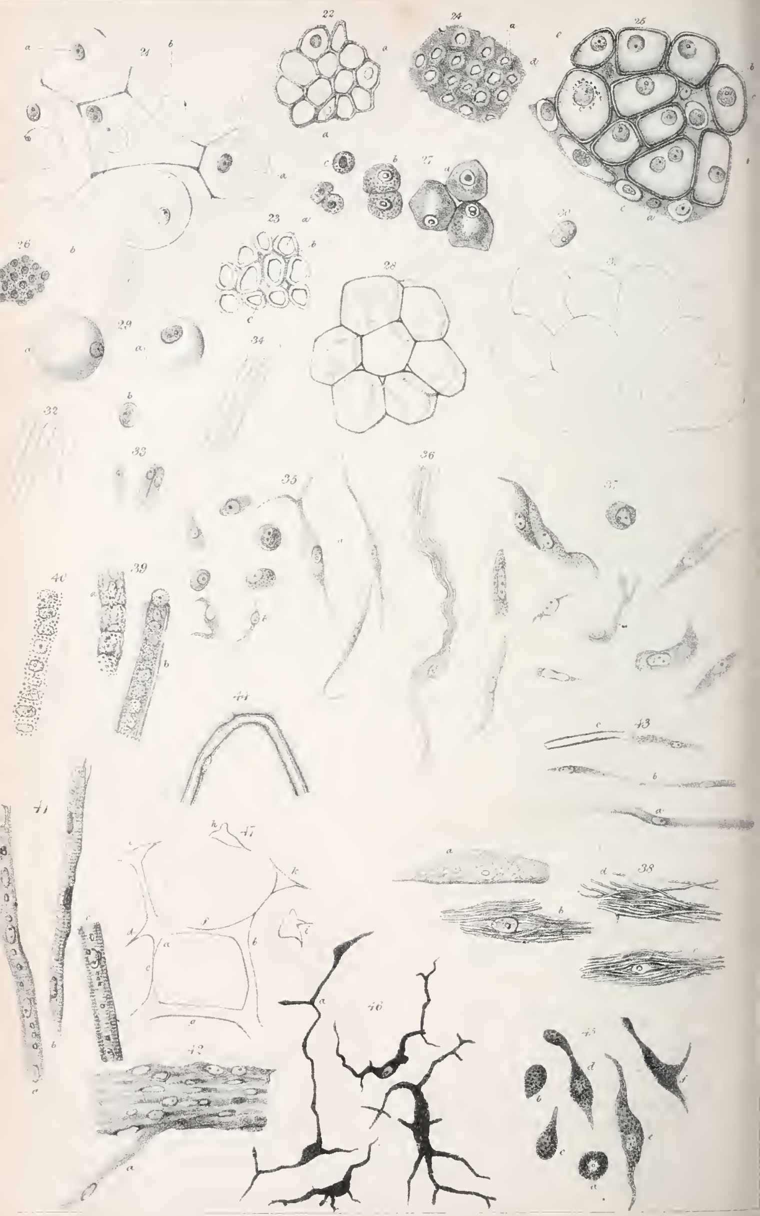


Explanation of the Figures.

Plate II.

Fig. 1. First stage of the formation of cytoblasts from organizable mucus. (Schleiden.)

2. A cytoblast with the cell forming on it. (Schleiden.)

3. A young cell with its cytoblast from *pimelia drupacea.* The cytoblast presents three nucleoli. (Schleiden.)

4. Cell with cytoblast more advanced. (Schleiden.)

5. Progress of organization of albumen in the embryo-sac of *chamædorea schiedeana ; a*, amorphous mass, with granules and cytoblasts beginning to appear, as in fig. 1 ; *b*, new cells still soluble in distilled water ; *c*, *e*, further progress of the same ; in the last, the cells are flattened against each other. (Schleiden.)

6. Portion of articulated hair from *potato*, showing the course of the currents in the cells. (Schleiden.)

7. Single cell from beaded hair of *tradescantia virginica*, showing the cytoblast (nucleus) and course of the currents. (Slack.)

8. Section of cells in advanced state, showing the cytoblasts imbedded in their walls. (Schleiden.)

9. Portion of ovule of *echium vulgare ; a*, embryo-sac ; *b*, embryo. (Schleiden.)

10. Pollen-tube of *orchis morio*, with cells forming in its interior. (Schleiden.)

11. Development of cells in ovule of *phormium tenax ; a*, embryo-sac ; *b*, pollen-tube ; *c*, embryo. (Schleiden.)

12. Vegetation of yeast in various stages ; *a*, isolated vesicles ; *b*, the same with buds ; *c*, further development of the same. (Turpin.)

13. Cells from leaf of *pinus sylvestris*, showing concentric layers of subsequent deposit. (Meyer.)

14. Cells from stone of plum, with sclerogenous deposit in their interior. (Lindley.)

15. Cells from albumen, showing canals of partial communication. (Schleiden).

16. Cells from gritty matter of pear, showing the radiated form of the cavity. (Lindley.)

17. Duct, showing remains of spiral structure in its walls. (Slack.)

18. Plan of the formation of the dotted duct. (Lindley.)

19. Vessels of the latex, from stipule of *ficus elastica.* (Schultz.)

20. Formation of the same from cells. (Slack.)

Plate III.

All the following Figures are copied from Schwann's Plates.

Fig. 21. Cells of the chorda dorsalis of *cyprinus erythrophthalmus ; a*, *a*, the nuclei ; *b*, *b*, *b*, and *c*, vesicles within the cells, perhaps young cells ; *d*, a small corpuscule contained in the wall of a vesicle.

22. Cartilage of the branchial ray of *cyprinus erythrophthalmus*, taken from near the free extremity of the ray ; *a*, *a*, nuclei.

23. The same taken from the middle of the branchial ray ; *a*, cavity of a cell ; *b*, its wall ; *c*, a space filled with intercellular substance.

24. The same from the base of the ray ; *a*, walls of the cells.

25. A lamina cut from near the apex of a branchial cartilage of *rana*

esculenta; *a*, the border corresponding to the free surface of the cartilage; *b*, a nucleus; *c*, *c*, young cells surrounding nuclei; *d*, granular matter deposited around the nucleus within a cell; *e*, a nucleus apparently in the act of formation around a nucleolus.

26. Nuclei not yet surrounded with cells from the medullary part of the shaft of a growing feather of a raven.

27. Cells from the same part in different stages of development.

28. Cells from the interior of the shaft of a perfectly-developed wing-feather.

29. Cells of the crystalline lens of a fetal pig; *a*, *a*, cells each containing a nucleus; *b*, a nucleus with a young cell developing upon it.

30. A nucleus with two nucleoli, upon which no cell is yet developed, from the crystalline lens of a fetal pig.

31. Cells in the process of transformation into fibres of the lens.

32. Enamel fibres from the imperfectly-formed tooth of a fetal pig.

33. Cells from the surface of the enamel membrane.

34. Fibres which compose the ivory of the human tooth in its early condition, separated by maceration during two days in dilute muriatic acid.

35. Cells undergoing transformation into fibres of cellular tissue, taken from beneath the cutaneous muscle of the neck of a fetal pig.

36. A further stage in the development of cellular tissue, by the elongation and splitting of fibres.

37. Cells apparently undergoing transformation into fibres of elastic tissue, from the middle coat of the aorta of a fetal pig.

38. Flattened cells becoming resolved into fibres, from the cortical substance of the shaft of a young raven's feather; *a*, one of the flattened nucleated cells; *b*, the same in greater part changed into fibres, the nucleus remaining; *c*, the same, absorption of the nucleus commencing; *d*, the same, the nucleus wholly absorbed.

39. Fibres from the dorsal muscles of a fetal pig.

40. The fibre *b* of the preceding figure, after it had been acted on by acetic acid.

41. Muscular fibres further advanced in development, from the brachial muscles of a fetal pig. The fibre *b*, viewed sideways, shows that the nuclei are attached to its walls, and not contained in its cavity. The fibres *a* and *c* illustrate the gradual deposition, on the inner surface of the tube, of the new matter which afterwards forms the ultimate fibrillæ; this deposition is further advanced in fibre *c*.

42. Fasciculus of nervous fibres from the brachial plexus of a fetal pig; *a*, a fibre partially separated from the fasciculus.

43. Single nervous fibres in different stages of development; *a*, from the nervus trigeminus; *b*, *c*, from the nervus ischiadicus of the same fœtus.

44. Nervous fibre from the vagus of a calf, showing nuclei apparently inclosed in its sheath.

45. Pigment cells from the tail of a tadpole, in different stages of development.

46. Ramified pigment cells from the tail of a tadpole; *a*, the point where two cells have apparently coalesced.

47. Diagram representing the mode of formation of capillaries in the area pellucida of a hen's egg; described in the text.

PART SECOND.

Bibliographical Notices.

Art. I.—*Elements of Natural Philosophy; being an Experimental Introduction to the Study of the Physical Sciences.* By Golding Bird, m.d., f.l.s., f.g.s., &c.—*London*, 1839. Small 8vo, pp. 407.

We quite agree with the author of this volume in the reason which suggested its compilation,—" the absence of any system of physics, sufficiently extended to include all those subjects with which men of education, especially members of a liberal and important profession like that of medicine, ought and are required to be familiar with; and at the same time not too diffuse to disgust or weary the student." The insulated treatises in Lardner's Cyclopædia, and the Library of Useful Knowledge, however valuable in themselves, are not adapted to convey to the student a *connected* idea of the objects of his pursuit; and they enter into details, in which it is not often desirable that one whose chief attention ought to be given elsewhere should follow them. The admirable Elements of Physics of Dr. Arnott have acquired a high and deserved popularity; but there are several objections to their becoming a text-book for the medical student. The first is a very serious one,—their price; another, even more grave, is the unfinished state in which they appear likely to remain. Moreover, we think that, for those whom it is an object to train in the school of physical science for the better employment of their faculties in the study and practice of medicine, something of a less diffuse manner and less copious illustration may be used with propriety, than that which Dr. Arnott has so judiciously employed as a means of attraction to the general reader.

If a general knowledge of physical science be made compulsory, as we trust it will be ere long, upon the medical student, we are disposed to believe that, if left to his own choice, he will seek to obtain it by attendance on lectures; provided these be made sufficiently attractive, as they may easily be, by experimental and other illustrations. The principles and general facts he will seek to impress more strongly upon his mind by reference to books; and he will require for this purpose a work containing as many of these as can be clearly stated, and made intelligible by application, within a small compass. This appears to have been the chief object of Dr. Golding Bird; who speaks of his work as " chiefly intended as a text-book for the student whilst attending lectures on physics." We are disposed to think very favorably of the mode in which he has fulfilled this purpose. A very large amount of important matter is comprised within a small compass, and it is almost uniformly expressed in a clear and simple manner. In this respect, we know of no other

work at all to be compared to it. The newest doctrines in the rapidly advancing sciences of light, electricity, and electro-magnetism, are stated in a form which will render them easily comprehended by the student who has already made himself acquainted with the old; whilst he who is entering upon them uninformed is not perplexed by prolix discussions, but is led at once to what seems established as truth.

As an introductory work for the solitary student, however, we do not think it so well adapted. It is impossible to fulfil both these objects satisfactorily. The compression required in a text-book for lectures rarely suits the general reader. Dr. Alison's philosophic Outlines of Physiology are distasteful to the student, until he hears them expounded and illustrated by their author. As a mere *elementary* work, therefore, we do not recommend Dr. G. Bird's volume; but as an accurate and faithful guide to the study of physical science, we have great pleasure in strongly urging its merits upon our readers.

The scope of this work very nearly coincides with the first portion of the outline which we have given, in another part of this Number (Art. VI.), of a proper course of instruction in physical science. The following is a sketch of its contents. After an introductory discourse, in which the *forms* of matter are treated of, as well as the general principles of natural philosophy, the first nine chapters are devoted to the *Physics of Ponderable Matter*, under the following heads: I. General Properties of Atoms and Masses of Matter. II. Attractive Forces exerted between Masses. III. Bodies in Motion or General Dynamics. IV. Effects of Gravitation. V. Theoretical Action of the Simple Machines. VI. Fluids at Rest, or Hydrostatics. VII. Gases at Rest, Aerostatics, or Pneumostatics. VIII. Fluids in Motion, or Hydro- and Pneumo-dynamics. IX. Sonorous Vibrations of Ponderable Bodies, or Acoustics.

We think that a chapter on astronomy ought to have been given after the fourth. In this, without prolix details respecting the planetary system, the complex effects of the great principle of gravitation might have been displayed, as has been admirably done by Mrs. Somerville, in her "Connexion of the Sciences." We hold such *deduction* to be fully as valuable an exercise of the mind as *induction*; and, where a principle is known that accounts for *all* the phenomena to which it can be supposed to relate, its full application should be indicated. In no other respect have we any fault to find with this portion of the volume; and we trust its author will see the propriety of attending to our suggestion when the opportunity is afforded him.

The second division of the volume comprehends the *Physics of Imponderable Matter*,—a term which we are disposed to think objectionable both in itself and as not expressing the present state of opinion as to the nature of electricity, magnetism, &c. This, however, is not a matter of great importance, since we do not perceive that the text has been affected by the idea contained in the title. This division contains sixteen chapters, of which the titles are as follows: X. Magnetism. XI. Primary Phenomena of Ordinary Electricity. XII. and XIII. Consequences of Electrical Induction. XIV. Phenomena of Atmospheric Electricity. XV. Voltaic Electricity. XVI. Electro-Dynamics. XVII. Electro-Dynamic Induction. XVIII. Thermo-Electricity. XIX. Organic

Electricity. xx. General Properties and Catoptric Phenomena of Unpolarized Light. xxi. Dioptric Phenomena of Unpolarized Light. xxii. Chromatic Phenomena of Unpolarized Light. xxiii. Phenomena of Double Refraction and Rectilinear Polarization. xxiv. Chromatic Phenomena of Polarized Light. xxv. Optical Apparatus, and the Eye considered as an Optical Instrument.

As electricity is Dr. Golding Bird's favorite object of pursuit, and a department of science in which he has greatly distinguished himself, it is not to be wondered at that rather a disproportionate space should be allotted to it in his work. We speak, however, more with regard to the *objects* of his volume than as to the relative importance of the *principles* of gravitation and electricity in an abstract point of view. It does appear to us, however, that the details given under the latter head might be sufficiently abbreviated to admit of the corresponding amplification which we have suggested in the former.

We are at a loss to account for the omission of *heat* from this division of the work. It unquestionably stands in very close relation both to light and electricity; and, if forms of imponderable matter can be conceived of, it is certainly entitled to rank among them. No less than one fourth of the whole volume is devoted to light; and here, too, we think that a little retrenchment may be advantageously made, for the admission of a subject so nearly allied, and of so much importance.

The volume, as it at present stands, approaches so closely to our idea of what such a treatise *should be*, that we have dwelt longer upon these trifling defects than we otherwise should have thought it worth while to do. The student who had made himself master of it, amended as we had proposed, would then be qualified to enter with advantage upon the study of chemistry. This would lead him to mineralogy on one side, and physiology on the other; and these two branches of science meet again in geology. We think that an additional volume, embodying the principles of chemistry (and especially dwelling upon the inorganic division of this science) with those of mineralogy and dynamical geology, would form a most appropriate continuation of the present treatise; and it could scarcely be executed by any one better qualified for the task than the author of this work.

Art. II.—*Observations on Yaws, and its Influence in Originating Leprosy; also Observations on Acute Traumatic Tetanus, and Tetanus Infantum.* By James Maxwell, m.d.—*Edinburgh*, 1839. 8vo, pp. 134.

This treatise on yaws was an inaugural dissertation, for which the senators of the university of Edinburgh awarded their gold medal to the author. Judging from the internal evidence afforded by the work itself, it seems to be written by a practitioner of considerable experience, not as these productions usually are, the compilation of a mere student. It is the most complete treatise which has yet appeared on a formidable disease which in this country has attracted no attention, as fortunately it is unknown; and even where it is prevalent, has been much disregarded by British surgeons, and left to the care of the blacks themselves. Those who are liable to be called on to treat the disease, would

do well to read Dr. Maxwell's book, and to try his remedy, hydriodate of potash. Appended is a sensible paper on tetanus infantum, which disease, from its extreme frequency and fatality, has always been one of the chief causes of decrease among the West Indian negroes. Dr. Maxwell found, from many years' observations, that twenty-five per cent. of all the negro children died of this disease in Jamaica. Dr. Hancock has asserted that it kills half the infants in many of the other islands. According to Dr. Maxwell, the sole cause is the neglected state of the navel, and that the disease may be prevented. The midwives apply turpentine, tobacco ashes, bark, &c., to the funis, wrap it up in burnt rag, apply a bandage, and give strict orders not to remove the dressing until the funis drops. With admirable consistency, they give the infant rum and castor oil until the ninth day, and keep it and the mother in a room from which air is excluded, and then light a fire.

The following extract illustrates Dr. Maxwell's prophylactic treatment, and the success with which it was followed:

"I was consulted by a lady, on account of the ravages which the disease was making on her plantation; she informed me that twenty-five infants had perished in a few years from locked-jaw. The women were removed from the negro-houses, and delivered in one of the airy apartments of the mansion-house, under her immediate superintendence; yet, notwithstanding all her care and attention, the mortality was as great as it had been under less favorable circumstances. The treatment was, to give the child a dose of castor oil shortly after birth, to dress the navel with bark, and each day to add a fresh portion to absorb the acrid matter, without washing it till it dropped. I advised her to change her method of dressing the funis, and to bathe it carefully with milk and warm water once a day, or twice if it was large, and after each ablution to apply a pledget of cerate or spermaceti ointment, carefully abstaining from purgatives and irritant dressings. Slight as this alteration was, she entered upon it with a determination to give it a fair trial, and her efforts were crowned with perfect success; for she lately informed me that, since its adoption, for several years past, she has never lost a child from tetanus."

There are few curses, we are daily convinced, more dreadful than the curse of nurses. The elderly ones are the incarnation of worn-out prejudice; every absurdity of a bye-gone day is treasured up and transmitted by some of these people, who like filters catch all that is worthless, and let the clear liquor run through. Some centuries ago the worst of them were called witches, and were burnt, and there was "wild justice" certainly in such treatment. Slavery is bad enough, but what are the whips and scorns of slave-drivers to grown-up men, compared to this turpentine to the tender navels of sensitive babies, with rum and castor oil for food, and a close black-hole warmth, ending in tetanic convulsions, and a mortality of twenty-five per cent.? The sufferings of white infants are not few or inconsiderable from the same sources: what with castor oil, opiates, gruel, gin and sugar, bandages round their chests to "support" them as they call it, and such like means, having all the authority of tradition; but these sink into nothingness, when compared with the deeds of the black midwives. If Dr. Maxwell has at all succeeded in introducing, even partially, a better system, he will have overcome no slight difficulties, and is deserving of much praise.

ART. III.—*The Intimate Structure of the Secreting Glands, by* JOHN MÜLLER, M.D., *with the subsequent discoveries of other Authors.* By SAMUEL SOLLY, F.R.S., Lecturer on Physiology, and on Comparative Anatomy, at St. Thomas's Hospital, &c. &c. With four plates, and woodcuts.—*London*, 1839. 8vo, pp. 166.

IN pursuance of his laudable endeavour to excite attention in this country to the great truth that physiology can only be pursued with the certainty of success when it is based on comparative anatomy, Mr. Solly has brought the substance of Müller's splendid monograph on the glandular system within the means of the English student; and he has incorporated with it several contemporaneous and subsequent discoveries made by other enquirers. Thus we find in this little volume the analysis of Dr. Boehm's observations on the intestinal glands, which appeared in our own pages; the description given by Mr. Morgan of the curious mammary apparatus of the kangaroo; a summary of Mr. Kiernan's researches on the minute anatomy of the liver; and notices of the points in which Sir A. Cooper differs from Müller on the anatomy of the testicle. Besides these larger additions, we have allusions to Hunter's labours, of which Müller knew little or nothing, and which had anticipated him (as well as almost every one else) in many points: and of some recent enquiries on the part of Mr. Owen. We could have wished that the long extract from Mr. Corfe's book on the kidney had not been inserted. The doctrines, both anatomical and physiological, which it contains, are so strange, that they must be received with suspicion; and, although Mr. S. speaks of having seen Mr. C.'s preparations, he does not appear satisfied of the accuracy of the description of them which he has quoted.

In an appendix we find a full account of Dr. Sprott Boyd's observations on the structure of the mucous membrane of the stomach, and of the researches of Bischoff and Purkinje on the same subject; as well as a summary of Dr. John Davy's late description of the male organs of some cartilaginous fishes.

From this summary of the contents of Mr. Solly's volume, it will, we think, appear that it is one well worthy of the student's acquaintance. The analysis of Müller's work is, on the whole, well executed, and the other selections are made with judgment. The chief fault we have to find is, that the materials are not very well blended together. The student will in vain look elsewhere, however, for the same amount of information in so narrow a compass and at so cheap a rate. We must not omit to notice the four lithographs, which contain upwards of seventy subjects, copied chiefly from Müller's folio plates, and well drawn by Mr. Solly.

ART. IV.—*Insecta.* By GEORGE NEWPORT, Esq. (*The Cyclopædia of Anatomy and Physiology.*)—*London*, 1839. 8vo, pp. 128.

THE work of which this article forms no unimportant part continues to advance regularly though slowly; but we shall not complain of this, so long as the period of delay is so well occupied as it must have been

in the preparation of this most elaborate essay. It is one which does credit to the judgment of the editor in the selection of the writer, as well as to the author in the combination and arrangement of his materials. We much question if any country could furnish a naturalist better qualified for such a task than Mr. Newport. To unwearied zeal in his pursuit, he unites great powers of observation, and sagacity in the employment of opportunities of research. He never allows himself to be led astray by theory, but honestly states facts. A very large proportion of this article consists of original matter; and the remainder is drawn from the most authentic sources. After a general view of the position of insects in the animal scale, an outline of their classification is given. The different states of the insect—larva, pupa, and imago—are then noticed, as they present themselves in the chief subdivisions of the group; and the principal phenomena in the life of each are detailed. The external anatomy of the perfect insect is then described with great minuteness; we should say too minutely for the character of the work. Details of this kind are of great importance to the naturalist, but of much less consequence to the comparative anatomist, and of still less to the physiologist. The muscular and nervous systems are then treated of; and the changes which the latter undergoes during the metamorphosis are exhibited. The organs of nutrition, secretion, and reproduction, with their respective functions, then come under review. We could have wished that the author had been a little fuller on some parts of this division, and had curtailed the former. We should have liked to have seen a general account of the food of insects, and of the functions of the group in the economy of nature; and, under the head of reproduction, some details relative to the extraordinary increase in number occasionally observed. However, we receive with much thankfulness what we have got; and we hope that Mr. Newport will long continue to prosecute his labours with the skill and enthusiasm which he here displays.

We cannot dismiss this notice without once more formally calling the earnest attention of our readers to the Cyclopædia of Anatomy and Physiology. It continues to possess the great excellence which it manifested at its commencement, and is, indeed, a work of unequalled value to the physiologist and comparative anatomist.

Art. V.—*Outlines of Physiology; with an Appendix on Phrenology.* By P. M. Roget, m.d., Secretary to the Royal Society, &c. First American Edition, revised, with numerous Notes.—*Philadelphia*, 1839. 8vo, pp. 516.

This volume contains the treatises on physiology and phrenology contributed by Dr. Roget to the new edition of the Encyclopædia Britannica. They have been republished separately in this country, and, from the influence of Dr. Roget's name we presume, they have been reprinted almost immediately in America. Numerous additions, several of them of much value, have been made, in the form of notes, by the American editor—we believe Professor Dunglison; and the treatise on phrenology,

instead of preceding the other, as it did in the English edition, is now converted into an appendix to it. This we deem its right situation, as phrenology, after all, is but a branch of physiology, and, to be well studied, must be studied on exactly the same principles. The character of the work has been well given by the American editor: "The present volume contains a concise, well-written epitome of the present state of physiology, human and comparative; not, as a matter to be expected, the copious details and developments to be met with in larger treatises on the subject, but enough to serve as an accompaniment and guide to the physiological student." It is written in that clear and simple style which characterizes all Dr. Roget's productions; and it is seldom behind the present state of knowledge. There is one remarkable inaccuracy, however, to which our attention was directed by a correctional note. The ultimate structure of the brain and nerves is described according to the now antiquated observations of De la Torre, Monro, the Wenzells, Edwards, &c.; and no allusion whatever is made to the fact, now satisfactorily ascertained by the observations of Ehrenberg, Berres, Valentin, and others, of their tubular character. Moreover, we find the medulla oblongata spoken of as the special seat of sensation—a doctrine which few now uphold. The chapter on the vital powers, on the other hand, contains a most excellent and condensed view of the chief questions which this subject involves. We are not altogether satisfied that the general character of this treatise is suitable to the work for which it was written. We should have been disposed to have made such an essay either more popular, by omitting the less important details and more fully explaining others, or more scientific, by including such higher generalizations as may be considered sound and permanent. We should have thought, too, that the human physiology ought to have followed, instead of preceding the comparative. But we shall presume that Dr. Roget was not altogether left to his own choice in the matter, and that the editor of the Encyclopædia is the party responsible for the general character of the treatise.

Art. VI.—*Rudiments of Animal Physiology, for use in Schools, and for Private Instruction.* By Dr. G. Hamilton, Falkirk. (*In Chambers's Educational Course.*)—*Edinburgh*, 1839. 18mo, pp. 104.

It always gives us pleasure to notice any work in which the elementary principles of physiology are adapted to popular comprehension; since we feel assured that the general diffusion of sound knowledge on this subject is intimately connected with the progress of mankind in *all* those objects which the true philanthropist regards with the deepest interest,—the physical, moral, and intellectual improvement of his race. During the whole course of our labours, we can truly say, we have not met with a stronger claim upon our warm approbation than that which this unpretending little work possesses. As one of a series which is already producing a great improvement in popular education, especially in Scotland, we were predisposed in its favour; and a careful examination of the treatise itself has satisfied us that it is *by far the best* of its class, in point of matter and illustration, whilst it is at the same time the

cheapest. It possesses a great advantage over all others that we have seen, in being written by one who has himself been concerned in giving elementary instruction on the subject. Dr. Hamilton mentions in his preface that, under his superintendence, the master of the Falkirk parochial school has for the last four years made physiology a part of the regular course of instruction; with the two good effects of interesting his pupils and communicating to them knowledge of practical value. This circumstance has led him to append to each section of his work a list of books which may be advantageously referred to by the teacher, and also concise instructions for the preparation and exhibition of the parts described. The woodcut illustrations are numerous and well executed; those illustrative of the circulating system are coloured. These illustrations are principally taken from the "Elemens de Zoologie," an elementary work by one of the first naturalists now alive, of which we formerly spoke with high approbation. In this country, men of scientific reputation think it beneath them to write elementary books; and hence these are generally the production of men of superficial acquirements, who introduce into them much that the student has to unlearn if he advances further. We have looked through this little book with much attention, for the sake of being able to give a positive opinion on its scientific merits; and we have much pleasure in stating that it appears to us to give *sound* and *lucid* views on almost every subject discussed. The only point of consequence in which we think that the work might be improved, is in the account given of the respiratory process, and of the dependence of animal heat upon it. Dr. H. does not seem to be aware how universally Crawford's theory is *now* abandoned by the best-informed physiologists, and how readily the phenomena of animal heat are explicable upon the doctrine now generally received. As he quotes Dr. Alison in support of his views, we would refer him to p. 251 of the *last* edition of Dr. A.'s "Outlines;" and also to chap. xii. of Dr. W. B. Carpenter's "Principles of Physiology." The page in question might be easily cancelled and replaced by another; and we should then regard the work as unexceptionable. We do not think that our professional brethren can do a better service to their younger friends, and to schools with whose conductors they have any influence, than by urging its employment amongst them. We hope that this is not the only time that Dr. Hamilton will present himself to the public.

The true bearing of works like the present, and their signal importance to the public, are so admirably stated in the *editor's preface*, that we cannot resist the pleasure of making an extract from it, and with this we shall conclude our notice:

"While it has been justly objected to popular medical guides and dictionaries that, read under the influence of imperfect knowledge, they tend rather to mislead than to instruct, and probably induce more diseases than they cure, a candid mind must regard in a very different light the various works which have been published of late years for the purpose of conveying a popular knowledge of animal physiology. The sole and certain result of these must be, by giving a familiar knowledge of the human organization and its laws, to put individuals into the best possible condition for *avoiding* diseases—a very different thing, indeed, from the attempt to *cure* them. The utility of this knowledge to the non-medical community is now beginning to be generally felt, though still some perhaps require to be convinced of it. To such persons it might be pointed out

that, though almost all, from the communications made to them in childhood, or from their own sensations and experience, are enabled to observe some of the more obvious laws of organization—as, for a familiar instance, those respecting simple overloading of the stomach,—there are others of those laws which most persons, for want of knowledge, are constantly breaking, to the great injury of their health. The general prevalence, amongst men of business, of an over-tasking of the brain; amongst ladies, of a neglect of out-of-doors exercise; the almost universal indulgence in stimuli of various kinds; and the tight-lacing of young females; are but a scantling of the errors which we everywhere see around us, as the result of a want of knowledge of the structure and functions of our physical frames. It is only, indeed, where the infraction of any organic law is followed very immediately by its appropriate penalty, as simple over-eating is by indigestion, that ordinary knowledge observes and records the fact. In the far more numerous, and generally much more important class of cases, where the effect is not to be readily traced to its cause, popular knowledge is completely at fault: nothing can there be of avail but a knowledge to some extent of the human organization and its laws. It may be true that the knowledge itself will not be sufficient to produce, in all, a proper attention to the rules of health; yet it is pretty clear that the knowledge *may* have such an effect, while without it nothing of the kind can be hoped for. It might also be expected that, were a knowledge of our internal organization thoroughly familiarized and made present to every mind, public opinion would become engaged in causing individuals of a negligent disposition to observe the laws of health. It would be thought wrong for a man having a family depending on himself to expose his life to hazard by daily-endured mental exhaustion; and a young lady entering a room with a waist reduced to half its proper circumference, would be shrunk from as a kind of monster. This knowledge would operate not only in a direct way upon individuals, but through one individual upon another."

Art. VII.—*Animal Mechanism and Physiology.* By John H. Griscom, m.d., Professor of Chemistry, &c.—*New York*, 1839. 18mo, pp. 357.

This is another work having the same laudable object, and by no means deficient in qualifications for success. It is principally compiled from Sir C. Bell's "Animal Mechanics," Dr. Arnott's "Physics," and Dunglison's "Physiology;" and therefore it is not likely to contain much harm. The author's physiology is, however, a little antiquated in many instances; and we cannot recommend it as a guide on that subject. We are surprised that our friends on the other side of the Atlantic pay so little regard to the style of typography, and especially to the manner in which their works are illustrated. From the announcement of "Woodcuts by Butler," in the title-page, we anticipated at least mediocrity in their execution; but we are obliged to declare that a worse set of illustrations never came before us. With one or two exceptions, the cuts are badly drawn, worse engraved, and printed worst of all. Indeed, the whole getting-up of the book is far inferior to that which we are now accustomed to see in the cheap popular works of our own country. In this respect, Messrs. Chambers have done wonders; and it is the neatness of their cheap and unpretending treatise which has led us to speak rather strongly on the subject of Messrs. Harper's more bulky and expensive one. We should certainly recommend our professional brethren in the States to send their drawings to England by the Great Western, and we will warrant their being well transferred to wood. Our only fear is that, when they are brought into

use, they will be spoiled in the printing; as it is well known that the best wood-engravings may be, in bad hands. We beg the excuse of our friends in New York for our plain speaking; and assure them that it will give us sincere pleasure to receive a correction in the shape of a well-executed series of woodcuts of *any* description.

ART. VIII.—*On the Anatomy of the Breast.* By Sir ASTLEY P. COOPER, Bart., F.R.S., D.C.L., G.C.H., Serjeant-Surgeon to the Queen, &c. &c.—*London*, 1840. 4to, pp. 265. With a volume of Folio Plates.

THIS beautiful and most valuable treatise has reached us at so late a period that we are barely able to announce its publication. We shall give a review of it in our next Number; but we cannot allow the present to appear without bearing to all our readers our earnest exhortation, that they do not wait for our detailed report, but possess themselves of the book itself. It is one which ought to be in the hands of every scientific member of the profession. It is one, moreover, which even the best reviewer can do but imperfect justice to, and which will prove a sure stumbling-block and rock of offence to the superficial scribbler. The text, which contains nothing superfluous, cannot be exhibited, even in fragmentary beauty, on the slender thread of the scissors-critic, "like orient pearls at random strung;" while the plates—those provoking plates—bid defiance to the pen of the most determined excerptor. The plates in the present work, by the way, are singularly fine, and seem to leave nothing to be done by the followers of Sir Astley in the same path. They contain innumerable figures, most carefully drawn, and many of them admirably coloured. There are, in all, twenty-seven plates—viz., fourteen of the female breast, three of the male breast, and ten of the comparative anatomy of the breast.

ART. VIII.*—1. *Anatomical Tables, containing Concise Descriptions of the Muscles, Ligaments, Fasciæ, Blood-vessels, and Nerves. Intended for the Use of Students.* By THOMAS NUNNELEY, Lecturer on Anatomy, &c., in the Leeds School, &c.—*London*, 1838. 12mo, pp. 240.

2. *Text-Book of Anatomy for Junior Students.* By ALEXANDER J. LIZARS, M.D. Part I.—*Edinburgh*, 1839. 12mo, pp. 272.

3. *The Anatomist's Manual; or, a Treatise on the Manner of Preparing all the Parts of Anatomy, followed by a Complete Description of these Parts.* By J. P. MAYGRIER, M.D.P., &c. &c. Translated from the last French Edition.—*London*, 1839. 12mo, pp. 564.

WE have classed the above "Manuals" together, because their professed object is one and the same: viz., to assist the learner in finding the high-road to anatomical knowledge, and because neither our space nor inclination would dispose us to yield to each a separate and lengthened notice.

1. Mr. Nunneley's "Tables" are designed, as he informs us in his preface, "to assist in the understanding and recollection of anatomy," rather than to teach it; and is by no means his wish or intention that it

should, in the least, usurp the place of those more extended and valuable books which are usually in the possession of the anatomical student. About this our author need not be uneasy, i. e., unless the student who employs his tables be disposed to *book* his anatomy, like a parrot. We can have no objection to Mr. Nunneley's classifying system, provided its use be limited to its intention, as a means of rubbing off the rust which will collect upon the anatomical knowledge of those who have ceased to use the knife.

2. Dr. Lizars's "Text-book" (which we presume will be completed in the second part) is a more extended manual of anatomy, well adapted for the use of the student in the dissecting-room, and, we may add, both nicely arranged and carefully and clearly written. The author's attention, in planning the compilation of a work like the present, was, as his preface informs us, drawn to the subject by "observing how much students are generally occupied, during lecture time, in taking notes of the anatomical demonstrations which are then being delivered." We quite coincide with Dr. Lizars in deprecating this practice, and have always impressed on our own pupils the folly of wasting that time in writing which should be occupied in observing, whilst the subject is purely demonstrative. Where physiology is combined, we are disposed, on the contrary, rather to encourage note-taking, within a limited compass, as the attention is thus kept alive, and the impressions so received and preserved are valuable, as being readily revived, and as forming links which are capable of recalling much more than is actually written.

3. M. Maygrier's work possesses many of the valuable characteristics of the French school of anatomy; yet we see nothing in it to make us yield the preference we have long entertained for the precise style and accurate information of Cloquet. Our author certainly begins, *ab initio*, with the "definition, origin, progress, mode of examining, and teaching anatomy;" and then proceeds to its "general and particular divisions." If we may judge by the style, the translation is accurate, even to a fault; for in almost every page the eye lights upon quaint expressions and gallicisms, which, not to mention their involving bad grammar, are disagreeable to read, and not well fitted for the use of young people. With the exception of this fault, we have no reason to complain of want of accuracy in the anatomical descriptions.

Art. IX.—*The Cyclopædia of Practical Surgery.* Edited by W. B. Costello, M.D. Parts IV-V.—*London, Jan. March,* 1840.

We are much pleased to find, by the appearance of the present parts, that the publication of this important work, after a long suspension, has recommenced, and is not likely to be again interrupted.

The editor informs us, "that the cause of the delay has been entirely removed, and that in future the parts will be published with punctuality," every alternate month. We are willing to believe the additional assurance of the editor, "that the interval has been profitably occupied in the improvement of the general plan, and in securing the cooperation of additional contributors of the most distinguished reputation." In-

deed, if excellence can be assured by numbers or by celebrity, surely the Cyclopædia of Surgery will be excellent, as we think the list of contributors on the cover exceeds, in both respects, that engaged in any previous work of the same kind. To be sure, out of the ninety names we have counted there, some (we shall not say how many) might be dispensed with without detriment to the reputation or probable utility of the work; but there would still remain enow to constitute a phalanx of which surgical science might be justly proud.

The present parts contain 27 articles (*Ankylosis—Burns*), by 17 different writers. They are of various value, but on the whole they are good, and some are excellent. On a former occasion, in noticing this work, we felt it to be our duty to protest against the vitiation of our language, by the unnecessary and improper introduction of foreign words and phrases. We are sorry to say that the Parts before us are not altogether free from the same fault; as where we read, "Every areole or cell of the deep surface of the derm;" "if the inflammatory irritation that assails the cellulo-adipose pellets lodged in the areoles of the derm can be arrested, the disease aborts;" "when the granulations have become *regularized*," &c. We hope the learned editor will see to this, and not forget that he is responsible for the language as well as the subjects of his articles; and that if English surgery does well to search other nations for the materials of its structures, it has at home a language wherewith to build them, that assuredly needs no foreign aid of ornament.

Art. X.—1. *An Atlas of Plates Illustrative of the Principles and Practice of Obstetric Medicine and Surgery. With Descriptive Letterpress.* By F. H. Ramsbotham, m.d., Physician to the Royal Maternity Charity, &c. Nos. I. II. and III.—*London*, 1840. 8vo.

2. *Illustrations of Midwifery, a Complete Atlas and Companion to all Obstetric Works.* By M. Ryan, m.d., &c. Parts I.-V.—*London*, 1839-40. 8vo.

These two works being of the same size and price, and nearly on the same plan, look as if they were the speculations of rival publishers, simultaneously suggested by the recognition of an acknowledged want in obstetric literature, and each put forth in the hope of winning the favour of the profession for itself. If this is really the case, the rivalry, we expect, will be such as between Hyperion and a Satyr, or between the Great Western and a Thames lighter; the whole "getting-up" of Dr. Ramsbotham's publication being as elegant as that of Dr. Ryan's is the reverse. "The Atlas" is, indeed, not only one of the neatest but one of the cheapest books we have met with for a long time; each number containing no less than six neatly-engraved steel-plates, besides numerous beautiful woodcuts, and from two to three sheets of handsome letterpress—all for eighteenpence! The "Illustrations," on the other hand, contain only, in each number, four plates, and these wretched specimens of lithography. The subjects of the plates seem, also, as different in conception as they are in execution in the two works. Indeed, several of the figures in Dr. Ryan's work, as, for instance, those in plates

15, 16, 17, 18, seem to us as utterly useless as they are disgusting, and will recall to the inspector the disagreeable impressions excited by some other works of unhappy notoriety published by the same author.

We strongly recommend the work of Dr. Ramsbotham to all our obstetrical readers, especially to those who are entering upon practice. If it proceeds as it has begun, it will be, when completed (announced to be in twelve monthly numbers), not only one of the cheapest, but one of the most beautiful and most useful works in midwifery.

Art. XI.—1. *New Remedies: the method of preparing and administering them; their effects on the diseased and healthy economy, &c.* By Robley Dunglison, m.d., Professor of the Institutes of Medicine in Philadelphia College.—*Philadelphia*, 1839. 8vo, pp. 503.

2. *A Compendium of Materia Medica and Pharmacy, adapted to the London Pharmacopœia, embodying all the new French, American, and Indian Medicines, &c.* By J. H. Lane, m.d., &c.—*London*, 1840. 12mo, pp. 308.

3. *A Compendium of Materia Medica, Pharmacy, and Toxicology. For the Use of Medical Students.* By D. Spillan, m.d.—*London*, 1839. 12mo, pp. 80.

We class these works together, because their general subject and object are similar: but this is their only resemblance.

1. The work of Dr. Dunglison is by far the completest that has yet appeared on the subject of the additions to our therapeutic means in these latter years; and it is, like all the productions of this accurate and industrious writer, full of information of great value to the practitioner. Each substance is treated of at great length, and mostly under the following heads: Method of preparing; Effects on the economy in health; Effects on the economy in disease; Mode of administering. Under this last head, many formulæ are generally given, original or selected from the best practical authors, British, American, or continental. The copiousness with which the different articles are treated of may be judged from the number of pages occupied by a few of the best known: thus, hydrocyanic acid occupies fifteen; creosote twenty-six; iodine twenty-eight. We strongly recommend this work to the notice of practitioners. It is equally suited to British as to American practice.

2. Dr. Lane's little volume is on the same general plan as Dr. Thomson's long-known and popular Conspectus; but it is much fuller in its details, more especially in the chemical department. It seems carefully compiled, is well suited for its purpose, and cannot fail to be useful.

3. Dr. Spillan's pamphlet is little more than a tabular view of the materia medica, presenting, in distinct columns, their name, effect, uses, and doses. It is intended and is only suited for the mere student.

Art. XII.—*Proceedings of the Border Medical Society. Instituted June* 27, 1838. Parts I. and II.—*Kelso,* 1838-9. 8vo, pp. 26.

We have been much gratified by the perusal of these small fasciculi, and still more by contemplating the honorable and zealous spirit from which they spring. To see established an association for the cultivation of medical science and the promotion of the noblest and most dignified objects of our profession, in so limited and retired a district as that to which this society is confined, and, yet more, to find this small body of country practitioners (not exceeding twenty-four in all) reading at their meetings, and then printing for circulation among the members and their friends, excellent medical memoirs, is surely to witness signs of times which our forefathers could have hardly dreamed of, and of which we may be justly proud. The following are the praiseworthy objects of this society:

"1. The increase of knowledge of the medical topography of the district, through statistical, meteorological, geological, and botanical enquiries. 2. Collection of useful information, whether speculative or practical, through original essays or reports, founded on private practice, on that of public institutions, or on medico-legal investigations before the provincial courts. 3. The investigation of epidemic and endemic diseases; tracing their connexion with peculiarities of soil or climate, or with the circumstances, habits, and occupations of the people; and ascertaining, in as far as the means of knowledge will permit, such local causes as may influence the character of diseases occurring within the district. 4. Maintenance of the honour and respectability of the profession generally in the district; and promotion of friendly intercourse and free communication amongst its members."

In another part of our Journal we shall select one or two passages from the "Proceedings" of this Society, to which we cordially wish all success. We recommend the following excellent observation, which forms the concluding paragraph of a well-written report on the diseases of Kelso, by Dr. Wilson, to the attention of all those (and we fear they are not few) who are disposed to undervalue the case-keeping and note-taking industry of practitioners in the ordinary walks of the profession:

"It is, of course, manifest, that we are not to expect important discoveries to start up before us at every stage of our progress; neither is the detection of novelties to be considered as by any means essential to our full success. He who gives to the most familiar truth a greater stability or a more extended application, or who confirms, by additional proofs, principles which may have been previously discovered but have never been fully evinced, and so contributes to render more secure the base upon which science is founded, is engaged in no unimportant or ungrateful task. It is by the spirit of cautious observation and induction, by the slow stealing from fact to fact, that the empire of knowledge is to be firmly established, if its first conquests are sometimes to be achieved by more brilliant and rapid movements. And surely, engaged as we are in the study of one of the noblest and most comprehensive departments of human science, there is not one of us so mean that he is entitled to derive no incitement to perseverance from the words of the illustrious Bacon:—"Non abs re fuerit etiam notare, facultates vel mediocres, si in magnos viros aut res magnas inciderint, graves et insignes interdum producere effectus."

Art. XIII.—*A Practical Treatise on the Diseases of the Eye.* By William Mackenzie, m.d., Surgeon-Oculist in Scotland in Ordinary to her Majesty, Lecturer on the Eye in the University of Glasgow. *To which is prefixed an Anatomical Introduction, Explanatory of a Horizontal Section of the Human Eyeball.* By Thomas Wharton Jones, Surgeon. Third edition, with two copper-plates, and numerous woodcuts.—*London*, 1840. 8vo, pp. 923.

We have had for some time in preparation a general article on ophthalmology, but the repeated accession of new books to our list has hitherto interfered with its completion. The work before us is, however, one of so much importance, so marked an influence having been exercised by the preceding editions of it on the state of ophthalmic medicine and surgery in this country, that we feel we should be neglecting our duty did we delay in giving a general notice of it at least.

As to the character of the work, it is not necessary for us to say much. The sale of two large editions in this country, the reprint of it at Boston in America, and the German translation of it published at Weimar, are sufficient proofs of its practical value. In the present edition, much new matter has been added: amongst other things, is an article on phlebitic ophthalmitis, and another on sympathetic ophthalmia, besides several new woodcuts; but, by enlarging the page, the work has been kept within its former bounds. "The whole," we are informed, "has again been carefully revised, and such alterations made as have been suggested by the author's continued experience, or by his perusal of the writings of others." "He trusts," the author further says, "that on every occasion he has endeavoured to treat his fellow-labourers in the same field with becoming deference and perfect fairness; never appropriating to himself the labours and improvements of others, but acknowledging openly what he has borrowed." Dr. Mackenzie's extremely full and accurate references are everywhere a proof of what is here expressed. Would we could say his fellow-labourers have always been as ingenuous towards him!

In regard to the anatomical part of the work by Mr. Wharton Jones, we are informed in the advertisement that, for this third edition, the author "has revised his Horizontal Section of the Eyeball, and Anatomical Introduction, comprehending in the latter the most recent discoveries in the anatomy of the eye." The anatomy of the appendages of the eye is not given. The sketch of the anatomy of the eyeball is brief but comprehensive; it and the figure of the horizontal section are mutually illustrative. For what is wanting in detail, full references are given to other works. In conclusion, Dr. Mackenzie's work may be truly characterized as the most complete treatise, theoretical and practical, on the diseases of the eye extant in any language.

Art. XIV.—*The Medical Portrait Gallery.* By J. S. Pettigrew, f.r.s. Vol. IV.—*London*, 1840.

The last Part (xxv.) of this publication has just made its appearance, and completes, we are sorry to say, not merely the volume but the work. We were in hopes that the encouragement extended to Mr. Pettigrew's labours would have enabled him to proceed for another year or two; as

we, in common with many others, were anxious to possess good likenesses of many other friends, ornaments of the profession, who probably would have come within the scope of the author's plan. As we have freely criticised this work on former occasions, with the good intention of improving it, we cannot finally dismiss it without recommending it as an elegant, amusing, and instructive publication. It ought to be in the library of every member of the profession, at least, who either cares for handsome books or loves to encourage literary enterprise.

This volume contains portraits and memoirs of Galen, Harvey, Cullen, Brown; of Sir James M'Grigor, Drs. Hall, Powell, Roget, Young; and Messrs. Copeland, Guthrie, Pettigrew, and Stafford. The whole of the last Part is occupied by an autobiography of the author, which is extremely amusing.

Art. XV.—*Observations on the Management of the Poor in Scotland, and its Effects on the Health of the Great Towns.* By W. P. Alison, m.d., f.r.s.e.—*Edinburgh,* 1840. 8vo, pp. 198.

We hope soon to have an opportunity of treating at large of the subject of this small but most valuable work; we cannot, however, allow the present quarter to pass without expressing our sense of its vast importance, and offering our tribute of praise and admiration to the patriotic and benevolent physician who has called the attention of his countrymen to it, in a tone which cannot be misunderstood, and will hardly, we think, be resisted. The object of Dr. Alison is to prove the necessity of extending the provisions of the English Poor Law, or a modification of it, to Scotland,—a necessity founded on the melancholy fact (too fully proved in his pages) of the total insufficiency of private or non-compulsory charity to supply the poor in Scotland with food and clothing adequate to the prevention of disease and death to a frightful extent.

"In following out this enquiry I have long since formed," says Dr. Alison, "and do not scruple to express an opinion, which I cannot expect to be in the first instance either well received or generally credited in this country, viz., that the higher ranks in Scotland do much less (and what they do less systematically, and therefore less effectually,) for the relief of poverty and of sufferings resulting from it than those of any other country in Europe which is really well regulated; and much less than experience shows to be necessary in any long-inhabited and fully-peopled country, in order that the lower ranks may be maintained in tolerable comfort, and a proper foundation laid for their religious and moral improvement." (*Preface,* p. viii.)

The natural result is, that in Edinburgh and Glasgow "the annual proportion of deaths to the population is not only *much beyond the average of Britain, but very considerably greater than that of London!*"

We earnestly recommend this book to the attention of our readers both in England and Scotland, and beg they will lose no opportunity of naming it and its terrible subject to all their friends in the legislature of this country. It cannot fail, sooner or later, to lead to a legislative enactment to cure the evils depicted in it;—an enactment which will hallow the name of Dr. Alison to the poor of all Scotland for ever, as it is already hallowed to the poor of his native city by the devotion and services of half a lifetime.

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

On the Injurious Qualities of Menstrual Blood, and on its Probable Causes. By Dr. Remak, of Berlin.

This paper is chiefly useful for the information it affords respecting the composition of a fluid which few have either the inclination or the liberty to examine. The author adduces but little fresh evidence in support of the universal popular belief that menstrual blood can produce urethral discharge or other affections of the male genital organs; but he thinks that the occasional occurrence of these symptoms, of which he has no doubt, depends on the condition, not of the blood, but of the mucus with which it is mixed.

By microscopical examination of the menstrual fluid under various circumstances, he found that whenever it had a red colour, it contained blood-globules, and that the intensity of its colour depended on their number. At the beginning and towards the end of its flowing, when it is pale and whitish, it contains a preponderating quantity of laminæ of epithelium and of mucus-corpuscles, but their quantity becomes proportionally less and less as the fluid presents more blood-globules and a deeper colour. The author believes that this mucus which is constantly present in the menstrual fluid, may, like the secretions of all mucous membranes when in a morbid state, possess the power of producing a similar morbid state in other membranes of the same kind. He relates a case which confirms this idea: a very obstinate gonorrhea, in which the menstrual fluid of the only female with whom the patient had connexion, and in whom there was not the least probability of gonorrhea existing, contained an abundance of thin pearl-coloured mucus. Connexion did not produce the gonorrheal symptoms except when the female was menstruating.

Medicinische Zeitung. Dec. 25, 1839.

Evolution of Heat in Plants. By MM. Vrolik and Vriese.

Some additional experiments on *colocasia odora* (see B. and F. M. R., Vol. IV., p. 25,) have been recently made by MM. Vrolik and Vriese. Besides affording a complete confirmation of the results obtained by others, they have established some new and important facts. A flowering spadix placed in oxygen was compared with a corresponding one in atmospheric air. Within half an hour after its immersion, its temperature rose 2·7° C. (4·9° F.), whilst that of the one in air was falling 1·1° C. (2° F.) During and after the emission of the pollen in both, the maximum temperatures were the following:

Spadix in Oxygen	30·6° C.	87·1° F.	Spadix in Atm. Air	27·8° C.	82° F.
Surrounding Temp.	23·3° C.	74·0° F.	Surrounding Temp.	22·8° C.	73° F.
Difference	7·3° C.	13·1° F.	Difference	5·0° C.	9° F.

The temperature of the spadix in oxygen rose much more rapidly than that of the other; so that early in the day the difference was 4·6° C; that of the surrounding media being ·3° in favour of the lowest, so as to make the difference really 4·9° C., or 8·8° F., whilst that of the maxima of both was 2·3° C., or 4·1° F. In these experiments, a large quantity of oxygen was observed to disappear; on the other hand carbonic acid was liberated, which, being absorbed into the water below, caused it to rise into the vessel.

A spadix beginning to flower was placed in nitrogen. It had already acquired an elevation of temperature to the amount of 2·6° C. This gradually diminished, although the expansion of the flower continued. At the time of the emission of the pollen there was no elevation; whilst a corresponding spadix in the open air showed an elevation of 8·6° C. No change in the bulk of the confined gas was noticed in this experiment.

These experiments afford a striking confirmation of the doctrine now rapidly gaining ground amongst physiologists, that the evolution of caloric from living beings is dependent upon certain organic processes, in which the combination of oxygen (either immediately or remotely derived from without) and the carbon of the system are principally concerned. These go on most actively during the period of inflorescence; but they are not restricted to it. The same process of *respiration* is constantly going on in almost every part of the living plant; but, owing to its feeble amount, and the extended surfaces by which the caloric is dissipated as fast as it is generated, it is difficult to acquire any evidence of its liberation.

By the use of the thermo-multiplier, however, which was so successfully employed by MM. Becquerel and Breschet in their experiments upon animal temperature, M. Dutrochet has succeeded in proving the existence of a *proper heat* in plants, more especially in the green parts, which are the seat of the more active organic changes. In order to obtain unequivocal evidence to this effect, it was necessary to devise means of excluding the other causes which produce an elevation of temperature in some parts of the vegetable structure. Thus, in a growing tree, the sap which rises in spring, being principally drawn from a depth of two or three feet in the soil, will be warmer, perhaps even by several degrees, than the atmosphere. On the other hand, the evaporation constantly going on from the soft and moist surfaces of rapidly-growing parts, will depress their temperature. M. Dutrochet employed in his experiments plants either growing in pots, or cut off and placed in water of the same temperature as the surrounding air; and he did not rely upon the temperature of the stem, but upon that of the leaves and young shoots, in reaching which the sap would have acquired the temperature of the plant. He thus avoided the first source of error. The second was prevented by making his comparison, not between these parts in a growing plant and the surrounding atmosphere, but between corresponding parts in a living plant, and in one recently killed by immersion in hot water, and therefore susceptible of the diminution of temperature which evaporation causes, to the same degree as the living plant. The experiment was in some instances still further prevented from error by the immersion of both plants in an atmosphere saturated with aqueous vapour. With these precautions the result was *constantly* the same. An elevation of temperature to the amount of one fourth or even one third of a degree (Cent.) was observed in the herbaceous parts of actively-growing plants, differing with the species, the activity of vegetation, and the period of the day. There is a sort of diurnal paroxysm, which generally manifests itself about noon; in some species an hour before, in others an hour after. The *proper heat* increases up to this period, attains its maximum, and then diminishes. The access of this paroxysm depends in part upon the presence of light; but it will occur, though in a diminished degree, for two or three days in a plant withdrawn from the influence of that agent.

Annales des Sciences Naturelles, N. S. Botan. Tom. xi.-xii.

Experiments on the Motor and Sensitive Roots of the Nerves.
By Dr. KRONENBERG, of Moscow.

DURING my stay in Paris in July (1839), Prof. Magendie had the kindness to show me his recent experiments on the nerves, which he had communicated to the Academy. These, as made in my presence, I will here detail, in the first place, and then notice my own experiments made some time afterwards.

1. M. Magendie laid bare the facial nerve of a dog near its origin and pricked it; at each prick a demonstration of pain was given. The nerve was then cut across, not far from its junction with the fifth pair. Pricking and pinching the cerebral end of the divided nerve produced no indication of sensibility; but pinching the other end obviously caused pain. It appears from this experiment that the sensibility of the facial nerve, as well after as before its union with the fifth pair, depends on this last, and not from any other anastomosis or the influence of the brain sent direct through its origin. M. Magendie then laid bare the roots of the lumbar nerves of a dog. The posterior roots were found as usual very sensible; the anterior were less so, still the dog whined and gave evident signs of pain each time they were pinched. M. M. then divided the motor root; pinching the spinal extremity of this produced no pain, but pinching the other (before its union with the sensitive root) caused the dog to cry out.

2. Some weeks after this I performed the following experiments on rabbits: The facial nerve before its union with the fifth was found sensible, at one time more so, at another less; on dividing it before its union, pinching its under (peripheral) end produced pain, yet not always. I then laid bare the lumbar portion of the spinal marrow, and found the motor roots sensible, but much less so than the sensitive. The following experiments, however, prove that the sensibility of the motor roots is not derived through fibres coming directly from the spinal marrow, but is dependent on the other (the sensitive) roots. When I stimulated the motor root, the sensitive root being undivided, pain was evinced, but when the latter was divided the same stimulus produced no sensation. Magendie's experiment equally proved this, and its repetition by myself confirmed the same. After division of the anterior root, the posterior being untouched, the under or peripheral end was always sensible, the upper not. The case is similar with the anterior column of the spinal marrow—pain being only produced when the posterior roots were uninjured.

In order to settle this point still more firmly, and to ascertain the course of the fibres, I made the following experiment: I made in the angle of union of the two roots a small incision, about half a line in extent, leaving both the roots untouched: it was then found that the same phenomena no longer took place; thus, the anterior root and the anterior column of the spinal marrow were now insensible, and on the division of the root both its ends were equally insensible.

This simple and easy experiment proves, first, that a portion of the fibres of the sensitive root extends to the point of union and is reflected back to the anterior column of the spinal marrow; and, secondly, that the return or reflection of the fibres takes place near the point of junction of the two roots.

[These experiments afford a satisfactory explanation of phenomena which have been very perplexing to physiologists. It has been almost constantly observed that some degree of sensibility appeared to exist in what Sir C. Bell regarded as exclusively the *motor* roots of the nerves; this sensibility being manifested by expressions of pain on the part of the animal when they were irritated. The sensibility of the portio dura has long been known, and was correctly attributed by Sir C. Bell to its reception of filaments from the fifth pair. A similar mixture of the filaments of the posterior roots of the spinal nerves with those of the anterior—these filaments passing *towards* the centre as well as *from* it—may be reasonably anticipated; and this supposition fully explains all the phenomena which have led Magendie and others to the opinion that the anterior roots are sensible. The supposed motor properties of the posterior roots are fully accounted for by Dr. Hall's discoveries; since irritation of these will produce reflex actions through the motor nerves distributed to the same parts.]

Müller's Archiv. Heft. v., 1839.

On the Nerves of the Cornea. By Dr. Pappenheim.

The cornea has been represented by M. Hippolyte Cloquet to be devoid of nerves as well as of blood-vessels. In 1830, however, Schlemm succeeded in tracing nerves as far as the margin of the cornea, though its density prevented his following them between its laminæ. The investigation thus commenced has been pursued by M. Pappenheim, who by a simple process, that of immersing the cornea in acetic acid, or a solution of caustic potass, has been able to trace nerves from the sclerotica into the substance of the cornea. That the nerves thus traced really belong to the cornea, the following facts appear to prove: 1. If the corneal conjunctiva is removed, the nervous filaments are seen on the inner, not on the outer surface of the corneal epithelium. 2. The removal of the iris and membrane of the aqueous humour makes no difference as to the ease with which the nerves may be seen. 3. The nerves are distinctly visible entering the margin of the cornea, but less so towards its centre, where they are ultimately lost between the laminæ.

The nerves may be distinguished from the folds of the choroid which mark the cornea by being colourless, and of uniform thickness; from the fibres of the cornea by being smaller, more superficial, scattered, and arranged in plexuses. Internally they are covered by the membrane of the aqueous humour, externally by the fibres of the cornea. The readiest way of discovering them is to immerse the dissected cornea in water, placing it between two plates of glass, with its inner surface turned upwards—gentle pressure, and the light of a lamp are required, and at first a slightly magnifying lens will be desirable. The nerves will then be seen taking the course of the blood-vessels, and composed of fasciculi for the most part separate.

Monatschrift für Medicin, &c. June, 1839.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Aneurism of the Ascending Aorta. By M. Dubois.

At the meeting of the Royal Academy of Medicine of Paris, 5th Dec., 1839, M. Dubois exhibited an interesting specimen of morbid anatomy, illustrative of the tendency of aneurismal as well as other tumours, to approach the surface, notwithstanding every natural obstacle. The diseased vessel was the ascending aorta, from which, while yet within the pericardium, might be seen a tubular prolongation, of the diameter of two inches, proceeding at right angles directly outwards, and divided near its middle in two unequal parts, by a kind of circular valve. Externally, this tube communicated with a large pouch of condensed cellular tissue, lying beneath the pectoral muscles, and extending into the axilla. Three intercostal spaces had been perforated, not only the intercostal muscles, but one of the ribs having been absorbed as the tumour grew, so that the rough edges of the bone were in contact with the fibrous coagula of the aneurism.

The whistling respiration and œdema of the upper extremities observed during life clearly arose from the pressure of the tumour.

[The foregoing case confirms the truth of the observation that aneurisms of the arch of the aorta more commonly rise towards the sternal surface of the chest. Corvisart, Duverney, Morgagni, and others have given instances of this kind.]

Bulletin de l'Académie de Médecine. Dec. 31, 1839.

On the Various Circumstances which appear, in the Course of Diseases, to produce the Curved Form of the Nails. By M. Vernois.

This paper is sufficient to determine the value, or rather the want of value, of curved finger-nails as a sign of disease. The results, obtained from the

examination by the numerical method of 276 cases of various diseases, are as follows:

In any collection of patients, whatever be their diseases, curved nails will be found in at least one third. Among different diseases, phthisis, scrofula, and other chronic affections, have a very marked, though not an absolute or constant influence. Of 88 patients with curved nails, 70 were phthisical or scrofulous; and of 188 with normally formed nails, 40 were suffering from the same conditions. Of the same 88, nine only laboured under acute diseases. Females present this alteration about three times more frequently than males. It is most commonly observed between the ages of ten and thirty. Occupation has no influence upon it. The constitution which coincides with it in five sixths of the cases in which it occurs is that which is marked by a pale, fine, and anæmic skin, blond hair, blue or brown eyes, bluish sclerotica, very long eyelashes, and weak muscles.

Archives Générales de Médecine. Nov., 1839.

Apoplexy of the Eyes. By Dr. HOLSCHER.

A ROBUST country girl, of eighteen, was hard at work in the harvest field one hot day, and had been frequently stooping to bind up sheaves, when, without any external cause, but with great congestion of blood about the head, she became suddenly blind. When examined a short time after, all sight was lost; the eyes were fixed and felt tense; the conjunctiva was very vascular, there was considerable congestion about the head, and the carotid arteries pulsated strongly. The pupils were widely dilated, and did not in the least contract even when a strong light was brought close to the eye. In the right eye, on viewing it from the side, a slight red tinge of the aqueous humour was perceptible; and with a lens a very small coagulum of blood was discovered lying at the bottom of the anterior chamber. The patient, except for her blindness, was in the same robust health which she had always enjoyed.

Feeling no doubt that the sudden loss of sight was produced by an equally sudden congestion of the eye by blood rushing, as in apoplexy, into the vessels of the choroid and retina, and producing paralysis of the latter, the author ordered a bleeding, to sixteen ounces, from the arm, cold lotions to the head, a saline foot-bath, saline medicines, and an extremely low diet. By a continuance of similar means, with the addition of mercury and a permanent blister at the back of the neck, the patient was so far recovered on the fourth day as to be able to distinguish objects placed before the eyes, and in three weeks she returned to the country with her sight perfectly restored.

Hannoversche Annalen. Band ii. Heft. 4.

New Clinical Researches on Sanguineous Concretions formed during life in the Heart and large Vessels. By Professor BOUILLAUD.

[OF two articles recently published with the above title, the first has no claim thereto, as it is simply an abstract of the opinions of Corvisart and Laennec, and of the author's previous writings on the subject of cardiac concretions. Of the second, in which M. Bouillaud endeavours to prove that the phlegmasiæ in general, and especially pneumonia, exercise a powerful influence on the formation of the coagula in question, the following summary contains the more important particulars. One case of angina tonsillaris, and fourteen of pneumonia, are briefly reported as furnishing the grounds for the doctrine.]

I cannot believe that the large masses of coagula found in the cases just related were wholly formed some days before death, although they possessed the characters assigned to concretions of that date: they were no doubt in part formed in *articulo mortis*, and after death. The mechanism by which concretions of this latter species are produced may be compared with that regulating the coagulation of the blood removed from the vessels during life: in both cases

there was a sort of plastic white fibrinous buff, so that these concretions, like the clot, may be termed *buffy*. Be this as it will, it is indubitable that in every case of acute fatal pneumonia, which has occurred in my wards within the last three years, I have found sanguineous concretions, *evidently formed in part before death*, in the heart and blood-vessels; whence I have no hesitation in establishing it as a LAW of pathological anatomy, *that these coagula exist invariably in subjects who die of well-marked acute pneumonia advanced to the second or third degree.* From our knowledge of the notable tendency of the blood to coagulate in direct idiopathic inflammations of the heart and vessels, we might have *à priori* announced that the same phenomenon would occur in the case of the prolonged febrile reaction accompanying inflammation of important organs. For, in truth, what is this febrile reaction, unless a *sympathetic* irritation of the sanguineous system? [Fever is, in the language of M. Bouillaud, merely an *angeio-carditis!*] This is so true, that in some instances we find actual inflammation of the internal membrane of the heart and aorta, instead of a simple sympathetic irritation. In pneumonia, too, the proximity of the inflamed parts to the heart and great vessels must not be forgot, as a condition facilitating the development of inflammation in the latter. [After enumerating the principal signs (none of these are novel) by which he was enabled to diagnosticate with more or less confidence the existence of cardiac concretions in these cases, M. B. continues:] The diagnosis of sanguineous concretions of the heart and great vessels is not a matter of mere curiosity. On the contrary, it is of real importance, in establishing the prognosis of pneumonia, to ascertain whether or not the complication in question exists. This complication seriously increases the patient's danger, when the concretions are abundant, and form a considerable obstacle to the free circulation of the blood through the cavities of the heart and great vessels. [But a moment since the writer laid it down as a LAW, that these concretions invariably exist in the second and third stages of pneumonia; he now bases his favorable prognosis in inflammation of the lungs on their *absence*.] In a therapeutical point of view also, this is an important consideration. It is not very unusual to find the pulse embarrassed, weak, and small, even when the patient himself is strong, the inflammation intense and extensive, and the heart's pulsations energetic. Under these circumstances, the majority of practitioners recommend us not to bleed; they consider the smallness of the pulse as an infallible sign of radical debility. But the truth is, that the phenomenon in question depends on the presence of coagula in the heart; bleeding is specially indicated for its removal, and, as I have ascertained in numerous instances, restores the pulse to its accustomed fulness, and raises, instead of depressing, the patient's strength.

L'Expérience, Nos. xcvi. and c., *Mai*, 1839.

On a Contagious Growth of Conferva on the Water Salamander (Triton punctatus). By Dr. A. HANNOVER, of Copenhagen.

THIS disease was first accidentally observed in the summer of 1838 on a dead salamander, which Dr. H. having dissected had put back into the same vessel with several living ones. After some days he found it covered with a confervoid efflorescence, which, as seen through the water, looked like mucus. Some of the living salamanders in the same vessel had had the tips of their tails cut, to observe the process of their reproduction; in some the tail had been quite removed, and in others the spinal cord only had been divided. The same efflorescence soon appeared in the wounds of all that had been thus operated on. It always began growing on the surface of the wound, and thence spread forward on the sides of the remains of the tail, giving all the parts that it attacked a dark colour. If it reached the anal aperture the animal always died. Sometimes, however, the cuticle separated and the efflorescence fell off with it, and the animal recovered. At first also it could be scraped off with the cuticle; the subjacent skin was then usually found smooth, but of a dark colour from com-

mencing gangrene. In this case, about sixteen hours after, the efflorescence would have again sprouted more thickly than before, and could not now be scraped off, probably because it had struck root in the cutis. In this short time it would reach the height of half a line, and in hours more of a line; sixteen hours later it would be half an inch higher, it would reach the anal orifice, and the animal would die.

Even a slight injury, such as the prick of a needle, was followed by the same phenomena, and in some cases the efflorescence appeared without a wound. The toes were in several cases thus affected without previous injury, the efflorescence shooting out in a tuft from them, and after a time falling off with all that it had infested, so as to maim one or more of the extremities. The same animal was liable to be thus affected several times.

The plant that produces this disease is a conferva of a similar kind to that which commonly grows on dead flies. It grows very rapidly, often half an inch in four or five days; then it ceases to increase in length, but the transparency which it had at first gradually changes to a whitish colour, and its filaments become covered with buds. The filaments are simple or very rarely branched membranous tubules containing a granular substance. They are of varied diameter, and pointed or sometimes swollen at their ends, and they are often partitioned into cells. When quite ripe the granular matter leaves the interior of the tubes and adheres to their outer surfaces.

Having established these observations, and meeting with M. Audouin's work on the Muscardine, Dr. Hannover determined on testing the contagious nature of the plant by inoculating unripe confervæ into the back of a healthy salamander. After sixteen hours they had grown and were a line high; the animal was severely affected, and swam with his back bent forwards, but he generally remained quiet at the bottom of the water. Sixteen hours after the efflorescence had extended over the whole wound, and was a line and a half high; in sixteen hours more it was two lines high, and its ends were covered with buds. The animal was now about to cast his skin, and after sixty-four hours it fell off with the efflorescence and left a perfectly smooth surface. The wound appeared healthy though not reduced in size, and as soon as the efflorescence was removed the animal became unusually lively.

Three days after, the same salamander was again inoculated in the same place and again recovered after casting his cuticle. A third time he was inoculated with ripe confervæ, and again recovered after presenting the same series of phenomena.

A very lean and weak salamander was similarly inoculated. In twenty-four hours the conferva had grown a line high, and he died shortly after. Another of the same kind was inoculated with conferva from a dead fly; he soon after exhibited signs of severe pain, turned on his side, and struck out violently with his tail; but after casting his cuticle he recovered in three days.

Müller's Archiv. Heft iv., 1839.

Remarkable Case of Ischuria Renalis, of Nine Years' Standing, with Vicarious Vomiting of Urine. By F. L. Kreysig, M.D., Dresden.

S. B., æt. twenty-five, six years after the comencement of menstruation, began to complain of abdominal pain, general spasm, and difficulty in voiding urine and fæces, succeeded by orthopnœa, and symptoms of thoracic and abdominal inflammation, which were treated antiphlogistically. The bladder required the frequent use of the catheter. The patient suffered pain in the right knee, and was obliged to keep it drawn upwards towards the abdomen. Two years after, the legs began to swell, respiration became still more difficult, the urinary secretion *ceased*, and the patient vomited a fluid containing, according to chemical analysis, the principles of urine.

M. Kreysig now detected, in the right iliac region and near the spine, a tumour extending towards the liver, exquisitely painful to the slightest touch,

and which the patient had noticed for four years. The bowels were invariably costive; and twice a day (five, a.m., and six, p.m.) urinous vomitings occurred, preceded by dyspnœa, so urgent as twice to require v.s. No urine was found in the bladder. Tepid baths, and mercurial and stimulating liniments to the region of the kidney, were recommended. During the time the patient was in the baths, the skin exhaled a very fœtid odour. The tumour gradually enlarged, and grew softer.

The patient now returned home, and four months after (January 24, 1835,) informed M. Kreysig that the tumour had daily increased, become harder and more painful, and pointed in the epigastric region. Fluctuation was now evident, and in three weeks the abscess broke, during a violent spasm, discharging a large quantity of pus, which was soon after succeeded by an increase of suffering. In about six weeks, the urinous vomitings ceased, and urine was voided by the bladder. Shortly afterwards, the pain being then more than usually intense, with an urgent desire to evacuate the bowels, the patient expelled, per anum, a mass about as large as a goose's egg, resembling fat mixed with pus, and intolerably fœtid. From this period, all abdominal swelling and pain ceased, and menstruation returned. In about three-quarters of a year, the health was entirely restored, not one of the former symptoms remaining.

Hufeland's Journal. July, 1839.

On the Use of Hydrochlorate of Baryta in Strumous Ophthalmia. By Dr. Payan, of Aix in Provence.

Dr. P. having observed that this remedy was used with excellent effect by Lisfranc in scrofulous diseases, he resolved to try it in an obstinate case of ophthalmia accompanied by a high degree of photophobia. The patient was six years of age. He dissolved two grains of the hydrochlorate in three ounces and a half of "eau sucrée," and this quantity was taken in portions during the course of the day. No particular effect being produced, on the third day three grains were taken, and the dose was gradually raised to ten grains in the course of the day. On the twentieth day the medicine was discontinued, the patient being considered nearly well. In another case, twelve grains in the course of the day were taken with excellent effect, and without any symptoms of gastric irritation being produced. During the administration of the remedy, Dr. Payan orders a light and sparing diet, considering that harm is frequently done by the tonic medicines and stimulating regimen which are generally ordered as a matter of course in strumous ophthalmia. It is true that the scrofulous constitution is often accompanied by an atony which is marked by a pale and pasty complexion, feeble circulation, a low degree of sensibility, and a general indolence of mind and body; but the same scrofulous disposition is frequently developed in persons of a lively temperament, animated countenance, habitually quickened circulation, and mobile and easily excitable nervous system. It is in this latter class of persons that we most frequently find that acute sensibility of the retina which produces photophobia, with the accompanying spasmodic contraction of the eyelids, and abundant secretion of tears. In these cases, a generous diet with wine and tonics will increase the general excitement, and consequent irritability of the retina, whilst the barium appears to act as a sedative, and combined with a mild diet produces a general soothing effect. *Revue Médicale. April,* 1839.

On the Treatment of Dysentery with Albumen. By M. Saucerotte, Physician to the Hospital at Lunéville.

The dysentery which annually attacks a considerable number of men of our garrison, affected only thirteen in the months of October and November, 1838, and ten in those of August and September, 1839. I employed in some of these cases the albuminous method of treatment recommended by MM. Bodin de la

Pichonnerie and Mondière. Comparatively with those by the ordinary method (by antiphlogistics and opiates), the results obtained by it were really of the most striking kind. My own experience and that acquired by others in the hospital shows that when the disease is at all severe, it generally lasts two or three weeks, sometimes becomes chronic, or terminates in death: besides this the reiterated application of leeches causes severe pain, the administration of enemata is continually attended with much difficulty, the convalescence is prolonged, and attended with extreme weakness when the disease is not stopped during the first week. Judging from the cases I have observed, the albuminous treatment is attended with precisely contrary effects—rapid cessation of the symptoms, quick convalescence, and recovery of strength, while the administration is attended neither with pain nor any sort of inconvenience.

[The albumen was exhibited both as a ptisan and in enemata, according to the formula of M. Mondière, and eight cases are related (some, however, with ludicrous brevity) in support of the above emphatic encomiums. In some of the cases there was undoubted inflammation of the large intestine; in all frequent bloody stools, tenesmus, &c.]

Gaz. Médicale de Paris. No. xlvii. *Nov.* 1839.

On the Employment of the Oil of Cod Fish in Scrofulous Diseases. By M. TAUFFLIED.

[THE beneficial influence of the oil of cod fish in certain forms of scrofulous disease, has within the last few years been dwelt on by various German writers; M. Taufflied reports eight cases from which he draws the following inferences confirmative of their accounts:] 1. The oil of cod fish exercises a favorable influence on the general state of lymphatic subjects. 2. If administered with proper care it possesses the property of curing scrofula of the bones, tabes mesenterica, and scrofulous or rheumatismal chronic arthritis. 3. Caries accompanied with solution of continuity and engorgement of the soft parts requires local treatment in addition to the oil administered internally. Under these circumstances compression and alcoholic ioduretted fomentations may be successfully employed. 4. The oil of cod fish has no efficacy in cases of gouty arthritis, nor does it exercise any influence on the engorgement of any lymphatic glands except the abdominal. It seems to have little or no effect on scrofulous phthisis, if this be at all advanced. 5. The oil should be administered perseveringly and for several months in order to secure advantageous results.

Gaz. Médicale de Paris. No. xlv. *Nov.* 1839.

SURGERY.

On the Treatment of Malignant Ulcers by Cauterization with Corrosive Sublimate. By M. ORDINAIRE.

M. ORDINAIRE for the last fifteen years has been in the habit of employing the bichloride of mercury as a caustic in the treatment of cancerous, scrofulous, venereal, and other intractable ulcers, and has never observed the least symptom of absorption, which he ascribes to the instantaneousness of its destructive action on the absorbents, and to its being decomposed by the albumen of the tissues. The quantity of the powder employed varies with the thickness and the nature of the parts requiring to be destroyed, but never exceeds seven or eight grains. If the cancerous ulceration be superficial, a single application of caustic suffices; but it is generally necessary to repeat it after the separation of the eschar. He also employs this salt for the cauterization of strictures of the urethra. [The following, the third case related, is a specimen of the activity of this surgeon's practice:] M. I., innkeeper, subject from infancy to prolapsus of the gut, began at ætat twenty to experience much difficulty in reducing it after defecation,

which increasing till his thirty-second year, reduction then became impossible. M. O. on examining the patient at this period, found at the orifice of the rectum a tumour as large as an egg, traversed by several deep sulci, dividing the mass into ulcerated lobes. The tumour was hardly tender to the touch, yet caused severe lancinating pain; it bled when handled, and discharged a very fœtid ichorous matter. Recognizing cancerous degeneration, the author removed with a bistoury the mass protruding beyond the anus, and having ascertained that diseased excrescences extended two inches up the rectum, determined on removing them with the bichloride. He prepared a cylindrical plug of shredded lint of the thickness of the finger and four or five inches long, smeared it with a glutinous matter, then sprinkled it all over with the caustic, and pushed it two inches and a half up the rectum. Severe pain soon came on, and there was great difficulty in persuading the patient to bear the plug for a few hours. Sloughs as large as an almond separated on the fourth day with the aid of injections and hip-baths, and further application of the caustic was only required to two spots at the margin of the anus, which had a suspicious appearance. The patient was entirely cured thirty-two days after the first cauterization; the sphincters performed their office well. The operation was performed twelve years since, and the patient enjoys perfect health.

Gaz. Médicale de Paris. No. xlvi. 1839.

On the Treatment of Synovial Tumours by Subcutaneous Incision.
By M. Barthelemy.

This mode of treatment was suggested by the very slight local or general disturbance which has been found to succeed the sections of muscles even of considerable extent, when the skin over them is not divided. The operation consists in raising a fold of the skin near the tumour, and passing a long thin lancet-shaped knife under it to the left side of the swelling, which is then by a sweep of the instrument split into two portions. The instrument is then withdrawn by the same narrow aperture at which it entered, and care is taken that no air should pass along the track of the wound. The fluid which the cyst contains diffuses itself into the surrounding tissue and soon disappears, leaving it is presumed no chance of the relapses which are common after every other mode of treatment.

The operation has been performed with success and with no subsequent ill effect by MM. Barthelemy, Marechal, and Malgaigne.

Gazette Médicale. Dec. 7, 1839.

On Division of the Muscles of the Back in Cases of Lateral Deviation of the Spine.
By M. Bouvier.

They who maintain that contraction of the muscles plays the chief part in the greater number of lateral deviations of the spine, and that these deformities should in consequence be treated by tenotomy, are required to prove the affirmation of the three following positions: 1. That in the majority of lateral deviations of the spine, as in deformities really produced by muscular contraction, the muscles corresponding to the concavity of the curvature are rendered extremely tense, when an attempt is made to straighten the spine. 2. That the permanent retraction of these muscles is not preceded in the greater number of cases by deformity of the spine. 3. That section or excision of these same muscles on the dead subject causes total or partial disappearance of the curvature. [M. Bouvier decides each of these points in the negative; and hence concludes that curvatures of the spine are not in the majority of cases capable of cure by the plan recently advised and practised by a rival orthopœdist of the French capital. The point at issue can, however, only be fairly settled by the results of the operation on the living subject.]

L'Expérience, No. cviii. *Juillet,* 1839.

Cure of an Old Dislocation of the Humerus by Division of the Pectoralis Major, Latissimus Dorsi, Teres Major, and Teres Minor Muscles. By Professor DIEFFENBACH.

HERR TH., a large landowner, upwards of thirty years old, had his right shoulder dislocated two years ago by a fall from his horse; the nature of the accident was not at first recognized, and afterwards, though all the usual means were adopted by several surgeons, the bone could not be returned to its place. The patient, therefore, came to Berlin; he was of a gaunt powerful form, with a pale complexion and but little fat, and his muscles were strong and prominent under the skin. The injured right shoulder was an inch higher than the left; the acromion formed a sharp angle; on the outer side the shoulder was deeply hollowed, and the scapula lay flat. The right arm was thinner than the left, and stood out far from the body. The head of the humerus lay on the anterior side of the chest, close to the clavicle, and two inches from the upper portion of the sternum. The patient had a constant sensation of cold in the limb, and the creeping which he had formerly felt had ceased. The pulse in the right radial artery was rather weaker than that in the left. The limb was useless, and only the hand could perform some slight actions.

By moving the arm in different directions, severe pain was produced in the part where the head lay surrounded by a thick wall of dense ligament into which it had worked itself. In drawing the arm outwards from the body, the pectoralis major, latissimus dorsi, teres major, and teres minor became tense with extreme pain. The last three of these muscles felt hard and tense, even when the arm was not drawn outwards. An attempt to reduce such a dislocation without dividing these muscles and the new joint would have been extremely dangerous, and had been found impossible; but (says the Professor) I anticipated success from the subcutaneous division of everything that resisted me.

The patient being placed on the table, with one folded sheet passed under the right axilla, and held by six assistants, another fastened round the right hand and held by six more, and a third round the upper part of the humerus held by three more (in the manner usually adopted by me in old luxations), the two first sets of assistants were ordered to pull against each other. I next bade them make a slowly increased extension, and then stop; I then passed a small scythe-shaped knife through the skin, and divided the most tense portion of the pectoralis major close to its tendon, which yielded with a cracking sound. I then again introduced the knife at the posterior border of the axilla, and divided one after the other the latissimus dorsi, the teres major, and the teres minor. All these muscles gave way with a cracking noise, which was increased by the resonance of the chest. I next passed my knife into three places by the head of the humerus, and divided in a similar manner under the skin the dense and hard false ligaments which surrounded the new joint, and lessening the extension, I loosened the head by a few rotations.

A powerful extension was now again commenced on both sides, and the three assistants behind the patient pulled suddenly while I conducted the humerus towards the joint into which it slipped on a sudden, without again springing out. One shoulder looked now just like the other. The thorax, the shoulder, and the arm were enveloped with bandages which were soaked with paste, and after a few hours they all became dry and hard, and prevented any motion of the right side.

The bleeding from the wounds, which were not larger than those made in phlebotomy, was at most a few drops. No unpleasant symptoms ensued, and the patient suffered even less than the majority of persons in whom I have reduced old dislocations. On the ninth day I took off the bandage; both shoulders had exactly the same level and form, and there was neither swelling nor pain. The punctures in the axilla had completely healed, and scarcely a trace of them could be found; there was no collection of blood or pus. The arm was already capable of motion, and its actions were far less hindered than they are sometimes after the reduction of recent dislocations; because in them there is often for a long time a sensitive contraction of the unnaturally stretched mus-

cles, while in this case the division of the resisting muscles and of the newly-formed joint not only rendered the reduction possible, but at the same time diminished its after consequences. The limb is now again restored to perfect utility.

The Professor adds that he had lately occasion to reduce a luxation of the foot backwards of upwards of a year's standing, by dividing the tendo achillis, which forcibly drew the heel upwards. This limb also became useful again.

Medicinische Zeitung. Dec. 18, 1839.

Case of Dislocation of the Elbow, in which the Radius and Ulna were thrown forwards, with Fracture of the Ulna. By M. Richet.

A man fell from a high scaffold, as it was supposed, on his back and the palm of the left hand, and was admitted into the Hôpital St. Louis, in a state of partial collapse.

On examining the bend of the elbow, a hard oblong tumour was seen a finger's breadth above the condyles of the humerus, elevating the biceps and brachialis internus, and rendering the brachial artery comparatively superficial. This tumour was moveable on bending and extending the fore-arm, and rolled under the finger towards the radial side during supination and extension, giving rise to an indistinct crepitation. The olecranon was prominent, moveable transversely, but maintained its natural position. On tracing the edge of the ulna downwards, a flesh-wound was seen two fingers' breadth below the olecranon, through which the bone protruded. The condyles of the humerus, though preserving their natural relations with the posterior part of the ulna, projected considerably, so as to stretch the skin.

The dislocation being immediately recognized as that of the radius and ulna forwards, with fracture of the ulna, was easily reduced by extending the fore-arm, and then suddenly and forcibly bending it, at the same time forcing the upper extremities of the radius and ulna downwards and backwards. All deformity quickly vanished, the bones having regained their natural position, when, in consequence of extension having been remitted, an involuntary contraction reproduced the dislocation with the utmost ease. It was again reduced, and kept so, by means of a cushion, and bandage surrounding both fore-arm and arm—the fore-arm being bent at a right angle with the arm, and the cushion filling up the angle and keeping the bones in place—at the same time, a splint was applied along the inner side of the joint. The patient died three hours after, from the other injuries received.

On examination, it was found that the triceps was firmly attached to the whole of the posterior fragment of the ulna, as were also the extensor carpi ulnaris and anconæus, thus accounting for the natural position of the olecranon. A longitudinal fracture was seen, somewhat oblique, from above downwards, and from before backwards, running along the middle of the sigmoid cavity to the external border of the ulna, and thence backwards and inwards to a point a finger's breadth below the olecranon, which was the part that had protruded.

The ulna was thus divided into two fragments, the larger formed by the body, surmounted by the coronoid process; the smaller and posterior by the olecranon, and an inch and half of the ulna below it, ending in a point, which constituted the protrusion. The radius and ulna were seen on the front of the humerus, half an inch above its condyles. The capsular ligament was torn throughout, which explained the easy reduction and reproduction of the luxation.

[This case is worthy of remark, since it proves the possibility of a dislocation of the elbow forwards *without* fracture of the olecranon, contrary to the assertion of Sir A. Cooper and others. Boyer doubted the fact of its occurrence, even complicated with fracture of the olecranon, though he could conceive the possibility of it. Neither Petit nor Desault ever met with the case; and the only author who records such a form of dislocation as M. Richet has described, is Delpech, in his "Clinique Chirurgicale de Montpellier."]

Archives Générales de Médecine. Dec., 1839.

Memoir on the Ophthalmia reigning in Belgium. By M. CAFFE.

THIS disease appeared in Belgium in 1814, but it is chiefly since 1830 that it has raged with such intensity as to have attacked an eighth of the soldiers, and in some regiments even the half. It has affected more than 100,000 individuals, and has deprived a considerable number of their vision, and left them chargeable to the state. A commission of Belgian practitioners, with the assistance of ophthalmic surgeons from Berlin and Vienna, failed in their endeavour to check the progress of this scourge, and in 1838 there were still 5000 ophthalmic patients in an army of about 50,000 men.

In its symptoms this ophthalmia does not appear to differ materially from those with which English readers are familiar in the works of Veitch and other writers on the Egyptian epidemic; the more important part of M. Caffe's memoir is contained in his investigations into its origin and mode of extension. Reviewing the various causes to which its raging in the army has been ascribed, the author shows that it cannot be attributed to the introduction between the eyelids of the pipe-clay used in cleaning the belts and other white leather accoutrements of the soldiers, or of the powder (*tripoli*) with which their buttons and copper ornaments are polished, because the ophthalmia has affected regiments which never use those substances, and has not shown itself among other European troops who employ them in large quantity. He proves also that it is not due to the diet of the soldier, the abuse of spirituous drinks, the too frequent cutting of the hair, fatigue, sudden suppression of perspiration, melancholy, chlorine-fumigations used in the treatment of itch or syphilis, nor to the unhealthiness of the lodgings, to which Jungken had too hastily assigned it. He refutes especially the idea of the *compressionists*, who, with M. Vleminckx, the inspector-general, hold that the ophthalmia is produced by the pressure of the veins of the neck by a hard stock and the collar of the coat, and of those of the forehead by an inelastic heavy cap. The inefficiency of these causes M. C. shows by the facts that the French uniform before 1830, though in these respects similar to the Belgian, never produced an epidemic ophthalmia, that even in Belgium certain regiments are unaffected though dressed like the others, that the recruits who have not yet suffered from compression are often attacked on their first joining a corps in which the disease is raging, and that a complete change in the soldiers' dress has not been followed by any decrease in the disease.

M. Caffe believes that the extension of the disease depends on its contagious nature, by which it is capable of communication either by direct inoculation, by the application of the matter secreted by the diseased eye on the fingers, the linen or fluids impregnated with it, to the previously healthy eyes, or by mediate contagion or infection of the air by miasmata arising from the evaporation of the secreted fluids. Its transmission by the immediate contact of the secretion of the inflamed eye is generally admitted. The inoculation of the ophthalmia has not only succeeded on dogs, cats, and guinea-pigs, but the same result has been obtained in man, by placing the virus on an opaque cornea, or when some one with too firm a faith in non-contagion has submitted healthy eyes to the same experiment. Instances of accidental inoculation are abundant in the records of the Belgian physicians, and some of the most remarkable are cited. M. Fallot states that on the 25th of January, 1834, there was not a single case of ophthalmia in the garrison of Namur; on that day two came into the hospital, and from that time the disease spread; two nurses were attacked and each lost an eye. A soldier went home with an ophthalmia from the hospital at Brussels. His father, previously well, was affected a few days after with a violent conjunctivitis, and his mother and five brothers and sisters all in succession contracted the same disease. Another soldier went home with ophthalmia from the same hospital, and immediately after his arrival his sister was attacked. In a third similar case, the soldier's mother, brother, and sister were affected, but his father and elder sister escaped. In a fourth, the father, mother, and five brothers and

sisters were all attacked. These and many other equally well-marked examples seem to leave no doubt of the extension of the disease by direct contagion.

The miasmatic or indirect contagion is less evident; but if, says M. C., one considers the influence which an atmosphere charged with exhalations from the purulent matter must have on the conjunctiva, as well as the rapid propagation and speedy aggravation of the disease in ill-ventilated places, whatever precautions are taken against direct contagion, one will be disposed to attribute at least some share in the production of the epidemic to miasmatic infection.

The means which M. C. recommends to put a stop to the progress of the disease are such as naturally arise from the opinion which he has formed, and, as it would seem, justly, that this ophthalmia, though probably originating in and often aggravated by peculiar local circumstances, spreads from one individual to another by direct or indirect contagion. They are chiefly directed to the careful exclusion of all in whom the disease or a suspicion of it exists, from association with their comrades.

Bulletin de l'Acad. Roy. de Médecine. Janv. 15, 1840.

On the Cure of Congenital Squinting by Division of the Internal Straight Muscle of the Eye. By Professor Dieffenbach, of Berlin.

[The following cases are masterpieces from the hand of a master. The operation is beautiful in its simplicity, and the result delightful to contemplate. Who shall set bounds to the progress of surgery? These operations are the first of the kind ever performed on the living subject; but Stromeyer has the merit of having suggested the operation, and he performed it on the dead body, with a direct view to proving its practicability on the living.]

Case i. The subject of this operation was a child seven years old, whose eye was drawn far into the inner angle of the eyelids, so as to produce considerable disfigurement. The operation was performed in the following manner: The head of the child was held against the chest of one assistant, while another with two hooks kept the eyelids widely apart. The operator then passed a third hook, which he gave to a third assistant to hold, through the conjunctiva, and to some depth in the subjacent cellular tissue at the internal canthus. He next fixed a fine double hook in the sclerotica at the inner angle, and taking it in his left hand drew the eye outwards. Then cutting into the conjunctiva close to the ball where it is continued from it to the internal canthus, and penetrating more deeply by separating the cellular tissue by the side of the sclerotica, he divided the internal rectus muscle close to its insertion with a fine pair of scissors. The eye was immediately drawn outwards by the external rectus, as if it had received an electric shock; and in another instant became straight, so that there was no difference perceptible between its direction and that of the other eye.

The hemorrhage during the operation was but slight though sufficient to impede it. The after-treatment consisted of cold lotions; no inflammation ensued, and within eight days the cure was completed.

Case ii. Carl Gerhard, æt. ten, affected with squint since his fourth year. His parents wishing him to become a printer, were anxious to have this defect removed as it interfered with composing. The right eye was so completely drawn into the inner angle that on a first view, the point of junction of the iris and sclerotica formed the centre of the anterior surface of the eyeball. By an effort the eye could be drawn from the canthus and placed straight, but could not be turned at all outwards. The operation was performed as in the last case, the conjunctiva being cut through, and the sclerotica laid bare to the extent of four lines, in order to bring the muscle into view, which was cut with a curved scissors as before. The squint was gone; the eyeball when at rest stood nearly straight, or rather a little turned outwards; and could be turned more readily by the patient's efforts in this direction than inwards. All the other movements of the eye were free. The bleeding was here much less than in the former case

and caused no interruption. The sudden turning of the eyeball outwards, observed in the first case, did not take place here.

The boy felt quite well on the following day. He could separate the eyelids without difficulty. The conjunctiva in the inner angle of the eye was red. The eye was nearly straight, only turned a little more outwards than the other. In eight days the cure was complete and the eye quite straight.

CASE III. Albert Victor, æt. fifteen, affected with strabismus of the left eye since his earliest infancy. The eyeball was turned deeply into the inner angle, but by an effort of the will it could be turned straight, but on this effort being relaxed, it instantly returned to the former position. The operation was performed precisely in the same manner, it being only here specified that the external incision in the conjunctiva was semilunar, and that the muscle was cut by introducing the pointed blade of the scissors beneath it. As soon as the hook that held the eye was removed, the ball turned at first outwards, but in a moment returned to the straight position. The edges of the wound did not gape, so that the external incision was barely perceptible. The eye was covered with a cold poultice and the patient subjected to the antiphlogistic regimen. In eight days the cure was complete and the squint entirely gone.

Medicinische Zeitung. Nov. 13, 1839., and *Feb.* 5 and 12, 1840.

Cases in Autoplastic Surgery—Formation of New Eyelids. By Dr. E. LABORIE.

THIS paper contains five successful cases of reparation of partially or completely removed eyelids, which have been observed by the author under the care of M. Jobert and M. Blandin.

In the first, of which the subject was a man æt. fifty-one, with a malignant tumour of the lower eyelid, M. Jobert removed it all from the external canthus to within two or three lines of the punctum lacrymalé, exposing and removing some of the fibres of the orbicularis muscle. He then cut from the upper part of the cheek a flap of skin of an elongated triangular form, with its base slightly rounded and directed externally, and of which the truncated summit which was to serve as a pedicle was situated a little below the outer angle of the eye. This flap, which when it was dissected up was an inch and a half long, and half an inch wide at its outer part, was now twisted upon its pedicle and applied in the surface of the wound made by the removal of the part of the eyelid. It exactly fitted, and was fixed by three sutures. The wound on the cheek was also united by suture, and a simple slightly compressing dressing was put over the whole.

During the first few days after the operation, the wounds and the flap of skin swelled very much, but after this had ceased the adhesion was found complete. Notwithstanding two severe attacks of erysipelas, the reparation was in the end more perfect than could have been imagined; the punctum lacrymale having been preserved, there was no running of tears over the cheek, and the fibres of the orbicularis palpebrarum that remained gave the new eyelid a certain degree of natural motion.

The subject of the second case was a scrofulous woman of twenty-three, who was also under the care of M. Jobert, with ectropion of the left upper eyelid, which from cauterization and other severe treatment was in a completely incurable state. An incision an inch and a half long was made horizontally along the affected eyelid. A flap of skin of an oval form, and of the same size as the previous incision, was then cut out from the cheek. Its pedicle was at the level of the upper and outer part of the malar bone, and it was inclosed between two vertical lateral incisions and an inferior incision which was slightly rounded. This portion being dissected up was twisted on its pedicle, applied on its under surface between the edges of the incision in the eyelid, and retained there by three sutures and a simple dressing.

The operation succeeded completely; the eye, which was before constantly exposed, was fairly covered by the new eyelid, which by the actions of what remained of the orbicularis muscle performed all the natural motions. A second operation of a somewhat similar kind was subsequently performed with the same success for the reparation of a loss of substance in the situation of the eyebrow. In this case a portion of the skin from the temple, which was in part covered with hair, was taken for the flap; the hairs continued to grow, and by cutting them to a proper length they formed an excellent substitute for the lost eyebrow.

The third case was one of ectropion of the lower eyelid from a burn, which was treated in the same manner as the second with the same favorable result.

The fourth was that of a young woman who from an acute inflammation and sloughing had lost the whole of the left upper eyelid except its free border, which contained the eyelashes, the cartilage, and the meibomian glands, from which a sticky fluid constantly running had fixed this remnant of the upper eyelid to the edge of the lower. There was acute inflammation of the exposed surface of the eye.

M. Blandin cut a flap of skin from the forehead, of an oval form with its pedicle at the root of the nose. Then having cut off the edges of the ulcerated aperture, he turned down the flap over it, and fastened it to them by sutures. Notwithstanding a very severe attack of erysipelas, the flap adhered perfectly at all parts. The new eyelid had lashes, and even a slight power of motion, though the orbicularis had been entirely destroyed; for a portion of the frontalis muscle turned down with the flap of skin preserved its contractility, and by the transverse position in which its fibres were now placed, it enabled the eyelid to be slightly raised.

The last case was one of ectropion of the lower eyelid, which was treated like the second and third, by fitting a flap of skin into the space between the edges of an incision made along the everted part. It was successful, and the eye obtained almost complete protection from the new eyelid.

Gazette Médicale. Janvier 13, 1840.

MIDWIFERY.

On the Influence of Digitalis on the Contractions of the Uterus. By M. Piedagnel.

The object of this paper is to recommend, from theory and from a limited practice, the administration of digitalis in false labour pains. The cases in which it has been found most useful are those in which during gestation there are vague pains in the uterus producing considerable suffering and fatigue, and those in which after delivery the pains continue for an unusually long period, for example, for more than two or three hours after the expulsion of the placenta. In these and in certain other cases in which it has been a common custom to administer opium, the author suggests that digitalis should be employed, which he believes acts by diminishing the force and frequency of the contractions of the uterus, as it is well known to do those of the heart. He has generally used an infusion of one fresh leaf or of two dried leaves in a cup of water, which is taken at once, or a drink, containing from thirty to sixty centigrammes of powdered leaves, of which the patient takes a spoonful every half hour or hour till the pains cease.

Bulletin Général de Thérapeutique. Janvier, 1840.

II. THE AMERICAN AND COLONIAL JOURNALS.

ANATOMY AND PHYSIOLOGY.

Report of Experiments on the Action of the Heart. By C. W. PENNOCK, M.D., Physician to the Philadelphia Hospital, and E. M. MOORE, M.D, late Resident Physician to the Frankford Asylum.

[These experiments, fifteen in number, were performed on sheep, calves, and one horse; we have only room for the conclusions deduced from them by the experimenters.]

1st. THE impulse is synchronous with and caused by the ventricular contraction, and when felt externally, arises from the striking of the apex of the heart against the thorax.

2d. The expulsion of the blood from the ventricles is effected by an approximation of the sides of the heart only, and not by a contraction of the apex towards the base; during the systole the heart performs a spiral movement, and becomes elongated. (Experiments 6th, 10th, and 11th.)

3d. The ventricle contracts and the auricle dilates at the same time, occupying about one half of the whole time required for contraction, diastole, and repose. Immediately at the termination of the systole of the ventricle, its diastole succeeds, occupying about one fourth of the whole time, synchronous with which the auricle diminishes, by emptying a portion of its blood in the ventricle, unaccompanied with muscular contraction. The remaining fourth is devoted to the repose of the ventricles, near the termination of which the auricle contracts actively, with a short, quick motion, thus distending the ventricles with an additional quantity of blood: this motion is propagated immediately to the ventricles, and their systole takes place, rendering their contractions almost continuous. (Experiments 15 and 16.)

4th. From the termination of their diastole to the commencement of their systole, the ventricles are in a state of perfect repose, their cavities remaining full, but not distended, while those of the auricles are partially so, during the whole time.

5th. The sounds are produced by the motions of the heart or its contents, and not by striking against the thorax, as proved in all the experiments; being much louder when the stethoscope was applied directly to the heart than when to the chest, or with the lungs interposed.

6th. The sounds are more distinct when the muscle is thin and contracts quickly. Hence the clear flapping character of the first sound over the right ventricle, as compared with the left.

7th. The first sound, the impulse, and the ventricular systole, are synchronous. This sound may be a combination of that caused by the contraction of the auricles, the flapping of the auriculo-ventricular valves, the rush of blood from the ventricles, and the sound of muscular contraction. From experiments 3d, 4th, 6th, and 10th, when the heart was removed from the body, the ventricles cut open and emptied of their contents, the auriculo-ventricular valves elevated, and a sound, resembling the first, still heard, it may be chiefly attributed to the muscular contraction. That these valves aid but slightly in its production may also be inferred from experiment 16.

8th. The second sound is caused exclusively by the closure of the semi-lunar valves, from the tendency to regurgitate of the arterial column of blood upon

them. This is proved by the greater intensity of this sound over the aorta than elsewhere, the blood having a strong tendency to return through the valvular opening; by the greater feebleness of the sound over the pulmonary artery, which is short, and soon distributes its blood through the lungs, thus producing but slight impulse upon the valves in the attempt to regurgitate; by the disappearance of the sound, when the heart becomes congested and contracts feebly; and, finally, on account of its entire extinction when the valve of the aorta was elevated.

9th. The second sound is synchronous with the diastole of the ventricle.

From these experiments it will be seen that our conclusions coincide very nearly with those of the British physiologists—the correctness of whose results, when compared with those of the French, may be mainly attributed to the use of larger animals. From our observations, calves, of from four to eight weeks old, are decidedly preferable to other quadrupeds for these investigations. The tenacity of life of calves of this age is greater than in older animals, whilst the cardiac pulsations are slower and more forcible than they are in the younger. The heart of this animal, too, is of large size, and the introduction of hooks for the elevation of the valves is readily effected.

Remarks [by the Editors of the Medical Examiner.] It will be perceived by a reference to the experiments, that besides confirming several points already set forth by the British experimenters, the experiments of Dr. Pennock have developed some new facts. These are the existence of an auricular sound, faint indeed, and to a great degree merged in that of the ventricles, but still real. Indeed, when the fact of real muscular contraction of the auricles is proved, we know from analogical reasoning that sound, which is the necessary consequence of muscular contraction, must be produced. The active muscular contraction of the auricles has been doubted by some physiologists, we hardly know why, for the evident muscular fibres of the auricles are certainly designed for no other purpose than other muscles, that is, contraction. The experiments of Drs. Pennock and Moore have set this question also at rest, and proved satisfactorily to all present, that the muscular fibres of the auricles were in action during their systole.

There are two other points ascertained positively by these observers, from direct experiment, which we believe were not noted in previous observations of the kind. These are—1st, that the ventricular sound on the right is much sharper and clearer than on the left, which is more dull and prolonged; 2d, that the second or valvular sound ceases by the congestion of the ventricles, ceasing first on the right side. With both of these facts we have long been familiar, from our observations of the sounds of the heart in the living body, both in the healthy and diseased condition of this organ; and it is not a little gratifying to us to find that the clinical observations which we have often made and pointed out to our pupils have been abundantly confirmed by the sure test of direct experiment. This observation throws light upon a large number of pathological facts, which are often with difficulty understood without knowing the effect of congestion upon the heart.

The last point of interest which has been for the first time explicitly stated, is the less degree of loudness of the second sound over the pulmonary artery and valves than over the aorta. This probably arises from the pulmonary valves being so much weaker and thinner.

American Medical Examiner. Nov. 2, 1839.

On the Sedative Effects of Ergot (Experiments with the Powder). By J. B. COTTMAN, M.D., Resident Physician of the Philadelphia Hospital.

THE subjects of these experiments were strong healthy men, but lunatics. A diminution in the frequency of the pulse, and, in several, of the volume, were the only phenomena observed, except in three cases; in these it produced slight nausea, but no vomiting; in two others, to whom it was given the day before, violent emesis was brought on in the course of one hour. When administered in half-dram doses, it either produced nausea—and consequently sedation—or diminished the frequency of the pulse, without nauseating, in the course of twenty-five or thirty minutes; but, when given in scruple doses, it appeared to excite rather than diminish the pulse in that time; in fifteen or twenty minutes, however, the pulse lost a few beats in the minute, but not to the same extent as when given in larger doses: in several cases it produced little or no effect when given in the smaller doses. No other cause operated in producing this result, unless we presume that the fact of giving the men medicine when they were in health excited them; and as I examined the pulse in every case just before giving the medicine, and forty or forty-five minutes afterwards, the excitement might have passed off, and, consequently, the pulse be diminished in frequency; this, indeed, is not unlikely, as the pulse in several cases was much above the natural standard.

It is necessary, I need scarcely say, to be very cautious in drawing conclusions with regard to the medical properties of any drug from a limited number of experiments, but I think we may safely infer, that the ergot has a power of depressing the pulse when given in large doses, but may question very much its sedative agency in small doses. The following is a list of the cases. The age of each individual is affixed to his name:

Names.	Age.	Pulse at time of administration.	Pulse forty-five minutes afterwards.
Daley .	62	72	60
Young .	20	80	64
Dougherty	30	88	70
Hews .	20	70	60
Daughney	45	104	80
M'Cally .	35	84	68
Buck .	35	80	66
M'Carit .	30	60	50
Thompson	50	84	72
Stewart .	40	76	70
Campbell	29	68	60
M'Donald	22	120	106
Daley .	62	68	60
O'Brien .	33	76	60
Cases in which Scruple Doses were given.			
Bush .	60	84	78
Leonard .	31	92	86
Leaman .	19	88	84
Caryer .	30	72	70
Nealy .	40	80	76
Campbell	30	64	58
Thompson	40	92	88
Fagan .	30	84	80
M'Nice .	40	68	62
Stomeyer	30	80	78

American Medical Intelligencer. No. xi

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Case illustrative of the Etiology of Spontaneous Amputation of the Limbs of the Fœtus in Utero. By A. H. Buchanan, m.d., of Columbia, Tennessee.

In the month of February last I was called in haste to the country, about three miles, to see a negro woman who was said to be suffering from severe pain in the back, and uterine hemorrhage. She was the mother of ten children, aged about forty years, and had miscarried three or four times. On my arrival, I found she had aborted, and that the uterine hemorrhage had ceased. Upon an examination of the fœtus, which was between three and four months old, and perfectly formed, except a considerable flattening of the head laterally, I found the umbilical cord twisted about the thigh and neck in the following manner: the cord passes from the umbilicus under the right thigh, just above the knee-joint, and continuing completely around it, passes under itself, and ascends in front of the chest to the right side of the neck, around which it twines twice, or rather twice and a half, so that two coils are seen in front of the neck and three behind; it then passes in front of the left shoulder to the placenta. From the compressed appearance of the cord opposite the left shoulder I think it passed under the left armpit to the placenta. Thus circumstanced, it is evident that any efforts made by the child to extend the thigh tightened the cord about the neck and also about the thigh, as well as dragged upon its umbilical extremity, and obstructed the circulation. The same effects also are produced by extending back the head; but in this last action, the placental extremity of the cord is immediately pulled upon. It is very fair to conclude that the fœtus thus situated came to its death either from the compression of its throat by the cord or from its obstructed circulation, or from both; and that the abortion was a consequence of its death. At many points where the cord twists upon itself it is very much compressed, or rather atrophied. But the object of communicating this case is to call attention to the effects produced upon the thigh by the twisting of the cord around it. It may be seen by any present, that at the point of compression only the integuments intervene between the cord and bone, all the other parts having disappeared; but the limb below the ligature appears as fully developed as its fellow, and the integuments immediately under the ligature appear sound. Now it is highly probable, had the child lived to its full time, the leg would have been amputated by the process of absorption carried on in consequence of the pressure of the cord around the limb, and that the opposite surfaces would have healed, as is usual in such cases, during the process of amputation; the limb below the ligature retaining its vitality by its connexion with the integuments, they being the last parts to give way during the amputation; that the leg below the knee-joint would have been more or less atrophied before its complete separation is almost certain.

American Journal of Med. Sciences. No. xlviii.

On the Causes of the Acute Diseases of European Soldiers in India. By J. Mouat, m.d., Surgeon of H. M. 13th Light Dragoons.

[The following Table constitutes only a small part of a most elaborate and valuable report on the health of the 13th Light Dragoons, published in the first number of the new "Madras Quarterly Medical Journal," which has not yet reached us. Our readers will perhaps be surprised at the very striking part played by *cold* in this tropical etiology:]

Table of the Causes or assigned Causes of Acute Disease in the 13th Light Dragoons, from 1831 to 1838, inclusive.

Who impute their disease to cold caught on line guard and picquet, by sleeping under the trees or in the open air, or in the sick lines and exposed to a current of air	259
Who impute their disease to cold caught on their posts whilst sentry at the sick lines, hospital, paymasters, and the barrack guards, or by going round with the relief	95

Who impute their disease to cold caught on barrack, mess-house, paymasters and choultry guards, by sleeping in the verandas or open air, and exposed to a current of air	125
Who impute their disease to cold caught by sleeping opposite windows or doors in the barracks that were left open	287
Who impute their disease to cold caught by being out with the band of the regiment at balls	4
Who impute their disease to cold, but are unable to particularize how .	261
Who impute their disease to cold caught by getting wet in the rain when on barrack guard, line guard, parades, sentry, shoeing horses, at lines, field exercise, whilst going to the privy, bazar, or by getting the soles of their feet wet .	197
Who impute their disease to cold caught by the changes of the weather .	43
Who impute their disease to cold caught by neglecting to put on their flannel jackets	6
Who impute their disease to cold caught by drinking cold water or buttermilk, or by bathing in cold water when heated	55
Who impute their disease to cold caught by sleeping on the floors of the barracks on account of being annoyed by bugs	2
Who impute their disease to cold caught by sleeping in the open air .	4
Who impute their disease to cold caught by sleeping uncovered in the barracks	16
Who impute their disease to cold caught on the march from Poonamallee .	5
Who impute their disease to cold caught during their confinement in the barrack guard and cells	11
Who impute their disease to cold caught by being a long time in water to get out a boat	2
Who impute their disease to exposure in the sun whilst on barrack guard, line guard, shoeing horses, sentry, on duty superintending horsekeepers at work, piling up hay, going to and returning from church, &c. . .	62
Who impute their disease to diet, as having eat mulligatawny and rice, country greens, cold meat, boiled and mashed potatoes, fat bacon, milk and bread, eggs, curry and rice, jackfruit, oranges, cucumbers, drinking hot tea, milk, and cold water	26
Who impute their disease to jerks or falls from their horses whilst at riding school, going to water, field exercise, or out in watering order, to the tightness of their full dress, to costiveness, to rubbing in mercurial ointment on mangy horses, to violent exertions, &c. &c.	59
Whose diseases were caused by intemperance	105
Who know no cause for their disease but have been drinking . .	14
Who know no cause for their disease	1658
Diseases readmitted from the above from changing to other acute affections .	98
Grand Total .	3394

India Journal. No. xlii.

III. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING FEBRUARY 29, 1840.)

ANATOMY AND PHYSIOLOGY.

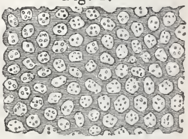
On the Structure of the Mucous Membrane of the Stomach. By R. B. TODD, M.D., F.R.S., Professor of Physiology, King's College, London.

[We regret much that our limits will only allow us to extract the part of this excellent paper which relates to the anatomy; the whole is equally worthy of notice.]

WHEN a portion of the mucous membrane of a true digestive stomach is examined, stretched upon a plane surface under water, we observe it to exhibit

a multitude of small cells more or less circular in form. These cells are present over the whole surface, where a thick epithelium visible to the naked eye does not exist, and their presence may be considered to be characteristic of the true digestive surface, as contra-distinguished from that of a simple macerating cavity. When the mucus has been well cleared away, we can see to the floor of each cell, which exhibits from three to five perforations, often rendered distinct by being filled with the white mucous secretion (fig. 1). The cells are separated from each other by partition-like elevations of the membrane, which vary in depth, and sometimes even form pointed processes, mistaken by some anatomists for villi, which they really do resemble when examined on an oblique section. The diameter of the cells is about 1-180th inch to 1-250th inch: it varies, however, in the different regions, and is always largest near the pylorus. Such is the general description of the mucous surface of the stomach of all animals in which I have examined it—in man, the dog, cat, lion, the fourth stomach of ruminants, in the pig, rabbit, horse, and ass; in the simple stomachs of the frog and waternewt, in the stomach of the turtle, and in those of the skate and cod, in the former of which each cell measured 1-360th of an inch.

Fig. 1.



This figure (from Bischoff) represents the cells seen on the surface of the stomach, with their floors perforated by the orifices of the tubes.

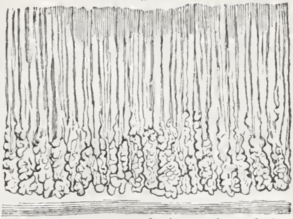
When the vessels of the stomach have been minutely injected with the size injection coloured red, nothing can be more beautiful than the vascular net-work which is then seen on the surface of the mucous membrane. The margin of each cell is surrounded by a vascular circle, which is joined at various points of its circumference by minute vessels emerging from the substance of the membrane (fig. 3), and all the circles anastomose with each other. I know nothing which more forcibly exhibits the intricacy of the capillary vessels themselves than this vascular net-work on the surface of the gastric mucous membrane.

Although the appearance which I have described is rendered visible by a lens of very low magnifying power, so low as three or four diameters, no trace of it can be seen by the naked eye. The orifices of the so-called gastric glands, which Sir Everard Home states may be seen at the pyloric and cardiac portions of the mucous membrane of the stomach of man and other animals, can correspond to nothing but the cells which I have described; yet it is difficult to imagine, if he really did see these cells at the cardia, how he could have avoided seeing them, similar in arrangement although different in size, all over the mucous surface. Not unfrequently a remarkable series of smaller wrinkles is seen on the pyloric and cardiac portions of the membrane. Three slight and very short fissures radiate from a central depression, and these occur in so great numbers, and at such regular distances from each other, that they are not unlikely to be mistaken for a peculiar structure, nor to be set down as glands, by those who are zealously in search of a distinct series of such organs in connexion with the stomach. I have seen this appearance many times on the human stomach, and always in that of the pig; and I am disposed to think that it is produced by the contraction of the muscular coat, although I am unable to explain exactly the manner in which it is effected.

When thin sections of the mucous membrane, cut vertically to the surface, are placed under the microscope, they are seen to be composed of a number of tubuli closely applied to each other side by side, their blind extremities being in contact with the submucous tissue, and their free extremities opening into the bottom of the cells. In some situations these tubuli are straight and short; in other parts they are longer, and at their blind extremities present an appearance which might arise either from a slight convolution of the tube, or from

some irregular dilatations of it in that situation (fig. 2). It very commonly happens that two tubuli coalesce or anastomose at their free extremities, and they will consequently open upon the floor of a cell by a common orifice; and hence it is that a greater number of tubules actually pour their contents into a cell, than would be indicated by the number of openings which pierce its floor. The diameter of the tubuli varies from 1-360th to 1-540th of an inch.

Fig. 2.



Tubes from the pyloric portion of the stomach, as seen by a vertical section.

The tubuli are straighter and shorter at the cardiac portion of the stomach, longer and more convoluted or irregular at their blind ends, at the pyloric portions. In a vertical section of an injected specimen, we see the vessels coming up from the submucous tissue, and passing between the tubuli, as in the annexed figure (fig. 3), to terminate in the vascular net-work of the surface.

Fig. 3.

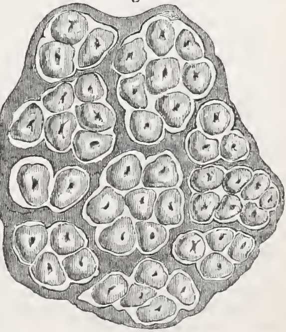
Vertical section of the mucous membrane, showing the vessels passing to the superficial network (from Bischoff).

From the examination of thin horizontal sections at various depths of the membrane, I have ascertained that the tubuli are arranged in bundles or groups, surrounded and bound together by a fine cellular membrane, the bundles varying in size, and in the number of tubules contained in them, as shown in fig. 4, which represents such a section of the dog's stomach, magnified about 100 diameters.

When a very thin horizontal section taken from the free surface of the membrane is placed under the microscope, the free surface being upwards, and covered with talc or thin glass, a very beautiful view of the mouths of the cells, and the arrangement of the membrane around and between them, is obtained. When viewed with an object glass of a quarter of an inch focus, the membrane surrounding each cell appears raised, so as to form around each cell a prominent circle from which the membrane inclines downwards into the cell; and if the section have gone below the level of the floors, we can see that the membrane is continued to the perforated floor of each cell.

From similar sections we are enabled to see very clearly the arrangement of the epithelium on the surface. I have already stated that the absence of a thick epithelium, visible to the naked eye, is characteristic of the true digestive stomach. An epithelium, however, nevertheless exists, of a very definite arrangement, which is distinctly brought into view by the use of high powers, of a quarter and eighth of an inch focus; and we are indebted to Henle for the first complete demonstration of the existence of an epithelium upon the whole mucous

Fig. 4.



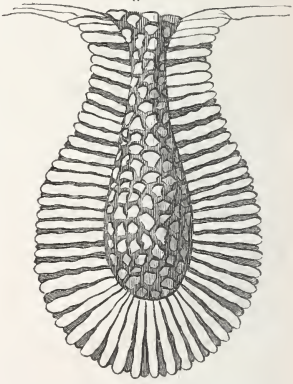
Transverse section of the tubuli in the dog.

tract from the mouth to the anus. My observation, however, does not confirm the statements of this anatomist with respect to the gastric mucous membrane: I have never seen the cylindrical form of epithelium in any part of the stomach. The whole surface of the membrane, on the contrary, appears to be, as it were, covered with a pavement of fine polygonal epithelium scales, which under the highest power present an appearance very similar to that of shagreen. The scales not only occupy the space between the cells, but pass over their margins, and are continued down to their floors. The diameter in the scales, in the dog, in the cardiac portion of the stomach, was from 1-3100th to 1-2325th of an inch. These scales resemble very much those of the deep layer of œsophageal epithelium, both in form and dimensions.

The matter contained in the tubuli appears to be of a very different nature from the scales of the epithelium: it is a soft, whitish substance, composed of minute granules, which exhibit no trace of structure even under the highest powers. This matter may be readily obtained by pressure from the tubes, in which it always exists in considerable quantity: it is in every respect the same as the layer of mucus which covers the membrane of the recent stomach.

The structure of the mucous membrane of the proventriculus or true digestive stomach of birds demands a separate description. Here, it will be recollected, the membrane presents a multitude of large follicles, which open into the cavity of the proventriculus. Each follicle may be considered as a little stomach in itself: when a simple follicle is laid open in its long diameter we observe a number of minute orifices on its internal surface, which are those of a series of tubuli arranged side by side, and vertically to its wall. The annexed figure (fig. 5) is a diagram representing a vertical section of one of these follicles in the pigeon: the tubes in this bird are all short and straight, and measure in diameter from 1-540th to 1-720th of an inch. The free surface of the membrane lining the cavity of the proventriculus is covered by a delicate epithelium; the scales of a less distinct character than usual, polygonal, but with one or two of the angles rounded off.

Fig. 5.



Vertical section of a simple follicle of the proventriculus in the pigeon.

The follicles of the proventriculus of the ostrich are of the most complex kind: into each follicle a series of smaller and simple follicles pour their secretions, so that one of these compound follicles may be said to represent on a small scale the proventriculus of the pigeon. Each of the smaller follicles has exactly the structure of the simple follicle of the pigeon. The compound follicle, then, consists of an aggregate of simple follicles placed side by side, and vertically to the walls of the larger ones, whilst each simple follicle consists of an aggregate of tubuli as before described. The epithelium is very distinct on the surface of the proventriculus, on which also there are numerous triangular processes not unlike villi.

From the preceding description of the structure of the mucous membrane of the digesting stomach in the vertebrata, we may not improperly designate this membrane as a gland; its constituent tubes being arranged perpendicularly to an extended surface, and pouring their secretions into a number of cells; not, as in other glands, into one or more canals or ducts.

London Medical Gazette. Dec. 13, 1839.

Observations and Experiments on the Mode in which various Poisonous Agents act on the Animal Body. By Mr. JAMES BLAKE.

THIS is an important contribution to the stock of our knowledge of toxicology, and merits the attention of both physiologists and pathologists. "Of the many opinions (says Mr. Blake) that have been entertained on the manner in which poisons produce their effects, there are only two which it is now necessary to notice, as they express the views of by far the greater number of physiologists of the present day. According to one of these opinions, before a poison can produce any general effects, it is essential that it should be mixed with the blood circulating over the body, and thus brought into contact with the nervous tissue, or, at least, that the poison should, in some manner, be strictly applied to the nervous centres. The other opinion is, that these poisons modify or destroy the functions of the nervous system, by an impression made on the nerves of the part to which they are directly applied, and which, being transmitted to the nervous centres, may destroy these functions independently of any contact of the substance with the nervous tissue generally." It is well known to our readers that the latter opinion has been supposed to derive, if not confirmation, at least strong support from the experiments of Mr. Morgan and Dr. Addison, performed twelve years since. In the present paper, Mr. Blake endeavours to show the inconclusiveness of these experiments, and to establish the reverse opinion:

"The principal physiological facts which have been supposed to support the opinion that poisons may produce their effects on the system, without being generally applied to those tissues, the function of which they appear to destroy, are derived from the instantaneous manner in which some poisons have been stated to act. The support derived from this fact, however, (Mr. Blake remarks,) has been founded on erroneous views which have been taken of the time required for the blood to circulate from one part of the system to another; and also from the statements of the instantaneous action of the more powerful poisons being deduced from incorrect observations." And, in order to ascertain this essential preliminary point, Mr. B. instituted several experiments here detailed, and which consisted in the introduction of certain substances into the circulating fluids, and tracing them from one part to another. These experiments led to the conclusion, that the time required for a substance to pass from any part of the vascular system back to the same part again (in dogs) varies from twelve to twenty seconds. Having thus obtained the time requisite for a substance to be circulated over the body, the experimenter proceeded to ascertain whether the poisonous substances alleged to act on the nervous system only came within the same category. The substances used for this purpose were hydrocyanic acid, woorara, nicotine, conia, and strychnia. The result is thus stated by the author: "These experiments, I think, furnish sufficient proof that the opinion of the instantaneous action of poisons must be founded on incorrect observations. An interval, never less than twelve seconds, has been shown to elapse between the application of a poison and the first symptom of its action; an interval in itself so short as might almost justify its being neglected, particularly in the present state of opinion of physiologists on the rapidity with which the organic processes are carried on; but which becomes of importance when compared with the time actually occupied for the performance of these processes, as it is quite sufficient for a poison to be brought into general contact with those tissues it affects. This being the case, it is unphilosophical to suppose that these effects on the nervous centres are owing to an impression communicated to them by the nerves, and not the result of the direct application of the poison to them." These conclusions are still further strengthened by experiments, which show "that the nearer to the nervous centres is the part of the vascular system into which the poison is introduced, the more rapid is its action."

Before concluding his paper, Mr. Blake briefly examines the experiments in-

stituted by Mr. Morgan and Dr. Addison, and then states the general conclusions derived from his own, as follows:

1. That the time required* by a substance to permeate the capillary vessels may be considered as inappreciable.

2. That the interval elapsing between the absorption of a substance by the capillaries and its general diffusion through the body may not exceed nine seconds.

3. That an interval always more than nine seconds elapses between the introduction of a poison into the capillaries or veins and the appearance of its first effects.

4. That, if a poison be introduced into a part of the vascular system nearer the brain, its effects are produced more rapidly.

5. That the contact of a poison with a large surface of the body is not sufficient to give rise to general symptoms, as long as its general diffusion through the body is prevented.

Edinburgh Journal. Jan. 1, 1840.

Observations on Corpora Lutea. By Robert Paterson, m.d., Physician to the Leith Dispensary.

[This is an elaborate anatomical history of the appearances in the ovaries which are termed *corpora lutea.* The present is only the first part of the essay, and contains—1st, The appearances presented by the true corpus luteum in its early stages, its anatomical position, and the time and mode of its disappearance. 2d, The different kinds of false corpora lutea. The much-debated question, whether or not the true corpus luteum ever occurs in the human subject, or any of the lower animals, without impregnation having taken place, is left for future consideration. The following are stated by Dr. Paterson to be the different sources whence *false* corpora lutea may arise, and the marks which distinguish them from the *true* :]

False corpora lutea may arise:

First, From the bursting and subsequent filling of a vesicle with blood, as in menstruation.

Second, From partial effusion of blood into a vesicle, either with or without rupture of it.

Third, By reabsorption of the fluid of a morbidly enlarged Graafian vesicle, giving rise to a puckered cyst.

Fourth, From effusion of blood into the tissue of the ovary, the apoplexy of that organ.

Fifth, Tubercular deposits.

Sixth, Cysts filled with yellow fatty matter.

These are to be distinguished from the true corpus luteum by the following marks:

They in general have an irregular form.

They want the central cavity lined with a distinct membrane, or the central puckered cicatrix.

They have no concentric radii.

They are frequently numerous in both ovaries.

Edinburgh Journal. Jan. 1, 1840.

* In regard to the statement as to the time required for substances to be diffused through the body, I would observe that all my experiments have been performed on full-grown dogs. It is probable that these observations might require to be slightly modified in applying them to other animals. It is also evident that a difference must exist in the time occupied by a substance in reaching the capillaries in different parts of the body. The time here given, or nine seconds, is that in which I conclude a substance may be applied over the greater part of the body, more particularly to the central parts of the nervous system. It would appear that seven seconds may suffice for its being conveyed to the capillary terminations of the coronary arteries. *Ed. Journal.*

Four Cases of Aneurism of the Arch of the Aorta, and a Case of Diaphragmatic Hernia. By John Reid, m.d., f.r.c.p.e., Lecturer on Physiology.

Dr. Reid informs us that, beside the cases here detailed, he has had occasion, during the last eighteen months, to inspect in the Royal Infirmary five other cases of fatal thoracic aneurism. Of these—in one, the aneurism was placed upon the middle part of the thoracic portion of the aorta, and burst into the œsophagus; a second was placed upon the lower part of the thoracic and upper part of the abdominal aorta, and burst into the left cavity of the pleura; in a third, the aneurism was placed upon the transverse portion of the arch of the aorta, had destroyed by its pressure the continuity of the left recurrent nerve, and produced sudden suffocation by its effects upon the movements of the muscles attached to the arytenoid cartilages of the larynx; in a fourth, the aneurism occupied the upper part of the descending portion of the arch, and proved fatal by its pressure upon the trachea and left recurrent nerve; in the fifth, the aneurism was placed upon the trunk of the arteria innominata at its origin, had acquired an immense size, extending upwards as high as the thyroid cartilage of the larynx, and produced absorption of the upper part of the sternum, and of the inner end of the right clavicle. The skin had become livid at two places over the surface of the tumour, the blood had even begun to ooze out, but he died before any external rupture had occurred.

The four cases given in full are, from the rarity of their termination, and the precise manner in which they are detailed, valuable contributions to the morbid anatomy of aneurism of the arch of the aorta. The first case terminated fatally *by opening into the right auricle;* the second, *by bursting into the pulmonary artery;* the third, *by communicating with the pulmonary artery*, in the position of the *ductus arteriosus*, but not, as Dr. Reid believes, from this duct continuing pervious; the fourth, *by bursting externally.*

Edinburgh Journal. Jan. 1, 1840.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Cases of Mortification of the Male Genitals, giving rise (as is supposed) to fatal Fever by Infection. By Robert Paley, m.d.

[The following narrative is very interesting and well worthy the attention of pathologists. The evidence of the infection is not unquestionable, but we are disposed to adopt Dr. Paley's views.]

The first case was that of a stout man, aged thirty, by profession a teacher of music, residing in a village near Halifax, and a married man of very regular habits; the surgeon who was in attendance informed me that he had seen him, for the first time, on the preceding day, and that the patient informed him that it was only on the previous day that he had first felt some uneasiness in the scrotum; that it very soon became enlarged and inflamed, in which state it was when the surgeon first saw it; he immediately applied a dozen leeches, and afterwards ordered fomentations and purgatives. On the following day I was consulted, being the third day from the commencement, when the whole of the scrotum and the penis had assumed a dark red colour, with here and there a black gangrenous spot; the tongue was dark coloured as in typhus, and the pulse indicated debility. We agreed to give our patient decoction of bark with porter, and to apply an ale poultice, with lint dipped in melted unguentum resinæ flavæ. This plan was continued, with the occasional addition of sp. terebinthinæ to the ointment, until the whole of the scrotum, prepuce, and a considerable part of the penis, sloughed off. The patient's strength was supported, and in the course of a few weeks he recovered so far as to live for seventeen or eighteen years, when he died, I believe, of phthisis. His death took place some years after I had left that neighbourhood.

The second case was that of a farmer, about thirty-five years of age, residing a few miles from Ripon, who thought that he had caught cold, which had brought on some itching of the penis and scrotum, both of which, in the course of two days, became very much inflamed and enlarged; on which account he sent for a surgeon, who took blood from the arm, applied fomentations, and gave him purgative medicines. On the following day, to his great surprise and dismay, a gangrenous spot appeared on the scrotum. The blood taken exhibited no indication of inflammation. Before he left the house I saw the patient with him, and told him what, in my opinion, would be the result, which he could scarcely credit: I then gave him an account of the former case, and, as far as lay in my power, put him on his guard. In this case the tongue exhibited the same appearances as in the former. We gave decoction of bark in the porter, and applied an ale poultice, with dressings, similar to those employed in the former case. The inflammation extended along the abdomen as high as the umbilicus, above which there was an eruption resembling the ecthyma cachecticum of Willan. Suppuration took place above the pubes, the whole of scrotum and prepuce sloughed off, and the patient is convalescent.

During my attendance in the first case, one morning, whilst the surgeon was dressing the patient, the scrotum and penis being in a gangrenous state, a messenger came to request him to go to a woman in labour, who resided about half a mile from our patient, and he obeyed the summons without loss of time. Four or five days after this, on meeting again, he said, "you will recollect that I was sent for to a woman in labour on such day." I replied yes, what of that? "She is dead; everything seemed to be going on well until yesterday, when she was seized with violent pain in the region of the uterus, and she died before I had time to do anything to relieve her." In the course of two or three days, on meeting again, he said, "It is very odd, Dr. Paley, I have lost another patient in the same unaccountable way as before;" and the next morning, at our meeting, he stated that he had another patient about two miles off seized in the same manner, whom he requested me to visit along with him. After seeing his patient, I told him that she was labouring under puerperal fever, and before we left the house he was sent for to visit another woman whom he had attended in labour in the same village. I accompanied him, and found her also the subject of puerperal fever. I believe that he had in all six cases of this disease.

I enquired of nearly all the general practitioners in Halifax and the neighbourhood if they had any cases of puerperal fever, but not one could I hear of; indeed, most of the medical men owned that they had never seen a case of it in the whole of their practice. About the same time I was requested to visit a married lady, aged fifty-four, who resided in the same village as the teacher of music, betwixt whom, however, there had not been any intercourse, but she had visited repeatedly the first woman who had died so unexpectedly. I found that she had been seized, on the day previous to my seeing her, with violent pain of the bowels, which had continued to increase in spite of the means employed. When I arrived she was *in articulo mortis,* and expired before I left the room, which was twenty-six hours after the time she had first felt any sort of uneasiness. In the present day this case would very probably have been considered as Asiatic cholera; and it is to be regretted that we were not permitted to make a post-mortem examination.

There is not the slightest doubt on my mind that the surgeon who was in attendance was the means of communicating something (call it what you please) from the patient labouring under the disease of the scrotum to the lying-in women, which in them produced puerperal fever; and with regard to the lady last mentioned, the first case of disease after parturition, which in all probability was that of puerperal fever, produced in her a species of enteritis, which in its progress bore a considerable resemblance to the disease of the music-master.

I pointed out these circumstances to the surgeon, and at the same time advised him to go from home two or three weeks, and to have his clothes washed

and fumigated; he did so, and the plague (for such it seemed) ceased. These circumstances I also mentioned to the surgeon in attendance on the case which has recently occurred in this neighbourhood. I advised him to wash his hands well previous to leaving the house of his patient, and not to attend any woman in labour or after her confinement, without first changing his dress. Notwithstanding this precaution, which I believe he rigidly observed, I received a note from him a few days ago, stating that he had some unfortunate cases of puerperal fever.

London Medical Gazette. Dec. 6, 1839.

Cases and Observations on Anuria Apyretica. By P. CAMPBELL, A.M., Surgeon.

[UNDER this title the author details, in a very lucid manner, the particulars of three fatal cases of the disease better known under the Cullenian name of *ischuria renalis.* As we have not room to copy them, we refer to them chiefly for the sake of young practitioners, whom we have often had occasion to see misled by the insidious and seemingly mild invasion of this deadly disease. Mr. Campbell's cases terminated in the usual manner, within a short period after the formal invasion of the malady. We quote the two concluding paragraphs of his well-written communication :]

A minute investigation, had it been permitted, might have discovered different morbid lesions to have accounted for the invasion of the disease in each of these three individuals, but the fatal termination was the same in all of them; profound coma, laborious, labial, occasional irregular respiration, with viscid frothy saliva, but without any symptom of palsy or dropsical effusion. Whether, therefore, we trace the origin of the disease to the solids or fluids, whether, from a paralytic state of the kidneys, their secreting functions become suspended, and the effete matter retained in the system; or whether, on the urine being secreted, but obstructed in its course to the bladder, and consequently reabsorbed into the circulation, the impure blood seems to act as a morbid poison on the brain, sufficiently powerful to occasion death, independent of effusion.

It unfortunately happens that, in cases of anuria, as in many other instances, the approach is so very insidious and masked, that the disease is not even suspected till it be fully formed, and the proper period for active treatment past. Prompt and free bleeding, both generally and locally; the warm bath with friction; stimulating external applications; demulcent, rather than irritating, diuretics, with purgatives calculated to produce watery digestions, seem to be the most likely means to combat this obscure but fatal malady.

Lancet. Dec. 14, 1839.

On the Treatment of Croup by large doses of Tartar Emetic.
By CHARLES WILSON, M.D., Kelso.

I HAVE now treated in all twelve cases of croup in children with high doses of tartrate of ammonia on the contra-stimulant plan, and of these two only have died. In one of the fatal cases there had been distinct and severe croupy symptoms for two days before I had any opportunity of exhibiting the medicine, yet its use was followed by a striking relief of these symptoms. Within the first seven days of treatment, the patient, a boy of four and a half years of age, took thirty-six grains of the tartrate, in doses which averaged half a grain, given at first every hour, and afterwards every two hours. The cough and respiration improved from the beginning, and at the close of the fifth day of treatment resembled what is heard in ordinary catarrh. The voice improved on the sixth day, and at the close of the eighth day of treatment it was heard distinctly, though previously little more than a feeble whisper. The medicine frequently caused vomiting, but no catharsis; the bowels, on the contrary, requiring to be regulated by enemata. On the eleventh day there was a relapse, and on the twelfth day the cough was again croupy and the inspirations sonorous. I hesitated, and I believe

improperly, in again employing the antimony; and the patient died on the fifteenth day after I had first seen him, and the seventeenth from the commencement of the disease. In the other case the patient died on the fourth day of the treatment, the antimony, however, having never been properly administered, nor my directions in other respects implicitly followed. With respect to the successful cases, I may observe, that I certainly met, for the most part, with little tolerance of the remedy in as far as the stomach was concerned, vomiting having been freely produced in nearly all; but there was no troublesome catharsis, and, even in one instance where there was a tendency to diarrhœa previously to the exhibition of the antimony, this ceased on the second day of the treatment, and it became necessary to have recourse to mild laxatives. Indeed, not a single case occurred, during some part of the treatment of which it was not requisite to exhibit castor oil, infus. sennæ, or some other remedy to act upon the bowels. The treatment was generally commenced with the application of leeches to the larynx, which were followed by warm poultices, frequently renewed; and, simultaneously with the leeches, the tartar-emetic was begun in doses of one fourth or one third of a grain, generally at first every hour till a decided impression was made, and afterwards every two hours till the patient was considered in safety; care being taken that its use was not intermitted at too early a period for the certainty of the cure. The remedy was usually exhibited in a mixture with a little mucilage, prepared with warm water to ensure its solution; and occasionally, with the older children, half a minim or a minim of T. Opii was joined to each dose, which seemed to have a marked effect in ensuring tolerance of the medicine, without diminishing its sanatory effects. The largest quantity given in any of the successful cases was sixteen grains, and in this case the tolerance was complete after the first two doses. In one of the cases the antimony, at a certain period, was combined with calomel, of which upwards of a drachm was exhibited. A blister was generally applied to the top of the sternum towards the close of the treatment, in order to obviate the risk of a relapse.

I am well aware that the tart. of antimony has been employed by numberless practitioners in croup, and that Cheyne among the rest has prescribed it in considerable and frequently repeated doses; but I am not aware that any one has exhibited it in precisely the same way or with the same aims as myself, or that they have sought from it anything beyond its ordinary well-ascertained action as an emetic. I have thus had no guide to direct me in the use of the remedy, an advantage which I probably might have possessed had my acquaintance with medical literature been more extensive; and, of course, I do not pretend that the limited experience which I have had of it has established its value beyond dispute. Having formed my diagnosis, however, with what care I could, and having treated my cases at periods when I heard of others attended with fatal consequences occurring in the vicinity, and my success in the treatment having been far greater than any I had hitherto attained with other methods, I considered myself justified in laying these details before you, and in recommending the remedy to your attention. *Border Med. Society.* 1839.

Contributions to the Pathology of Children. Tubercles of the Brain. By P. Hennis Green, m.b.

[We have only room for the *Remarks* appended to these five valuable cases. These remarks merit the best attention of pathologists and practitioners.]

The first circumstance which strikes us is, that tuberculous deposit, even to a very great extent, may exist in any part of the cerebral substance without producing any disturbance of the economy during life. This is a very remarkable fact. If the brain be the organ of the mind, and if (as it seems to be highly probable) different parts of the brain minister to different mental faculties, is it not strange that a portion of the cerebral substance, as large as a closed fist, may be totally disorganized without any influence being exercised on the intellectual faculties or the other functions of the brain? But without speculating

let us content ourselves with noticing the fact. In about one sixth of the cases of cerebral tubercles which I have seen there was no symptom whatever of their existence during life. In several others the symptoms were highly equivocal, and might have been attributed to any other cause. Thus, in the case of Mettaver, the only premonitory sign was habitual headach; after this had lasted for a certain time, symptoms very closely resembling those of acute hydrocephalus set in, and terminated fatally. In the other cases, however, the premonitory symptoms were much better marked, and consisted in some remarkable lesion of the motory power. It is here, in fact, that we must look for the essential symptoms of cerebral tubercle: and I believe that whenever long-standing lesions of this function exist in scrofulous subjects, they will generally be found to depend on tubercle of the nervous centres. The history of the case, if accurately noted, will often throw much light on its nature; the parents or relatives of the young patient will be found to have suffered from scrofulous affections, he himself, perhaps, bears external and visible signs of the malady; if the chest be carefully auscultated the presence of tubercles may be detected in that cavity, or we may, perhaps, find that some of the mesenteric ganglia are enlarged. All these circumstances will aid us in the formation of a diagnosis, which, it must be confessed, is extremely difficult. The nature of the malady must be determined mainly after the lesions of motility; yet how various, how uncertain are these; how difficult to connect them together, or refer them to any particular part of the organ which we suppose to be injured.

In our first case the child had some indeterminate cerebral attack at the age of seven, which was followed by amaurosis for nine days. Now, amaurosis is one of the occasional symptoms of cerebral tubercle; but how difficult was it to connect the symptom and its cause in the case of Humanery, to which I now refer. During two years the muscles of the face were occasionally convulsed in a peculiar manner, without any other symptom, and the child died suddenly, with symptoms which resembled those of hydrocephalus in its clot. Here I would beg the reader's attention to a point which seems to me of the very highest importance in the history of cerebral diseases. A great portion of the obscurity and seeming irregularity of cerebral diseases in children depends on our not having observed them accurately; and I am convinced that most of the cases which have been described as cases of "irregular hydrocephalus," of "cerebral inflammation, with abnormal symptoms," were simply nothing more than examples of cerebral tubercle; the symptoms, progress, and termination are exactly the same in both cases.

In the second case, the symptoms produced by the tubercles in their chronic stage, were a peculiar nervous tremor of one side of the body, and then true epilepsy; in the third case, convulsions, with a peculiar rotatory movement of the head; while, in the fifth case, the only premonitory symptom was habitual headach. I had, therefore, some reason to say, that the symptoms of cerebral tubercle were very various and uncertain. Nevertheless, I feel hope that when I shall have placed before my medical brethren a series of forty cases, some advance will have been made towards a better understanding of this difficult question.

Lancet. Jan. 18, 1840.

SURGERY.

On the Treatment of Varicose Veins. By A. T. S. DODD, Esq., Surgeon of the Chichester Infirmary.

[THIS is a valuable practical paper, and forms an appropriate *English* appendix to the Essay of M. Bonnet in our last Number. According to the views of this surgeon, it will be necessary for Mr. Dodd hereafter to ascertain the *permanency* of his cures, before the full value of the practice is admitted.]

At the Chichester Infirmary, during the last twelve months, eleven cases have been treated by the insertion of one or more needles under the vein, and the

application of the twisted ligature. Of these, ten have been entirely successful in remedying the varicose state of the veins, and healing the accompanying ulcer. One was unsuccessful, the varicose state of the superficial veins being cured by the obliteration of those that were so affected, but the ulcers still continuing. In three cases troublesome symptoms occurred from the application of the needles. In one of these, the patient complained of severe pain in the sole of the foot. In the second, ulceration followed the introduction of the needles, but the cure was ultimately complete. In the third, inflammation of the veins occurred, which, however, readily yielded to treatment.

The following is a summary account of the eleven cases:

No.	Name.	Age.	Duration of the Ulcers.	No. of Needles Inserted.	Result.	Duration of Treatment.
1	John Downham	49	16 years	5	cured	7 weeks
2	Eliza Luffe	33	9 years	1	cured	7 weeks
3	Do., readmitted for an ulcer on the other leg	33	9 years	2	cured	5 weeks
4	Redman Mary	59	20 years	1	cured	5 weeks
5	Henry Herman	49	15 years	1	cured	4 weeks
6	Wm. Barnes	44	many years	5	uncured	
7	Richard Dyer	55	2 months	3	cured	3 months
8	Sarah Luffe	28	1 month	2	cured	4 weeks
9	Martha Puddick	38	13 years	2	cured	7 weeks
10	Sarah Pullen	58	no ulcer	5	cured	4 weeks
11	James Dawson	52	several years	2	cured	5 weeks

The plan of treatment which is the subject of this communication was first recommended, as far as I know, by M. Velpeau. It consists in passing a needle (I prefer a flat one, slightly curved at the point, such as is used in inspections,) through the integument, a little to one side of the vein, taking care that it shall go under the vessel without wounding it, and bringing it out at the same distance on the opposite side. A waxed silk ligature is then passed round the projecting ends of the needle, in form of a figure 8, just as it is applied in hare-lip. The projecting point of the needle is guarded by a little bit of cork, to prevent its catching in the clothes, or injuring the other leg. Very slight pressure is sufficient to accomplish the object to be gained by the ligature, therefore it need not be drawn tighter than is necessary to obstruct the circulation through the vein. In about from twenty-four to thirty-six hours, inflammation is excited in the immediate neighbourhood of the ligature, which goes on increasing slowly while the needle remains. As this inflammation is the effective means of cure, by producing permanent obstruction to the flow of blood through the vein, and obliterating the cavity of this vessel, it is obvious that it should be allowed to proceed just so far and no further than is necessary for the accomplishment of this object, and that the length of time which the needle is allowed to remain must be entirely regulated by this. My colleague, Mr. Duke, and I have generally found that from four to eight days have been sufficient to effect our object, though the exact time must of course vary according to the nature of the individual constitution and its aptitude in taking up the inflammatory action. I am guided by the appearance of the local inflammatory symptoms, and by the fact of the commencement of ulceration at the point of the skin where the needles pierce it. If this has begun, it is time to remove the needle, and, generally speaking, enough inflammation is found to follow for the sealing up of the vein. If ulceration has not taken place when the needle is withdrawn, it is generally necessary to repeat the operation, from the circulation being renewed through the diseased vessel. At the same time it is necessary to be careful not to allow the process of ulceration round the needle to remain in action long, as from this cause, in one case, troublesome ulcers resulted in this

very spot, though the sore for which the practice was adopted was cured. I have not found this to occur when the above precaution was adopted. The only adjuvant treatment that has generally been required is low diet, recumbent position, and, when the needles are withdrawn, the application of the warm-water dressing. Should the inflammation appear more than needful, of course the treatment would be modified as circumstances arise.

The time required for the cure must of course vary much, according to the peculiarities of the case, from the habit of the individual, or the nature of the disease. In all the cases, but one, that we have treated, there was an ulcer connected with the diseased veins, but the duration of the treatment seemed to be not at all influenced by the previous duration of the ulcer, or indeed of the diseased state of vein, as will be seen by reference to the table. If the system bore the operation kindly, and too great irritation and inflammation was not set up, the cure generally proceeded rapidly, and was always effectual and permanent, if the treatment was carried to a proper extent. The shortest period required in any of our cases for the completion of the cure was three weeks, the longest seven weeks. The one case, which was of simple varicose veins, without an ulcer, had them immensely enlarged, and acutely painful and tender, and apparently ready to burst, so that the patient was unable to attend to her duties. Four weeks were here sufficient to make a permanent cure, the affected veins being reduced to a hard cord.

With regard to the number of needles necessary to be applied, though one was sometimes found to accomplish the obliteration of the vein, yet it was found a more effectual plan, and not productive of more after-suffering, to insert two under the same vein, at about two inches distance apart, though it is not always that the enlargement of the vein offers sufficient length for this. Where, however, it does, the obstruction produced by the two needles is more certain, and more effectual, without any greater risk, as far as my experience goes, of injurious effect to the vein. In some cases the ulcer does not yield, or yields only partially, to the application of the remedy to one vein, and then it is necessary to take up a second, or even a third, found in its vicinity. In one instance an enormous ulcer was rapidly reduced to about a fourth of its size by the obliteration of one vein; but though five needles were at different times inserted, and effectually, as regards the obstruction of the veins, yet the ulcer was never effectually cured. In the case of varicose veins, unaccompanied by ulcer, Mr. Shute, the house-surgeon, under whose care it was, inserted at the same time five needles—two upon the principal branch, and three others upon branches in the immediate neighbourhood, which communicated with it. Some erysipelatous inflammation was excited, which readily yielded on the removal of the needles, on the eighth day. The number of needles, however, which we employed in most of our cases was two.

The rapidity with which the curative effect upon the ulcer displayed itself was surprising. In twenty-four hours the commencement of cicatrization was seen in one instance, and in forty-eight hours the tender cuticle had more than half covered the granulations. This was not, however, so strikingly the case in every instance, though in all the progress was satisfactory. I observed, however, that the latter stages of healing were not commensurate with the earlier, in the progress of cure being much slower, and, in one instance, remaining stationary, after being readily reduced to about a fourth of its former size.

I was struck with the appearance of the cicatrix in all these cases. Instead of the reddish tender-looking cicatrix which we usually find after a recently healed ulcer, I observed that after the application of the needles the resulting cicatrix had, in the course of a few days after its formation, the firm whitish appearance of one of considerable standing, and even a scar of an old wound, in the neighbourhood of the ulcer under treatment, had its appearance modified, in becoming clean and free from a scaly incrustation, and assuming the white fine appearance of a healthy cicatrix.

Med. Gazette. Dec. 20, 1839.

On Thread Setons. By JONATHAN OSBORNE, M.D., Dublin.

THE ordinary seton, although a measure of great value when it is desired to keep up a permanent counter-irritation, is yet often attended with much unnecessary pain and inconvenience. Especially in the nape of the neck, the size of the strap of threads or of gum elastic introduced into the opening is productive of general discomfort and teasing, not well calculated to diminish a tendency of blood to the head, but rather the contrary. I have adopted a plan which will be found better suited to many of such cases. It is to make a seton with an ordinary sewing-needle of the thickest kind, and one thread of oiled silk. This is passed through a piece of the skin held between the finger and thumb, about six inches of the thread being allowed to remain. In twenty-four hours considerable redness comes on, and in a few days a purulent discharge is set up, much more in quantity than a comparison between the size of it and of the ordinary seton might lead to expect. The opening gradually enlarges, and no doubt in process of time, like the perforations made in the ears for ear-rings, assumes the function and secretion of a mucous membrane. The trifling degree of pain, however, inflicted by the operation enables us to multiply those setons, and to substitute new for old ones; so that I think it is evident, that in this way a greater discharge and a more efficient counter-irritation may be maintained, with less inconvenience than by the ordinary setons, and in places where the former would be impracticable.

Dublin Journal. Jan. 1840.

Observations on Diffuse Inflammation. By HENRY KENNEDY, M.B.

THIS is an elaborate essay on a very important subject. We regret that we have only room for the conclusions which Dr. Kennedy considers as warranted by the investigation entered upon by him. They are as follow:

1. That diffuse inflammation will not attack a person in perfect health.

2. That the bad state of health preceding diffuse inflammation is powerfully caused by anxiety of mind, by great bodily fatigue, by shocks of the nervous system, by improper diet, or by anything which has a tendency to lower the general healthy tone of the system.

3. That this deranged state of the health is shown principally in a vitiated state of the bowels.

4. That when once this unhealthy condition is established, the slightest cause is capable of inducing diffuse inflammation.

5. That venous inflammation does not necessarily cause diffuse inflammation.

6. That venesection may cause diffuse inflammation, the vein, however, remaining healthy.

7. That when venous inflammation does exist, the fever which accompanies it is more likely to be of the typhoid type than when diffuse inflammation exists alone.

8. That diffuse inflammation may attack several parts of the body in rapid succession, or it may be confined to one part, as the hip, or one organ, as the lung.

9. That pus may be poured out into the joints, serous cavities, or cellular structure, without any appearance of surrounding inflammation.

10. That, at the very onset of the attack, the free application of the actual cautery holds out a fair probability of checking the disease; but, when once formed, free and deep incisions are the only treatment on which any reliance can be placed.

Dublin Journal. Jan. 1840.

PART FOURTH.

Medical Intelligence.

ON THE TORULA CEREVISIÆ, OR YEAST-PLANT.

We are the more induced to notice this curious subject in our present Number, because of its close relationship to the important discoveries of Schleiden and Schwann, detailed in a preceding article (Art. X., p. 495); and we have thought it of sufficient consequence to deserve illustration (see Plate II., Fig. 12, *a*, *b*, *c*). The description of the yeast-globules is derived from a paper in the *Comptes Rendues* (Aug. 20, 1838); and we have ourselves tested its accuracy.

When *yeast* or *ferment* is examined with a microscope, it is seen to consist principally of a number of minute disconnected vesicles, much resembling (except in colour) those of the protococcus (fig. 12, *a*). These, like seeds, may possess a dormant vitality for almost any length of time; their power of growth, when placed in proper circumstances, not being destroyed by exposure to such extremes of temperature as 212° and 76°, or by being entirely desiccated. When these are placed in a saccharine solution, they commence vegetating actively, provided the temperature be sufficiently high. If a fluid in a state of fermentation be examined at short intervals, it is observed that each vesicle puts forth one or more little prolongations or buds (fig. 12, *b*), which in time become new vesicles like their parents; these again perform the same process, so that within a few hours the single vesicles develope themselves into groups of four, five, or six (fig. 12, *c*). This is not the only way, however, in which they multiply: sometimes the vesicles are observed to burst, and to emit their contents; and new individuals originate from the germs thus liberated. This simple form of reproduction has evidently a fundamental correspondence with that which has already been described in regard to the pollen-grain of the flowering-plants, and the spores of the higher cryptogamia; and it exactly corresponds with that of the red snow, which does not, however, *also* increase by buds as this does. By the time that five or six vesicles are found in each group, the fermentation is sufficiently advanced for the purposes of the manufacturer, and he then takes measures to check it. The vegetation of the yeast is then suspended, and the groups of vesicles separate into individuals, the mass of which constitutes the yeast thus largely increased in amount. This discovery was made almost concurrently by three observers, MM. Cagniard-Latour, Schwann, and Kützing.

All fungi appear to have, in a greater or less degree, the power of hastening the decomposition of the organic substances on which they grow, for the purpose, it would seem, of thus procuring for themselves the supply of carbon which other plants derive from the atmosphere. The common *mould*, for example, destroys the palatability of sweet preserves; and the *dry-rot* causes the decay of the timber it infests. Like the rest of its tribe, the simple but important plant we have just described (which has received the appellation of *torula cerevisiæ*) decomposes the organic matter in which it developes itself; and it carries a step further the process which the conversion of the barley into malt by incipient germination had already commenced. This had changed the fecula of the seed into sugar, and an equivalent of carbon was thus liberated. Another equivalent is now abstracted for the use of the fungus, and the sugar is converted into alcohol.

But the wonders of this humble vegetation do not stop here. The same germs which thus develope and multiply themselves in an actively-fermenting fluid may produce a plant of very different aspect at a subsequent time. When malt liquor is becoming stale, a slimy production is found on its surface, which, when microscopically examined, is found to be of a vegetable nature, and to originate almost certainly from some of the vesicles left in the fluid.

In placing a mass of yeast into a fermentable solution, therefore, we sow the germs of myriads of fungi, by the development of which that change is effected in the fluid which it is our object to produce. But, it will be asked, how does fermentation commence in fluids which have not been thus treated? The solution of this question, according to M. Turpin, is to be found in the hypothesis, that the cell-germs (cytoblasts) contained in the vesicles of the malt or other vegetable structure from which the saccharine solution is made, being liberated from their relation with the parent structure, may develope themselves as separate individuals in a lower grade of vegetable existence. In other words, the same germs may produce cells which shall form part of the future barley-plant, or which shall exist as the *torula cerevisiæ*, according to the circumstances of their development. This is certainly a bold hypothesis; but it is one not unsupported by facts. According to M. Turpin, these simple forms of vegetation may take their origin not only from higher plants, but from the results of animal organization. He has observed the *penicillium glaucum* (a common form of mould) to spring from globules of milk; and a form not very dissimilar to arise from the albumen of the egg. Reasons have been lately given for the belief that the globules in the fluids of the living animal body have sufficient independent vitality to multiply themselves by spontaneous rupture and liberation of the contained germs of others; but upon this subject we do not yet feel competent to offer a decided opinion. In the mean time, we think that there is sufficient evidence for the reception of M. Turpin's hypothesis as not improbable; though we ought by no means to build upon it with anything like certainty. It will be seen to fall in with the more general idea which we formerly expressed (Vol. IV., p. 20), that beings of a high degree of organization may, under peculiar circumstances, develope, from various parts of their tissues, forms corresponding with those of inferior organisms.

THE RESULTS OF DR. MARSHALL HALL'S ENQUIRIES INTO THE NERVOUS SYSTEM, AS STATED BY HIMSELF.

We have readily acquiesced in Dr. Hall's request that we should insert a concise statement of his present views and claims. We cannot accuse ourselves, however, of having either neglected them or treated them unfairly. We have followed him, step by step, in his researches,—not giving an immediate assent to his opinions when they seemed to us deficient in solid foundation,—but not condemning them as absurd or untenable. We have pointed out instances in which Dr. Hall was anticipated by previous experimenters; but we have not accused him of making an unfair use of their researches. And, in ultimately giving our assent to his peculiar doctrines, because additional evidence has been adduced in their support, we have expressed, in the most decided manner, our conviction of their importance. If our readers will turn to pp. 101-4 of the present volume, they will see the doctrine of the *distinct spinal system* and its peculiar functions, enunciated by Dr. Hall in his *first*, *second*, *fourth*, and *fifth* propositions, set forth as fully as the object of that article permitted. We there speak of the discovery of these as constituting the *third great era* in the history of neurology, and as almost entirely the work of Dr. M. Hall. We said "almost," because the *anatomical* evidence in its support, which we deem of much importance, was *not* furnished by him, but by others. At a subsequent part of the same article (pp. 115-6), we speak of the discovery of *this distinct system* as even more the work of Dr. M. Hall than the discovery of the distinctness of the motor and sensitive nerves was of Sir C. Bell. We cannot see what more Dr. Hall can require from us. We should have thought, too, that he would have regarded *our* accordance with his doctrines as more valuable than that of individuals or journals whose assent was given hastily, and without close examination of their merits. But we fear that he is so convinced that we always *must be opposing* him, that he does not know when we coincide with him.

In regard to Dr. Hall's *third* proposition, we must confess ourselves at present unable to see its importance. We know *nothing* at present of the mode in

which any impressions—sensory or volitional, incident or motor—are conveyed by the nerves. To refer them all to one common head is all that we can do; for anything we know, the modes of action may be the same in all these instances, or it may be different. The terms *vis nervosa, nervous action*, and the like, can only be used, therefore, in the same vague and general sense as *vitality*, or any other abstract character of that nature.

Some of Dr. Hall's later researches (of which the results have been only just published) we have noticed in our present Number (p. 438); and to the remainder we shall return when they are incorporated in the Third Memoir, which we understand Dr. Hall has in preparation, and which we shall take the first opportunity of noticing. Those of our readers who feel an interest in this question may advantageously refer to a *former* statement of his claims by Dr. M. Hall, to which we gave admission (Vol. III., p. 577). It appears to us that Dr. H. now relinquishes (as having been previously set forth by others) what he then claimed as his own.

To the Editor of the British and Foreign Medical Review.

Sir,—Will you do me the favour to insert the following paragraph relative to what I myself consider to be the results of my own enquiries into the Nervous System?

It is well known,—

1. That a series of *experimental* phenomena were observed in decapitated animals by Redi, Whytt, Legallois, Blane, &c.

2. That similar phenomena and certain *sympathetic actions* were referred, by Prochaska, Dr. Alison, and Mr. Mayo, to certain nerves termed sentient, certain parts of the brain and spinal marrow, and motor nerves.

3. That Blane distinctly stated that the motions observed in a decapitated animal did not depend upon sensation.

It is obvious that, whatever my own claims may be, therefore, they cannot include any of these points. What, then, are they?

I claim, as the peculiar result of my own investigations, the discovery,

1. That in the same theca vertebralis there is, beside the *cord* of cerebral, tactile, and voluntary nerves, a distinct organ and nervous centre, which I have designated the true spinal *marrow*.

2. That in the same neurilemma with the tactile and voluntary nerves, and also exclusively of these, there is a *system* of incident and reflex nerves, of which the true spinal marrow is the centre, the combiner, &c.

3. That the *principle of action* in this subdivision of the nervous system is the *vis nervosa* of Haller, acting in directions and according to laws unknown and even denied before.

4. That through this same subdivision of the nervous system, and on this same principle of action, *all ingestion* and *all egestion* in the animal economy are effected—and as consequences: 1, The preservation of the individual; and 2, the continuance of the species.

5. That there is indeed a special system of *respiratory* nerves; but that this includes incident and excitor nerves, as well as the respiratory nerves of Sir Charles Bell, and that it is only a part of the more general excito-motory system.

6. That, in a word, every act of ingestion and of egestion is a *spinal* act, resulting from *excito-motory* influences of the vis nervosa, urged in *reflex* directions, along the incident and reflex *nerves*, to which I have adverted.

7. That upon the same anatomy, motor principle, &c., the whole class of spasmodic diseases, their pathology and their therapeutics, also depend. I may add to these statements, as the results of more recent enquiries, that I believe it to be established:

1. That, whilst *volition* acts upon the cerebrum and through cerebral nerves alone, *emotion* acts upon the medulla oblongata, and the excito-motory nerves.

2. That the *irritability* of the muscular fibre depends upon the true spinal marrow and the ganglia, exclusively of the cerebrum.

3. That as consequences of these laws, in hemiplegia the *opposite* side of the body is at once *paralyzed* to *volition*, but *agitated* by *emotion;* that the irritability is augmented in *cerebral*, but diminished in *spinal* paralysis.

If I should be wrong in asserting these claims, and any critic candidly point out my error, I shall be at once obliged and ready to confess my mistake. But it is useless to continue to disprove claims which I never made, whilst my real claims are allowed to pass unnoticed.

May I take this opportunity of recording a fact, observed during the late autumn, and communicated at that time to some of my friends? If we take the *libellula*, or dragon-fly, remove the head, separate the thorax, and divide the abdomen into three parts, we have *five* distinct portions of the insect manifesting rhythmic respirations. In each we must consequently have the "analogue" of the medulla oblongata in the vertebrated class. I propose to pursue the enquiry when the spring again brings out these interesting insects. It remains to be determined whether each ganglion singly and all unitedly be analogous to the medulla oblongata, quoad respiration, associated with their incident and reflex excito-motory nerves, and to trace the nervous system of deglutition, defecation, &c.

I am, Sir, your obedient servant,

London; Feb. 21, 1840. MARSHALL HALL.

NEW BILLS OF MORTALITY FOR LONDON.

It is most gratifying to us to be able to state that the metropolis of England can, at last, boast of weekly bills of mortality of the most authentic kind, and which leave the medical statistician little to desire. For these we are indebted to the enlightened views entertained by Mr. Lister, the registrar-general of births, deaths, and marriages, who has authorized his able coadjutor, Mr. Farr, to draw the map according to the form and principles adopted by this gentleman in his admirable Report noticed in Art. II. of our present Number. A bill is printed weekly containing the deaths from all causes registered during the week ending on the Saturday immediately preceding, and is circulated very widely. The ages are classed only under three heads, viz., from birth to 15, from 15 to 60, from 60 upwards—a limitation for which, doubtless, there are good reasons, but which we cannot help greatly regretting. We think the divisions might have been, at least, doubled, if not extended to decennial periods—the first ten years being still further subdivided. We are, however, most thankful for what we have got.

The weekly bill is framed from the deaths registered in the thirty-two parochial districts into which London and its vicinity are divided. These include the City of London within and without the walls, the City and Liberties of Westminster, the Out Parishes within the bills of mortality; and the parishes of St. Mary-le-bone; St. Pancras; Kensington; Fulham; Hammersmith; St. Luke, Chelsea; Paddington; St. Mary, Stoke Newington; St. Leonard, Bromley; St. Mary-le-bow; Camberwell; Greenwich; St. Nicholas and St. Paul, Deptford; and Woolwich. For the sake of convenient comparison, the districts are classified as follows: *West districts*—Kensington; St. George, Hanover Square; Westminster; St. Martin in the Fields; St. James. *North districts*—St. Mary-le-bone; St. Pancras; Islington; Hackney. *Central districts*—St. Giles and St. George; Strand; Holborn; Clerkenwell; St. Luke; East London; West London; City of London. *East districts*—Shoreditch; Bethnal Green; Whitechapel; St. George in the East; Stepney; Poplar. *South districts*—St. Saviour; St. Olave; Bermondsey; St. George, Southwark; Newington; Lambeth; Camberwell; Rotherhithe; Greenwich. The population of the whole, according to the enumeration of 1831, was 1,594,890.

It is our intention henceforward to give, in detail, quarterly abstracts of these tables, arranged, probably, according to the seasons. These will soon constitute a series of authentic statistical documents, such as have never yet appeared

in this country, and fit to form the basis of the most momentous hygienic conclusions. These abstracts we shall commence in our next Number, contenting ourselves, at present, with giving the gross results of the first two months of the present year, which are as follows:

		January. Week ending 11th,	18th,	25th	*February.* Week ending 1st,	8th,	15th,	22d,	29th	Total.
Weekly Deaths in the	West Districts ...	149	160	122	108	125	98	121	138	1021
	North Districts...	171	170	163	157	152	145	141	192	1291
	Central Districts..	199	230	182	187	157	172	184	170	1481
	East Districts......	225	216	207	200	167	190	199	171	1575
	South Districts ...	223	221	242	183	217	208	210	245	1749
	Total...........	967	997	916	835	818	813	855	916	7117
Weekly Deaths at different ages, viz., from	0 to 15...........	404	392	384	360	346	356	345	375	2962
	15 to 60..........	332	361	302	291	278	282	319	330	2495
	60 and upwards...	230	244	230	182	194	174	189	211	1654
	Total...........	966	997	916	833	818	812	853	916	7117

BOOKS RECEIVED FOR REVIEW.

ENGLISH.

1. Observations on the Management of the Poor in Scotland, and its effect on the health of the great towns. By W. P. Alison, M.D., F.R.S., &c.—Edin. 1840. 8vo, pp. 198. 3s. 6d.

2. The Cyclopædia of Surgery. Parts IV. V. Edited by W. Costello, M.D. (Ankylosis to Burns.) — London, Jan., March, 1840. 5s. each Part.

3. Medical Portrait Gallery. By T. J. Pettigrew, F.R.S. Parts XXIII-V. 3s. each No.

4. A Dictionary of Practical Medicine. By James Copland, M.D., F.R.S. Part VI. (Infection—Insanity.) — London, Jan., 1840. 4s. 6d.

5. The Intimate Structure of Secreting Glands. By John Müller, M.D. With the subsequent discoveries of other authors. By Samuel Solly, F.R.S. With plates.—London, 1839. 8vo, pp. 166. 7s. 6d.

6. Illustrations of Cutaneous Disease. Parts IX. to XV. By R. Willis, M.D., &c.—London, 1839. 5s. each Part.

7. Medical Etiquette; or an Essay upon the laws and regulations which ought to govern the conduct of members of the medical profession in their relation to each other. By Abraham Banks, Esq., Surgeon.—London, 1839. 12mo, pp. 104. 3s. 6d.

8. Rudiments of Animal Physiology. For use in schools and for private instruction. By Dr. G. Hamilton, Falkirk. (Chambers's Educational Course.)—Edin. 1840. 12mo, pp. 104. 1s. 6d.

9. Preservation of the Teeth indispensable to comfort, appearance, health, and longevity. By John Gray, Dentist, M.R.C.S. —London, 1838. 12mo, pp. 77.

10. Practical Observations on various subjects relating to Midwifery. By James Hamilton, M.D., F.R.S.E., Professor of Midwifery in the University of Edinb. Second Edition, revised and enlarged.—Edinb. 1840. 8vo, pp. 418. 12s.

11. Aphorisms on the Treatment and Management of the Insane; with considerations on public and private lunatic asylums, pointing out the errors in the present system. By J. G. Millingen, M.D., &c.—London, 1840. 12mo, pp. 202. 4s. 6d.

12. A Compendium of Materia Medica and Pharmacy, adapted to the London Pharmacopœia. By J. Hunter Lane, M.D., F.L.S., &c.—London, 1840. 12mo, pp. 308. 5s.

13. A Treatise on the Ear; including its Anatomy, Physiology, and Pathology;

for which the author obtained a gold medal in the University of Edinburgh. —London, 1840. 8vo, pp. 255. With Plates. 10s. 6d.

14. Animal Mechanism and Physiology; being a plain and familiar exposition of the structure and functions of the human system. Designed for the use of families and schools. By J. H. Griscom, M.D.—New York, 1839. 12mo, pp. 357.

15. A Compendium of Materia Medica, Pharmacy, and Toxicology. For the use of medical students. By D. Spillan, M.D. —London, 1839. Sm. 8vo, pp. 80.

16. Twenty-fifth Annual Report of the Directors of the Glasgow Royal Asylum for Lunatics.—Glasgow, 1839. 8vo, pp. 25.

17. Proceedings of the Border Medical Society. Instituted June 27, 1838.—Kelso, 1838-9. 8vo, pp. 26.

18. Outlines of Physiology; with an Appendix on Phrenology. By P. M. Roget, M.D., F.R.S., &c. Revised with numerous Notes.—Philadelphia, 1839. 8vo, pp. 516.

19. New Remedies; the method of preparing and administering them; their effects on the healthy and diseased economy, &c. By R. Dunglison, M.D., M.A.P.S., Professor of Medicine in the College of Philadelphia, &c.—Philadelphia, 1839. 8vo, pp. 503.

20. A Treatise on the causes and consequences of habitual constipation. By John Burne, M.D., Physician to the Westminster Hospital, &c.—London, 1840. 8vo, pp. 257. 7s. 6d.

21. Elements of the Practice of Medicine. By C. Lendrick, M.D., Queen's Professor of the Practice of Medicine, &c. Part I.—Dublin, 1840. 8vo, pp. 123. 3s.

22. Anatomical, Pathological, and Therapeutic Researches on the Yellow Fever of Gibraltar of 1828. By P. C. A. Louis, M.D. Physician to the Hotel Dieu, &c. Translated from the MS. by G. C. Shattuck, jun. M.D.—Boston, 1839. 8vo, pp. 374.

23. An Atlas of Plates illustrative of the Principles and Practice of Obstetric Medicine and Surgery. By F. H. Ramsbotham, M.D., &c. Nos. I.-III. 8vo, 16 in each. 1s. 6d.

24. Medical Communications of the Massachusetts Medical Society. Vol. II. Parts I.-III.—Boston, 1837-8-9. 8vo, pp. 257.

25. Practical Observations on the Nature and Treatment of Talipes or Clubfoot, particularly of Talipes Varus. By W. M. Coates, M.R.C.S.—London, 1840. 8vo. pp. 42. With Two Plates. 2s.

26. On the Influence of Artificial Light in causing Impaired Vision, and on some Methods of preventing or lessening its injurious action on the Eye. By James Hunter, M.D. Surgeon to the Edinburgh Eye Dispensary.—Edinburgh, 1840. 8vo, pp. 94. With a Plate. 3s. 6d.

27. A Treatise on Syphilis. By Herbert Mayo, F.R.S. Surgeon of Middlesex Hospital.—London, 1840. 8vo, pp. 128. 5s. 6d.

28. On the Anatomy of the Breast. By Sir Astley P. Cooper, Bart., F.R.S., D.C.L., G.C.H., Sergeant-Surgeon to the Queen.—London, 1840. 4to, pp. 193. With a volume of Folio Plates. 3*l*. 3s.

29. Pathological Observations on the Diseases of the Uterus. By Robert Lee, M.D., F.R.S., Physician to the British Lying-in Hospital, &c. Part I.—London, 1840. Folio, pp. 12. With Four Coloured Plates. 14s.

FOREIGN.

1. Untersuchungen zur Physiologie und Pathologie. Von Dr. Fried. Nasse und Dr. Herm. Nasse. Zweiten Bandes, Zweites Heft.—Bonn, 1839. 8vo, pp. 155.

2. Sul Cholera Morbus Avvisi medici di Giuseppe Maria Stilon, M.D. &c.—Malta, 1839. 8vo, pp. xxiii. 61.

3. Die diagnostische bedeutung der einzelnem Symptome der hitzigen Hirnhöhlenwassersucht der Kinder. Von Dr. H. Wolff, prakt. Arzte zu Bonn.—Bonn, 1839. 8vo, pp. 63.

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